Who Takes the Blame? The Strategic Effects of Collateral Damage

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Can civilians caught in civil wars reward and punish armed actors for their behavior? If so, do armed actors reap strategic benefits from treating civilians well and pay for treating them poorly? Using precise geo-coded data on violence in Iraq from 2004 through 2009, we show that both sides are punished for the collateral damage they inflict. Coalition killings of civilians predict higher levels of insurgent violence and insurgent killings predict less violence in subsequent periods. This symmetric reaction is tempered by preexisting political preferences; the anti-insurgent reaction is not present in Sunni areas, where the insurgency was most popular, and the anti-Coalition reaction is not present in mixed areas. Our findings have strong policy implications, provide support for the argument that information civilians share with government forces and their allies is a key constraint on insurgent violence, and suggest theories of intrastate violence must account for civilian agency.

"When the Americans fire back, they don't hit the people who are attacking them, only the civilians. This is why Iraqis hate the Americans so much. This is why we love the mujahedeen."[1] —Osama Ali 24-year-old Iraqi

"If it is accepted that the problem of defeating the enemy consists very largely of finding him, it is easy to recognize the paramount importance of good information."[2] —Gen. Sir Frank Kitson (Ret.) Commander-in-Chief UK Land Forces

Why does violence against civilians in civil war sometimes attract civilians to the insurgents' camp and in other cases repel them? Studies of the interaction between civilians and armed actors in civil wars have shown that both outcomes are possible (cf. Stanton 2009; Valentino 2004). Attacks that harm noncombatants may undermine civilian support or solidify it depending on the nature of the violence, the intentionality attributed to it, and the precision with which it is applied (Downes 2007; Kalyvas 2006; Kocher, Pepinksy, and Kalyvas 2011). Existing empirical research on the subject has studied the consequences of large-scale violence against civilians (Valentino 2004), indiscriminate violence against civilians (Lyall 2009), and targeted killings of specific individuals (Jaeger and Paserman 2009). Unfortunately, little empirical attention has so far been paid to the consequences of collateral damage, that is, to what happens when civilians are caught in the cross-fire.

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[1]Dexter Filkins, "Raising the Pressure in Iraq," *The New York Times* (September 14, 2004).

[2]Kitson (1971, 95).

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This lack of attention is unfortunate. First, the consequences of mistreating civilians are a first-order policy concern, with military units from Western countries frequently being called upon to accept high levels of risk in order to protect noncombatants. Moreover, the laws governing international armed conflict codify and strengthen norms against intentionally harming civilians but leave military commanders on all sides substantial tactical latitude (Gray 2000). While liberal democratic states face substantial pressures to protect civilians in warfare (Crawford 2003), there is often substantial uncertainty as to what abiding by legal principles such as "discrimination"—the obligation of military forces to select means of attack that minimize the prospect of civilian casualties—actually entails (Walzer 2000, 138–59). If the data provide convincing evidence that avoiding civilian casualties helps an armed actor achieve its military objectives, then the case for accepting greater risk in exchange for killing fewer civilians will be strengthened. Our analysis provides evidence about the impact of civilian casualties on the short-term military objective of reducing insurgent attacks; we do not address the possibility that actions which lead to short-term increases in attacks can lead to positive long-term consequences (by leading to a negotiated settlement, for example).

Second, understanding the manner in which civilian killings impact subsequent patterns of violence can help shed light on theoretical arguments about the nature of insurgency and civil war. If collateral damage has an impact beyond the expected mechanical correlation, i.e., government-caused civilian casualties predict fewer attacks in the short run because they proxy for aggressive combat operations, that provides strong evidence against theories which discount civilian agency in these settings. More specifically, if the impact of civilian killings depends on the political context in which the killings occur—if killings by one side have a different impact on subsequent violence than killings by the other, for example—then theoretical models should take that into account. This would be especially important for two classes of models. First, for models that seek to explain the consequences of conflicts, such results would have strong implications as government and rebel commanders are constantly making choices that affect the probability that they harm civilians, choices that would be informed by the dynamics we study.[3] Second, for three-actor models of insurgency that give a prominent role to civilian decisions (see, e.g., Berman, Shapiro, and Felter, forthcoming), the potential heterogeneity of civilian responses should be taken into account.

To advance such understandings, we ask a simple question: what are the military consequences of collateral damage? Using weekly time-series data on civilian casualties and insurgent violence in each of Iraq's 104 districts from 2004 to early 2009, we show that both sides pay a cost for causing collateral damage. Coalition killings of civilians predict higher subsequent levels of insurgent attacks directed against Coalition forces, whereas insurgent killings of civilians predict fewer such attacks in subsequent periods. These relationships, however, are strongest in mixed ethnic areas and in highly urban districts, suggesting the reaction to collateral damage depends on both the local political environment and the nature of the tactical problem facing combatants on both sides.

We explain this variation using a theory of insurgent violence that takes civilian agency into account. In line with a long tradition of theoretical work (Berman, Shapiro, and Felter, forthcoming; Kalyvas 2006; Kitson 1971), we argue that insurgents' ability to conduct attacks is limited by the degree to which the civilian population supplies valuable information to counterinsurgents.[4] We hypothesize that collateral damage causes local noncombatants to effectively punish the armed group responsible by sharing more (less) information about insurgents with government forces and their allies when insurgent (government) forces kill civilians. Such actions affect subsequent levels of attacks because information shared with counterinsurgents facilitates raids, arrests, and targeted security operations which reduce insurgents' ability to produce violence. It thus follows that collateral damage by Coalition forces should lead to increased insurgent attacks against Coalition forces, while collateral damage caused by insurgents should lead to fewer such attacks.[5] Our data not only are consistent with this argument, but also allow us to cast doubt on several prominent alternative explanations. These findings suggest noncombatants have substantial agency in armed conflict and that models of their interaction with armed actors should take both this agency and the heterogeneity of civilian reactions into account.

Overall, the civil war in Iraq affords a unique opportunity to understand the effects of collateral damage. The robust media coverage of the war and remarkable data collection capabilities of Coalition forces mean we are able to track daily trends in both battlefield violence

[3] Examples include the size of bombs insurgents use, the timing of attacks, and rules of engagement at checkpoints.

[4] This approach can be contrasted with theories that place insurgents' supply of fighters at the center of conflict dynamics (cf. Dube and Vargas 2009).

[5] We thank Jim Fearon for this felicitous phrasing of our argument.

and civilian casualties for each of the 104 districts of Iraq from February 2004 through February 2009. This means that the microdata we use avoid two major methodological pitfalls. First, their resolution allows us to avoid ecological inference problems associated with cross-national research whose hypotheses imply testing at the microlevel but instead provide quantitative analysis at the national level (e.g., Cederman and Giradin 2007; Lyall and Wilson 2009). Second, because the data show patterns of violence in small geographic units across an entire country in weekly time series over a period of five years, we can circumvent some forms of selection bias that are of concern in qualitative subnational studies.[6] Experts conducting careful fieldwork in conflict situations can only observe a small part of the conflict in time and space at any one time (e.g., Kilcullen 2009) and can rarely follow a principled sampling strategy in choosing where to work for a host of logistical and security reasons. This study, and others of its kind, should therefore be a welcome addition to those who care about practical and theoretical issues surrounding insurgency and the challenges to restoring social and political order.

We proceed as follows. First, we briefly motivate the article by discussing the literatures on the interaction between armed actors and civilians in civil war and on the effects that violence against civilians has had on armed actors' objectives. We then describe our data and identification strategy, discuss the core results, and buttress them with a series of robustness checks. After presenting the results, we outline a simple theory regarding the informational aspects of the relationship between civilian casualties and insurgent violence directed against Coalition forces which can explain our findings and provide evidence against some prominent alternative explanations. We conclude by discussing the implications for policy and future research.

The Treatment of Civilians in Civil War and Insurgency

In fighting against each other, insurgents and counterinsurgents (henceforth, armed actors) make many decisions every day about how to deal with the noncombatant civilian population. At the most general level, encouraging cooperation from civilians and discouraging it with the enemy is a key goal. More specifically, civilians can provide valuable information to armed actors, such as the whereabouts of the other armed actor or which civilians actively aid the other armed actor (Kalyvas 2006). For competent militaries engaged in counterinsurgency, identifying rebel fighters can be the critical tactical challenge (Kitson 1971).

The war in Iraq is no exception; Coalition forces have struggled throughout the war to identify insurgents and gain the support of the local population (Cockburn 2007). One American soldier neatly summarized the problem: "The hardest part is picking out the bad guys" (Cockburn 2007, 138–39). Civilians in Iraq not only provided critical intelligence to Coalition forces, but also supplied insurgents with valuable information (Chehab 2006, 7). Because of the importance of information, Coalition forces have, on occasion, resorted to collective punishment of Iraqi civilians in the hope that the desired information will be forthcoming. Farmers belonging to the Khazraji tribe in Dhuluaya village, 50 miles north of Baghdad, saw this strategy in action first-hand. Cockburn reports that "US troops had told [farmers]. . .that the fruit groves were being bulldozed to punish the farmers for not informing on the resistance. A local . . . delegation was told by an officer that trees and palms were being destroyed as punishment of local people because 'you know who is in the resistance and you do not tell us'" (2007, 125–26).

Government and insurgent forces in other conflicts have found that violence against civilians can cut both ways: it can motivate civilians out of fear or push them into the enemy's camp (Kalyvas 2006, chap. 6; Valentino, Huth, and Balch-Lindsay 2004). Carr (2002) examines cases from the long history of warfare, illustrating that violent strategies of civilian punishment or deterrence are militarily and politically both counterproductive and costly. Kalyvas (2006) argues that selective violence is strategically superior to indiscriminate violence, at least when an armed actor exercises control over the population. After all, unless civilians perceive that violence is being used only on the "guilty," what incentive is there to comply with the armed actors' demands? Indeed, a recent study of the effect of Israeli Defense Force house demolitions on subsequent suicide attacks in the Gaza Strip provides evidence that targeted violence against civilians can have opposing effects in the same context, depending on whether civilians perceive the violence as justifiably inflicted (Benmelech, Berrebi, and Klor 2010).

Yet governments and rebels routinely engage in violence against civilians, perhaps because, in some cases, it works (Birtle 2008). Indiscriminate violence can cow civilians into submission and cooperation, reducing subsequent insurgent violence, either through intimidation or outright large-scale elimination (Downes 2007, 2008).

[6]Recent work by Nepal, Bohara, and Gawande (forthcoming) circumvents these problems in the cross-section for all Nepalese villages from 1996 to 2003 but does not have a strong temporal component.

Lyall's (2009) finding that random artillery fire directed against villages in Chechnya reduced subsequent insurgent activity in those villages suggests that, under some conditions, indiscriminate violence against civilians can achieve the desired effect.

Despite the ambiguous evidence from academic studies, the consensus among U.S. policy makers is clearly that mistreatment of civilians provokes insurgent violence. Testifying before Congress in January 2009, Defense Secretary Robert Gates highlighted this issue in the context of operations in Afghanistan: "I believe that the civilian casualties are doing us enormous harm in Afghanistan, and we have got to do better in terms of avoiding casualties . . . because my worry is that the Afghans come to see us as part of the problem, rather than as part of the solution. And then we are lost."[7] Politicians and commanders believe the issue matters enormously because civilians decide whom they will support based partly on how they are treated—or perceive they are treated—by insurgent and incumbent forces.[8]

In light of this fact, military commanders and their civilian superiors face quite a dilemma in designing rules of engagement. How much risk must soldiers absorb in trying to discriminate between noncombatant and insurgent? Moreover, does it even matter if U.S. forces go the extra mile in order to protect innocent lives? Does the civilian population in Iraq blame U.S. forces for civilian casualties, no matter which side caused the damage? Do civilians place some of the blame for casualties on insurgents, and if so can they punish them short of organizing competing militias?[9]

Data and Descriptive Statistics

Together, then, the academic literature and received wisdom from the military and policymaking communities suggest a broad consensus that collateral damage should be militarily problematic, but there is little systematic evidence for that contention. To develop such evidence, we combined data from a broad range of sources. This section describes those data and presents descriptive statistics and figures to demonstrate just how varied the landscape of violence in Iraq has been.

Civilian Casualties

There are no complete or perfect data for civilian casualties in Iraq or in any other conflict.[10] The casualty data used in this article come from Iraq Body Count (IBC), a nonprofit organization dedicated to tracking civilian casualties using media reports, as well as hospital, morgue, and other figures.[11] These data capture 19,961 incidents in which civilians were killed that can be accurately geolocated to the district level, accounting for 59,245 civilian deaths.[12] The full data run from March 2003 through June 2009, but we use a subset to match the data on insurgent attacks.

We divide these killings into four categories: (1) Insurgent killings of civilians that occur in the course of attacking Coalition or Iraqi government targets; this category explicitly excludes insurgent killings that are unrelated to attacks and are better classified as intimidation killings related to dynamics of the civil war (see below);[13] (2) Coalition killings of civilians; (3) Sectarian killings defined as those conducted by an organization representing an ethnic group and which did not occur in the context of attacks on Coalition or Iraqi forces; and (4) Unknown killings, where a clear perpetrator could not be identified. This last category captures much of the violence associated with ethnic cleansing, reprisal killings, and the like, where claims of responsibility were rarely made and bodies were often simply dropped by the side of the road.

[7] Congress, Senate, Committee on Armed Services, *Hearing to Receive Testimony on the Challenges Facing the Department of Defense*, 111th Cong., 1st sess., 27 January 2009, 21.

[8] Michael Mullen, "Building Our Best Weapon," *Washington Post* (February 15, 2009), B7.

[9] Some analysts argue the dramatic reduction in violence in Anbar governorate which began in mid-2006, the "Anbar Awakening," occurred when the population's frustration with excessive civilian casualties caused by foreign militants led them to turn against the insurgents. Others disagree, arguing that the local militias who made up the majority of the insurgents in Anbar switched sides: they turned from fighting the Coalition to working with it. There is no solid consensus in the literature, but Long (2008) and Biddle (2008) provide nuanced discussions.

[10] See general discussion of this issue in Spagat et al. (2009).

[11] See http://www.iraqbodycount.org/. The data we use were produced through a multiyear collaboration with IBC and contain several improvements on the publicly available IBC data, including more consistent geo-coding.

[12] The full data contain 21,100 incidents, 14 of which cannot be geo-coded to the governorate level and 2,612 of which cannot be geo-coded to the district level. Because media reports sometimes provide varying information on whether those killed were in fact civilians, or, less often, on how many civilians were killed in a given event, each incident in the data has a minimum and maximum value of civilian casualties. We use the minimum value of each civilian casualties variable to avoid coding combatant deaths as civilian.

[13] IBC separates killings that occur during the course of conflict with Coalition forces from those that occur elsewhere using information in press reports. Incidents are coded as "sectarian" in nature (category 3 above) when the killing is not incident to an attack on Coalition or Iraq targets and the perpetrator is a clearly identified militia.

A natural concern is that there is likely to be enormous noise associated with attributing casualties across these categories and that such measurement error would be nonrandom with respect to levels of insurgent attacks, posing significant problems for our analysis. To check for such a possibility, we investigate whether the percent of civilian casualties (both the number of casualties and the number of casualty-related incidents) in our "unknown" category is a function of the rate of insurgent attacks (variable described in next subsection). If it is not, then the measurement error is likely random with respect to our core dependent variable and therefore less of a problem. Results of this test are reported in the supporting evidence (SE Table 1A). Once we control for the sectarian composition of the area, or introduce district and time fixed effects, there is no clear relationship between unknown casualty events and attacks against Coalition and Iraqi government forces. This suggests that our coding of civilian casualties does not suffer systematic measurement error.

A related concern is that the probability an incident is excluded from our analysis because it lacks the information necessary to match it to a district location may be correlated with violence. If reporters avoid high-violence areas, for example, then districts with high levels of violence would have more missing data. By contrast, if the desire for a good story (or other career concerns) pushed reporters to cover the most dangerous places, we might see the opposite bias. Because our data include 2,612 incidents for which the governorate is known but the district is not, we are able to test for this possibility by analyzing whether the proportion of incidents at the governorate level that cannot be attributed to a specific district correlates with levels of violence. This test is reported in the supporting evidence (SE Table 1B). There is no significant relationship between levels of insurgent violence and the proportion of incidents that cannot be resolved to the district level.

Our analysis focuses mainly on civilian casualties attributable to Coalition forces or insurgents (categories 1 and 2 above), as these capture collateral damage. Sectarian violence and its relationship to insurgent violence are analytically and theoretically distinct from the concept of collateral damage and so we deal with those subjects elsewhere.

Attacks

Our measure of attacks against Coalition and Iraqi government forces is based on 193,264 "significant activity" (SIGACT) reports by Coalition forces that capture a wide variety of information about ". . .executed enemy attacks targeted against coalition, Iraqi Security Forces (ISF), civilians, Iraqi infrastructure and government organizations" occurring between February 4, 2004, and February 24, 2009. Unclassified data drawn from the MNF-I SIGACTS III Database were provided to the Empirical Studies of Conflict (ESOC) project in 2008 and 2009.[14] These data provide the location, date, time, and type of attack for incidents but do not include any information pertaining to the Coalition force units involved, Coalition force casualties, or battle damage incurred. Moreover, they exclude Coalition-initiated events where no one returned fire, such as indirect fire attacks not triggered by initiating insurgent attacks or targeted raids that go well. We filter the data to remove attacks we can positively identify as being directed at civilians or other insurgent groups, leaving us with a sample of 168,730 attack incidents.[15]

Civilian Population Ethnicity

To estimate the ethnic mix of the population we combined maps and fine-grained population data from LandScan (2008).[16] After collecting every map we could find of Iraq's ethnic mix, we geo-referenced them and combined them with the population data to generate estimates of the proportion of each district's population that fell into each of the three main groups (Sunni, Shia, Kurd). Using the figures from what we judged to be the most reliable map, a CIA map from 2003, we coded districts as mixed if no ethnic group had more than 66% of the population; otherwise the district was coded as belonging to its dominant ethnic group.[17]

[14]ESOC is a joint project based at Princeton University. It collects microdata on a range of conflicts, including Afghanistan, Iraq, Pakistan, the Philippines, and Vietnam.

[15]We thank LTC Lee Ewing for suggesting the filters we applied.

[16]The LandScan data provide worldwide population estimates for every cell of a 30" X 30" latitude/longitude grid (approx. 800m on a side). Population counts are apportioned to each grid cell based on an algorithm which takes into account proximity to roads, slope, land cover, nighttime illumination, and other information. Full details on the data are available at http://www.ornl.gov/sci/landscan/.

[17]An alternative approach is to code all parties participating in the December 2005 legislative election, which saw broad Sunni participation, according to their sectarian affiliation. Using that approach, one can calculate the vote share gained by each group's (Sunni, Shiite, Kurd) political parties. Unfortunately, the election results were never tabulated at the district level for security reasons and so that approach can only yield governorate-level estimates. Twenty-five of 104 districts are coded differently using these two approaches, mostly in districts that were coded as Sunni, Shia, or Kurdish using governorate-level vote shares but were coded as

Population

We use the World Food Program's population estimates generated in 2003, 2005, and 2007 as part of its food security and vulnerability analysis.[18] Using repeated observations of the population helps minimize the probability that our results are sensitive to biases driven by the substantial population movements Iraq suffered during the war. For districts in Erbil and Dahuk governorates that were not surveyed in the 2003 WFP survey, we use NCC Iraq estimates.

Descriptive Statistics

For this article we created a district-week dataset, from February 4, 2004, through February 24, 2009. Descriptive statistics of key variables for all of Iraq across this time period as well as for Sunni, Shia, and mixed areas are in SE Table 1C.

Figures 1–3 illustrate the variation in insurgent attacks and civilian casualties over time for the entire country and within select districts. For the entire country (Figure 1) we see a steady upward trend in attacks (right axis) until fall 2007. Total civilian casualties (left axis) follow a similar trend, but, looking at the breakdown by the armed actor responsible, we see that only sectarian killings mirror the macrotrend. Casualties attributed to Coalition forces and insurgents remain relatively stable throughout the war, with insurgent-caused casualties generally higher.

Most of Iraq's districts are relatively devoid of insurgent attacks on a per capita basis (SE Figure 1), but where there is violence there is noticeable variation in its severity and timing. The trend in violence, for example, does not look the same in two neighborhoods of Baghdad, Al Sadr (commonly known as Sadr City), and Karkh (the area across the Euphrates which contains the Green Zone), though both were quite violent at times (Figure 2). In terms of civilian casualties, we see similar patterns (Figure 3; SE Figure 2). Sectarian violence in our data is very highly concentrated in mixed districts, suggesting our coding rules accurately distinguish it (SE Figure 3).

Empirical Results

This section assesses the relationship between collateral damage and subsequent insurgent attacks. We first describe our identifying assumption and introduce a simple statistical model designed to deal with the range of potential complicating relationships that might exist between insurgent attacks and various types of civilian casualties. After some basic robustness checks, we then show how the relationships we identify vary across sectarian regions and levels of urbanity and present an alternative matching estimator as an additional robustness check.

Our identification strategy relies on the randomness inherent in weapons effects. The path of shrapnel, how a bullet ricochets, and the pattern of overpressure from an IED all have a large stochastic component, as does the physical location of noncombatants on a minute-to-minute basis. Once we control for how past levels of violence affect the general care with which civilians conduct their lives, then whether someone is standing in the wrong spot at the moment a misaimed round enters their house, or whether they happen to be walking by a window at the moment an IED creates fatal overpressure on their street, is largely random. The implication for estimating the effect of civilian casualties is that once one controls for the systematic sources of variation in both civilian casualties and insurgent attacks—Coalition units might operate more aggressively in pro-insurgent districts, for example—then we can give a causal interpretation to coefficients from a regression of current attacks on past civilian casualties.[19]

We check this identifying assumption in our preferred specification in detail below, but as a first cut consider one striking pattern: the week-to-week bivariate correlation in the number of insurgent attacks per district is .95, but only .04 for Coalition-caused casualties and .14 for insurgent-caused ones. To provide visual intuition, we plot (Figure 4) the weekly time series of insurgent attacks for Baghdad governorate's nine districts with the weekly time series in civilian casualties

mixed using the map-based method. It is not clear a priori which approach is more accurate. The vote shares are based on observed recent behavior and so constitute a direct measure, but suffer from aggregation issues. The ethnic population shares are based on fine-grained data but ultimately rest on an outside organization's guess as to the sectarian mix in Iraq.

[18]See WFP (2004, 2005, 2007) and http://www.wfp.org/.

[19]If they were available, this strategy would entail controlling for direct measures of each side's tactical aggression (number of rounds fired and the like). Such measures are not available for the Iraq conflict, but as Coalition and insurgent units moved around much less than monthly, and as Coalition units clearly developed distinct cultures with respect to their interaction with civilians, differencing and employing a range of time * space fixed effects seems likely to do fairly well at controlling for unit-specific factors. For evidence on variation in unit culture, see the numerous articles in *Military Review* and other military professional journals in which commanders frequently recount the particular methods that succeeded (or failed) during their tours. See, for example, Smith and MacFarland (2008).

FIGURE 1 Insurgent Attacks and Civilian Casualties in Iraq by Party Reponsible (Feb. 2004–Feb. 2009)

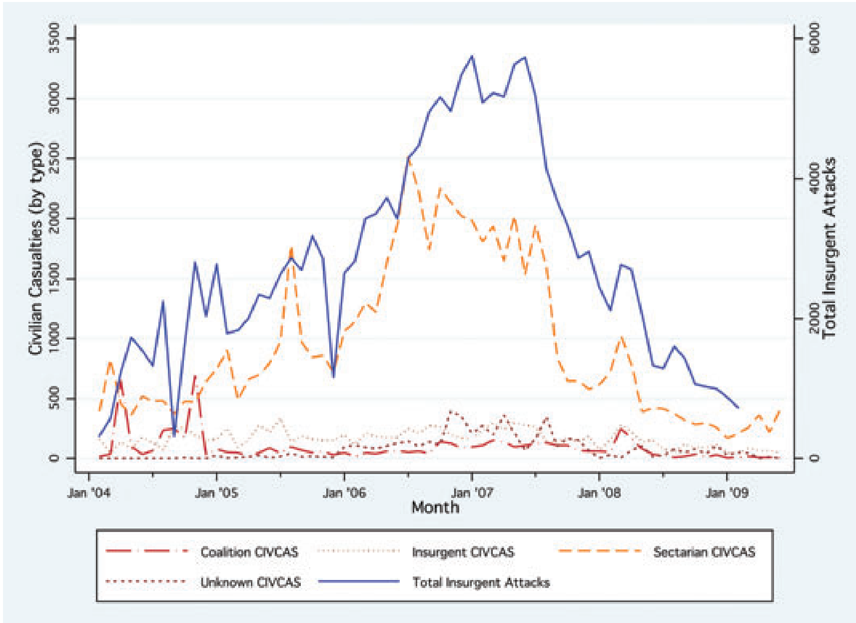
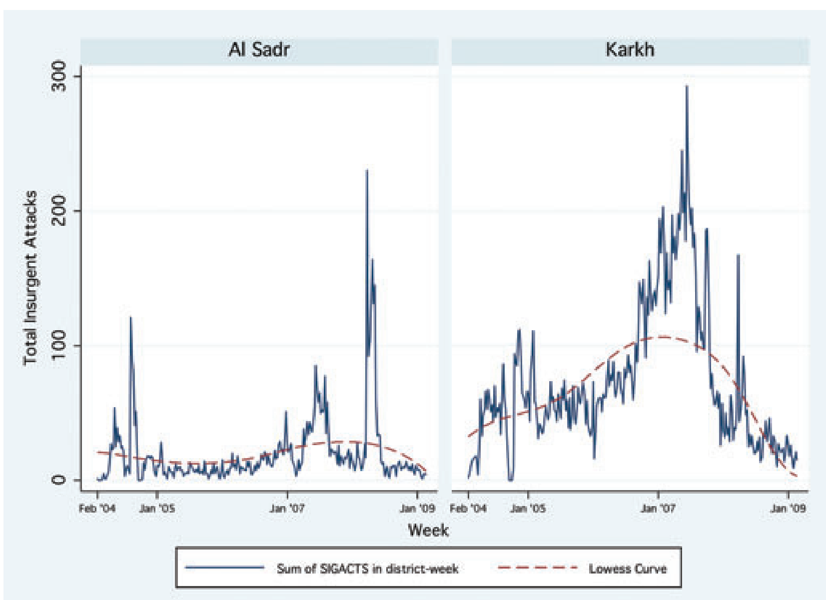


FIGURE 2 Insurgent Attacks in Al Sadr and Karkh (Feb. 2004–Feb. 2009)



attributable to Coalition and insurgent forces. There is no clear relationship between overall insurgent attacks and civilian casualties. Even the long-term relationship is largely absent in the two single-sect districts of Baghdad. In Al Sadr (a densely populated Shia area), the two time series seem to be strongly correlated. In Mahmoudiya (a Sunni suburb south of Baghdad) and Tarmia (a Sunni suburb north of Baghdad) there are high numbers of attacks but few civilian casualties. In Al Resafa (a mixed-sect central commercial district) there

FIGURE 3 Civilian Casualties in Al-Muqdadiya and Al-Musayab (Feb. 2004–Feb. 2009)

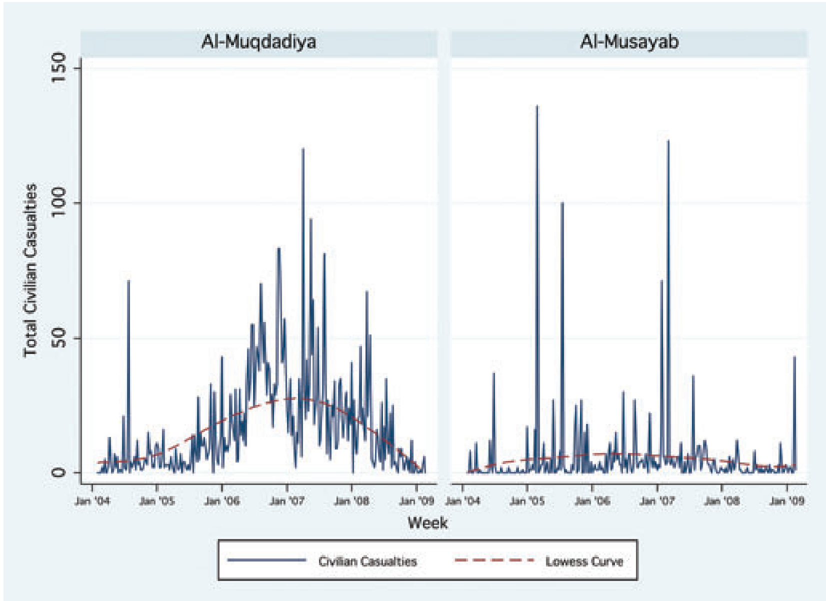
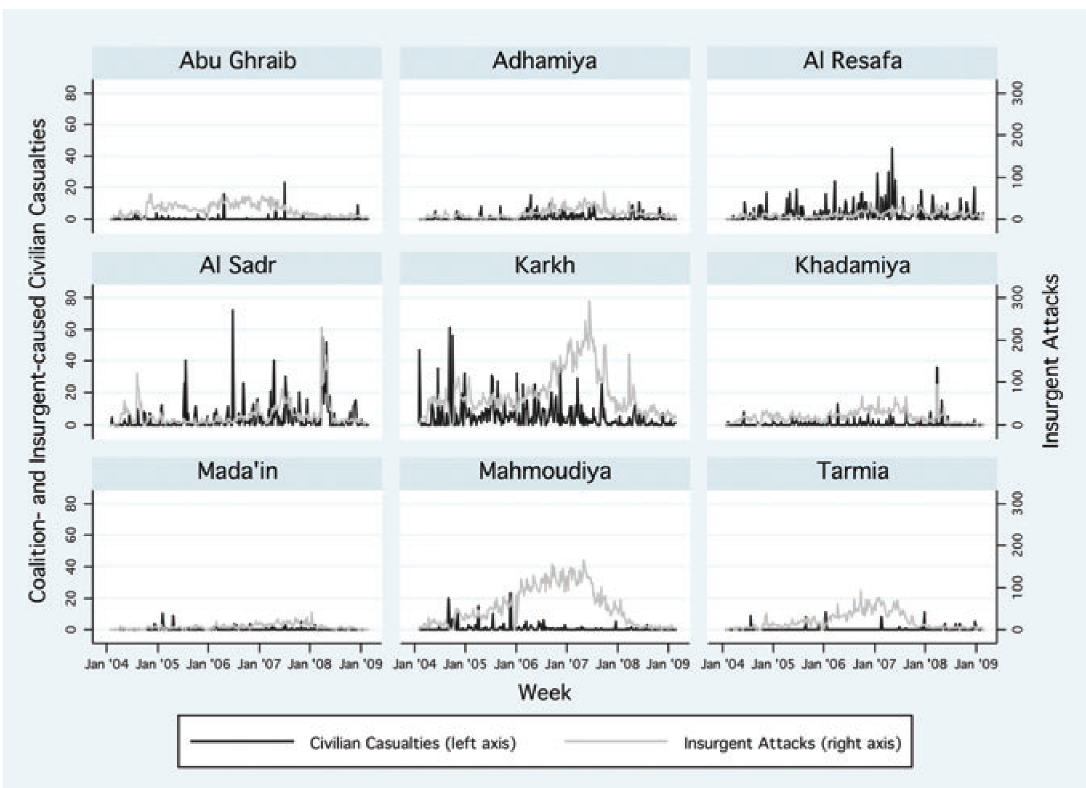


FIGURE 4 Insurgent Attacks and Civilian Casualties in Baghdad Districts (Feb. 2004–Feb. 2009)



are high numbers of civilian casualties, relatively few insurgent attacks, and the two time series seem uncorrelated. That is, while the area-specific trends in insurgent attacks are highly correlated over time, the same is not true for civilian casualties, which cannot be predicted well at all on the basis of previous levels. This is evidence of greater randomness in the occurrence of civilian casualties, which we exploit in trying to identify the causal effect of those casualties on insurgent attacks.

To motivate the statistical model, suppose that insurgent attacks in area i at time t are a function of the information available to government forces plus insurgent group-specific strategic goals plus time-varying district characteristics. The information that is available to the government is a function of how each side treated civilians in the recent past and underlying local attitudes. Assume that civilians respond to past actions so that information passed to the government is increasing in past civilian killings by insurgents and decreasing in past killings by the government and its allies. This leads to the hypothesis that attacks in t will be increasing in government killings of civilians in t-1 and decreasing in insurgent killings of civilians in t-1.

We test that prediction by estimating the following model, which uses a combination of differencing and fixed effects to control for factors affecting both civilian casualties and attacks:

$$a_{(i,t)} - a_{(i,t-1)} = \alpha(c_{(i,t-1)} - c_{(i,t-2)}) + \beta(\iota_{(i,t-1)} - \iota_{(i,t-2)}) + \mathbf{G}\mathbf{x}_{(i,t)} + \mathbf{S}_{(s,h)} + \mu_{(i,t)}$$

where $a_{i,t}$ is the number of insurgent attacks per capita in time t against Coalition and Iraqi targets while $c_{i,t-1}$ and $\iota_{i,t-1}$ are the number of civilians killed by the Coalition and insurgents,[20] respectively, in the previous periods.[21] There is no reliable time-series survey data for Iraq with subnational resolution and so we estimate the model in first differences to control for district-specific underlying political attitudes which might affect both noncombatants' propensities to share information and the aggressiveness of insurgents and Coalition forces.[22] As we do not know which of the 20-some insurgent groups operating in Iraq were responsible for each attack, we control for groups' political goals by including a sect-by-half-year fixed effect, $\mathbf{S}_{s,h}$. These fixed effects account for the mean level of insurgent attacks in each sectarian area for each half-year period and are designed to account for time-varying political factors such as the dramatic political realignment in Sunni areas from 2006 to 2007, commonly known as the Anbar Awakening. Time-varying district characteristics that impact how people live and thus their susceptibility to being killed are captured in a vector, $\mathbf{x}_{i,t}$, which includes population density and the unemployment rate. We estimate our model at the district level as this is the smallest geographic unit for which reliable time-series population data are available.[23]

Main Results

Our core results are presented in Table 1. We report the first differences specification above as well as the analogous regression in levels to highlight the need to control for underlying local attitudes via the differencing.[24] This approach is designed to pick up the average effect across areas with different levels of insurgent attacks.[25] Estimating the same regression only for district-weeks with above median levels of insurgent attacks yields somewhat stronger results.

Table 1 shows that Coalition-caused civilian casualties in *t-1* are positively associated with incidents of insurgent violence in period t and insurgent-caused civilian casualties are associated with less violence in the

[20]As explained above, insurgent killings are only those that occurred in the course of attacks against military or government targets, so they do not include intimidation killings.

[21]We population-weight attacks to avoid spurious correlations arising from the fact that the rate of attacks may be mechanically higher in places with more people (i.e., if 1% of the population are insurgents, a district with 500,000 people will have 4,000 more insurgents than a district with 100,000 people). We do not population-weight killings as we think it is unlikely that Iraqis take population numbers into account when processing information on civilian killings, that is, they read the news that 10 people were killed in district X as carrying the same weight whether district X has 100,000 people or 500,000 people. Our core results are strongest in mixed areas, and in these regions the results become substantially stronger when placing population-weighted civilian casualties on the RHS.

[22]Given the fact that we see strong district-specific trends in both time series (i.e., the time series are serially correlated), differencing is preferred to using fixed effects and estimating the regressions in levels.

[23]Unfortunately, there are no highly detailed data that would allow us to control for Coalition force levels. Concerns that this introduces omitted variable bias should be mitigated by the consistency of our results in models, including (1) the previous period's trend, (2) a district fixed effect to control for predictable sources of variation in the trend, and (3) those that employ nonparametric matching on the recent violent history of districts.

[24]Controlling for the average number of civilians killed by each side over the previous month does not change the results, which should ease concerns that differencing does not adequately control for the magnitude of previous attacks.

[25]The results should not suffer omitted variable bias because incentives for the mistreatment of civilians vary across Kalyvas' (2006) zones of control to the extent that the sect*half fixed effect and differencing account for which zone a district-week is in.

TABLE 1 Predicting Population-Weighted SIGACTs per Week (*Linear Regression*)

DV	(1) SIGACTs/100,000 population	(2) SIGACTs/100,000 population	(3) SIGACTs/100,000 population	(4) SIGACTs/100,000 population	(5) SIGACTs/100,000 population
Coalition Killings	0.00249*				0.00270**
(*lagged difference*)	(0.0014)				(0.0013)
Insurgent Killings		−0.0165**			−0.0168**
(*lagged difference*)		(0.0081)			(0.0081)
Sectarian Killings			−0.000667		
(*lagged difference*)			(0.0010)		
Unknown Killings				−0.0133***	
(*lagged difference*)				(0.0043)	
Constant	0.00901	0.00897	0.00900	0.00899	0.00898
	(0.0070)	(0.0070)	(0.0070)	(0.0070)	(0.0070)
Observations	26,416	26,416	26,416	26,416	26,416
R^2	0.002	0.002	0.002	0.002	0.002

Note: All models include sect*half-year fixed effects. Population density and unemployment rate variables not shown; coefficients are statistically and substantively insignificant. Robust standard errors clustered by district in parentheses.
***p < 0.01, **p < 0.05, *p < 0.10.

TABLE 2 Predicting First Difference of SIGACTs per Week as a Function of Civilian Casualties (*Linear Regression*)

DV: SIGACTs/100,000 population (*first difference*)	(1) Entire Country	(2) Sunni	(3) Mixed	(4) Shiite	(5) Kurdish
Coalition Killings	0.00270**	0.0265	0.00275**	−0.0108	−0.0694
(*lagged first difference*)	(0.0013)	(0.049)	(0.0011)	(0.0075)	(0.070)
Insurgent Killings	−0.0167**	−0.0323	−0.0176**	−0.00610	−0.0218
(*lagged first difference*)	(0.0081)	(0.053)	(0.0072)	(0.0039)	(0.055)
Constant	0.00897	0.0288	−0.00108	0.000898	0.00308
	(0.0070)	(0.045)	(0.011)	(0.0016)	(0.0038)
N	26,416	4,064	4,826	10,414	7,112
R^2	0.002	0.002	0.005	0.001	0.000

Note: All models include sect*half-year fixed effects. Population density and unemployment rate variables not shown; coefficients are statistically and substantively insignificant. Robust standard errors clustered by district in parentheses.
***p < 0.01, **p < 0.05, *p < 0.10.

subsequent period.[26] But is this effect causal? We believe the balance of the evidence suggests it is.

As a first cut, the results do not change if we control for a broad range of potential confounding factors. Controlling for changes in insurgent attacks in previous periods (ΔY_{t-1}) allows for the possibility that selection into certain levels of insurgent attacks and civilian casualties is due to historical processes captured by preexisting trends—Coalition units that have experienced increasing rates of attacks may tend to use more firepower, for example—and does not change the results (SE Table 2A). Neither do the results change if we add district fixed effects to control for the possibility that the error term in first differences is predictable across districts (SE Table 2A). The results in Table 1 are also robust to dropping Baghdad from the analysis (SE Table 2B), meaning they are not driven by the peculiarities of the sectarian conflict in that city.[27]

[26] An interesting finding in Table 2 is that unknown killings predict lower levels of attacks in subsequent periods. Since most unknown killings reflect intimidation by anonymous perpetrators (bodies found in the street and the like), this suggests selective insurgent violence is either counterproductive or a substitute for attacks on Coalition and Iraqi forces.

[27] SE Tables 2E–2K in the online supporting evidence provide additional robustness checks.

Two further concerns arise with Table 1. One concern is that the results may be driven by natural oscillations in the time series of attacks and casualties. A second concern is that because we rely on a temporal difference to identify the causal impact of past collateral damage on current levels of insurgent attacks, we may simply be seeing a correlation because trends in insurgent attacks and civilian casualties are both driven by some omitted variable. Placing the lead of differences in casualties on the RHS instead of the lag, a standard temporal placebo test, can check both possibilities. Leads of casualties have no predictive power, providing evidence that our results are not driven by oscillation or parallel trends (SE Table 2C). Put in plain English, future changes in civilian casualties are not correlated with current changes in insurgent attacks, but past ones are, on average and in mixed areas.

Finally, the most serious concern with a causal interpretation of the results in Table 1 is the possibility that the number of civilians being killed by the Coalition or insurgents reflects their expectations about the level of attacks in subsequent periods. Suppose, for example, that Coalition forces correctly predicted high levels of insurgent violence in the near future. One rational reaction could be to increase the number of raids and aggressive checkpoints today, leading to more civilian casualties today. This seems unlikely given how robust the results are to including past trends and district fixed effects, but we test for this in two additional ways.[28]

First, following a suggestion in Wooldridge (2002), we tested for the possibility that casualties in *t-1* are driven by (correct) anticipation of future attacks by testing H_0: $\gamma = 0$ in the equation $\Delta \mathbf{a_t} = \Delta \mathbf{x}_{t-1}\mathbf{b} + \mathbf{w}_{t-1}\gamma + \Delta \mathbf{u_t}$, where $\mathbf{w}_{t-1}$ is the vector of Coalition- and insurgent-caused civilian casualties in *t-1* (the subset of the variables whose endogeneity concerns us) and $\mathbf{x}_{t-1}$ is the matrix of coefficients described above, including differences in civilian casualties. Using a robust Wald test we fail to reject the null for insurgent casualties in all models, which amounts to formally showing that the number of civilian casualties in period *t-1* is uncorrelated with the residuals in our core model. For Coalition-caused casualties in Table 1, we reject the null at the p = .900 level in Model (1) and at the p = .953 level in Model (5), suggesting we have some reason for concern about selection effects with respect to Coalition attacks. When, however, we drop the 693 district weeks that cannot be matched with other districts on their history of violence (as described below in the matching section) and experience more than one civilian casualty caused by both actors, the coefficient estimates become slightly larger in the same direction and the p-values on the test for exogeneity drop below the 90% level in both Model (1) and Model (5) (p = .867 and p = .880, respectively). This gives us confidence that the selection bias which remains in our core specification is unlikely to be driving the result. In our second test, we directly check for selection effects by regressing various types of civilian casualty levels on the rate of insurgent attacks in the next period. The lead of insurgent attacks does not predict Coalition-, insurgent-, or sectarian-caused civilian casualties once we difference to account for district-specific characteristics (SE Table 2D), providing further evidence that civilian casualties are affecting subsequent insurgent attacks on Coalition forces, not the other way around.[29]

On balance, Table 1 and the various robustness checks provide good evidence that Coalition-caused casualties lead to increased levels of insurgent violence against Coalition and Iraqi forces, while insurgent-caused casualties have the opposite effect. Given the nature of the data and aggressive set of robustness checks we employ, the balance of the evidence clearly supports the hypothesis that Coalition-caused collateral damage leads to increased attacks and insurgent-caused collateral damage leads to reduced attacks.

Geographic and Functional Heterogeneity in Results

So how do these results vary across areas? We find two key patterns. First, the effects we observe are being driven by districts whose population is more urban than the median district, where 48.5% of the population lives in urban areas according to the WFP surveys (SE Table 2F). The anti-Coalition effect is absent in nonurban districts (in fact, the effect is in the opposite direction) and the anti-insurgent effect is severely muted.

Second, and more critically, the results vary tremendously by the sectarian character of an area. Table 2 reports our core specification from Table 1 (Model 5) broken down by sectarian mix, showing the effects are being driven by mixed areas.[30]

In mixed areas, a one standard deviation increase in the number of insurgent-caused civilian casualties

[28]We thank one of our anonymous reviewers for emphasizing the need to formally test for this possibility in a range of ways.

[29]Note that we have the expected spurious correlations in levels without a district fixed effect in SE Table 2D, which provides additional confidence that differencing is accounting for most of the selection bias.

[30]SE Table 2M reports the same results dropping the 7.6% of incidents (n = 397) in which both Coalition forces and insurgents killed civilians. The results are substantively the same except that in the full sample, the coefficient on insurgent killings is no longer statistically significant.

predicts approximately 0.5 fewer attacks in the next week for a 12% drop from the average number of attacks per 100,000 in mixed areas.[31] Interestingly, the effect is substantively larger in Sunni areas, especially the anti-Coalition effect, but is less consistent and so is not statistically significant.[32] Notice that these results are not what we would expect to find if it were true that, in the context of the civil war and sectarian violence, civilians in mixed areas were more likely to look to various militias or insurgent groups for protection and were thus likely to stay quiet and share no information with Coalition forces for fear of losing their local protection. If that story described civilian incentives in mixed areas, we would expect a null finding on insurgent-caused casualties in mixed areas.

Finally, we study how the impact of civilian casualties varies across four types of insurgent attacks: (1) direct fire attacks involving weapons that can only be used with a direct line of sight to the target, such as rifles and the like, thereby exposing insurgents to the risk of detection while setting up and to a high chance of death or capture during the incident; (2) indirect fire attacks involving weapons such as rockets which can be fired far from their target, implying less risk of exposure during setup and none during the incident; (3) IED attacks that involve planting an explosive which is detonated when Coalition forces pass by, involving some risk of exposure during setup, some risk to the attacker during the incident if the weapon is command detonated, and some risk of failure if civilians tip Coalition forces off to the weapon's presence; and (4) suicide attacks which are typically very resistant to exposure during setup (Berman and Laitin 2008). As SE Table 2G shows, the symmetric anti-Coalition and anti-insurgent effects are present for direct fire and IED attacks. The rate of indirect fire attacks, however, is decreasing in Coalition-generated casualties and the rate of suicide attacks is decreasing in insurgent-generated casualties. The overall effects we identify thus seem to be driven by the two forms of attacks that we believe to be most sensitive to information sharing by the population.

Alternative Matching Estimator

This section presents an alternative semiparametric approach to estimating the causal impact of civilian casualties on subsequent insurgent violence. The basic idea for this alternative, less model-dependent approach is that we can estimate the causal effect of collateral damage on subsequent violence by comparing outcomes across districts/weeks that are matched on factors influencing the propensity of both sides to kill civilians, such as average levels of violence or whether one side or the other feels it is winning in an area. Many such factors are unobservable, but we might think most of the information about them is captured in the history of violence through time t in district i. If we look at the set of district-weeks that have experienced similar levels of insurgent attacks in the past—say $t-4$ to t—as well as similar trends over that period, then we might think that Coalition and insurgent forces operating in those district-weeks would face similar incentives regarding the use of force and level of care taken to avoid civilian casualties. Put more starkly, insurgents who believe they are losing an area might be expected to operate more aggressively than they otherwise would.

This expectation suggests a simple analytical path:[33] (1) use a matching algorithm to identify district-weeks with similar histories; (2) within each stratum, use a regression model to estimate the relationship between the number of civilians killed today and the number of insurgent attacks in the next period using a cubic time trend at the quarter level to control for broad secular trends;[34] and (3) take the average of these results weighting by the size of the strata. The resulting estimate provides the average treatment effect for district-weeks that experience any history of violence represented in the set of strata used at step (2).

We match district-weeks using the Coarsened Exact Matching (CEM) algorithm implemented in the *cem* package for Stata (Iacus, King, and Porro 2008). The procedure is quite simple. First, coarsen the data on each matching variable so that it falls into meaningful bins, just as one would when constructing a histogram. For the average number of attacks in the last five weeks, for example, the bins might be zero attacks, one attack, two to five attacks, and so on. Second, perform exact matching on the coarsened data so that all district-weeks with roughly the same history and intensity of violence are placed in a common stratum. This procedure does not use a parametric model for selection to treatment and so is very amenable to matching for continuous treatment variables. It also has a variety of desirable properties relative

[31]SE Table 5 reports substantive significance in detail, showing the effect of a 1SD increase in the number of civilian casualties on the number of insurgent attacks for the core model estimated on different periods of data.

[32]The difference in statistical significance is not due to sample size; our coding identifies roughly the same number of Sunni and mixed districts.

[33]We thank Kosuke Imai for suggesting this approach.

[34]Many strata are small and so we believe this approach strikes a better balance between losing small strata and robustness of the control than does half-year or quarter fixed effects.

to more commonly used methods such as propensity score matching, including reduced model dependence.[35]

Our core matching solution replicates the intuition behind the first-differences specification. To capture incentives created by the level of insurgent attacks, we match on average rate of attacks per 100,000 people in periods *t* to *t-4* by dividing that rate into five bins at the 10th, 33rd, 66th, and 90th percentiles of the average. To capture incentives driven by the history of violence, we code the differences in insurgent attacks week to week according to whether they increase, remain approximately the same, or decrease.[36] This three-level coding over five periods leaves us with 243 possible histories, of which 241 are observed in the data. This approach leaves us with 493 strata with district-weeks in both treatment and control groups and is justified to the extent that we believe matching on past insurgent violence effectively controls for characteristics impacting the propensity of actors to kill civilians.[37]

Table 3 summarizes the matching approach in two ways. First, we report the average marginal effect of killing one additional civilian on future insurgent attacks per 100,000 in a district-week. Second, to reduce model dependence further we report results where we have dichotomized the independent variable, providing the difference in mean levels of future insurgent attacks per 100,000 people between weeks in which Coalition or insurgent forces kill no civilians and weeks in which they kill one or more civilians.

Three facts stand out from this matching exercise. First, we can confirm our previous findings on Coalition-caused casualties for the entire country and for mixed areas. In the entire country we find a significant positive treatment such that each additional civilian killed by Coalition forces predicts approximately 0.16 additional attacks in the following week per 100,000 population. This effect is fairly substantial. The median Coalition-caused incident resulted in two civilian deaths. This

[35]See Iacus, King, and Porro (2008) for a detailed comparison of CEM to other matching techniques.

[36]The cut-point for an increase or a decrease is a movement of more than one insurgent attack per 100,000 residents, approximately the 50th percentile of the absolute value changes for the 12,714 district-weeks in which the number of attacks changed from the previous week. SE Figure 5A shows the results from calling movements beyond the 25th percentile of movement (a change of .5 in attacks per 100,000) a –1 or 1. That match achieves similar balance, but over time civilian casualties appear to have slightly different patterns.

[37]The challenge in doing this matching is to coarsen the data so that in matched strata there is zero contemporaneous correlation (or close to it) between insurgent attacks and civilian killings—i.e., within matched strata civilian killings at $t = 0$ are uncorrelated with past insurgent violence—without matching so finely that there are too few district-weeks in each history. Full replication code available from the authors.

TABLE 3 Matching Estimate of Impact of Coalition or Insurgent Killings in Period *t* on SIGACTs/100,000 at Period *t*+1

	(1) **Entire Country**	(2) **Entire Country**	(3) **Sunni**	(4) **Sunni**	(5) **Mixed**	(6) **Mixed**
Treatment	*# of Coalition Killings*	*# of Insurgent Killings*	*# of Coalition Killings*	*# of Insurgent Killings*	*# of Coalition Killings*	*# of Insurgent Killings*
Marginal Effects	**0.160**	−0.019	**0.378**	−0.023	**0.160**	−0.019
	[0.08 – 0.24]	[−0.05 – 0.01]	[0.11 – 0.64]	[−0.11 – 0.05]	[0.08 – 0.24]	[−0.05 – 0.01]
N of Matched District-Weeks	16398	16398	4229	4229	5192	5192
Treatment	*Coalition Kill > 0*	*Insurgent Kill > 0*	*Coalition Kill > 0*	*Insurgent Kill > 0*	*Coalition Kill > 0*	*Insurgent Kill > 0*
Difference in Means	**0.371**	−0.011	**0.760**	−0.071	**0.333**	−0.013
	[0.22 – 0.52]	[−0.11 – 0.09]	[0.28 – 1.23]	[−0.27 – 0.14]	[0.22 – 0.52]	[−0.11 – 0.09]
N of Matched District-Weeks	20766	20766	4229	4229	5192	5192

Note: Results significant at the 95% level in two-tailed test in bold with 95% confidence intervals in brackets. Matched on average SIGACTs/100,000 population in periods *t*, *t*-1, *t*-2, *t*-3, *t*-4, and trends over previous four weeks. Trends are history of changes in rate of SIGACTs/100,000 codes as 1 if rate increase by more than 1, 0 if it stayed about the same, and −1 if it dropped by more than 1. There are 243 possible histories, of which 241 are found in the data. This match created 700 strata, of which 504 had three or more district-weeks. Multivariate L_1 distance for match = 0.419, pre-match L_1 distance was 0.666. Regressions run within matched strata; table reports mean and 95% CI for within-strata estimates weighted by stratum size. Results do not include the three extremely large strata with more than 300 district-weeks, all of which had very little violence.

means that for an average district in Iraq—which has 277,238 residents—an average Coalition-caused incident results in roughly 0.9 extra insurgent attacks on Coalition forces in the subsequent week. Second, there is also evidence that insurgent-caused collateral damage leads to fewer insurgent attacks, though the effects are statistically weaker in the matching estimate. An average insurgent-caused incident involves 3.7 civilian deaths, meaning that it predicts roughly 3.6 fewer insurgent attacks on Coalition forces in the next week in the median-sized mixed district of roughly 559,900 people. Third, the positive finding for Coalition forces is substantially larger in Sunni areas, while the negative one for the insurgents is statistically absent.

Figure 5 provides a graphic intuition for these results. The x-axis in each plot is the number of weeks before or after period t. The y-axis in the top plot is the average marginal effect of Coalition civilian killings in time t on SIGACTs/100,000 population for the entire sample. The y-axis in the bottom plot is the average marginal effect of insurgent civilian killings, where those effects are calculated by averaging coefficients from regressions run within matched strata weighting by the size of those strata.

If our procedure matched effectively and there is no causal impact of past insurgent attacks against Coalition forces on current civilian casualties within matched strata, then these differences will be close to zero through period t (or at least relatively constant) and will then spike up (or down) for at least one period after week t, reflecting the effect of killing civilians.[38] These plots confirm that our matching exercise effectively controls for selection on some unobservable characteristics, at least to the extent that those unobservables would have led to differential pretreatment trends in violence. As in our core specification, greater violence against civilians by the Coalition predicts higher levels of insurgent attacks. Once we match on past histories in this manner, greater violence by insurgents against civilians (in the course of attacks on Coalition and Iraqi government forces) appears to be a weaker predictor of lower levels of attacks than in the parametric results. These plots also provide strong intuition for how to think about the duration of the treatment effect. In the Coalition cases, the treatment effect lasts two to five weeks before dropping back to statistically insignificant levels. In the insurgent case it lasts one week, is followed by increased violence in $t+2$, and then returns to levels that are higher than, but statistically indistinguishable from, the pretreatment trend.

In interpreting these plots a note of caution is warranted. The results from the matching exercise do not exactly match the parametric results. They are, after all, different estimators being applied to different samples (many district-weeks are dropped from the matched sample). What seems clear from Figure 5 is that there is a large positive change in the rate of insurgent attacks following Coalition killings. That lasts for at least two weeks. There is a smaller negative change in the rate of attacks following insurgent killings but it is much less dramatic, and there is a positive pretreatment correlation between insurgent killings and the rate of attacks in the matched sample. SE Figure 5A shows the same plot where we employ a slightly looser definition of changes in insurgent attacks to generate the history and use a cubic time trend within matched strata to control for residual imbalance (instead of the year fixed effects and linear time trend in the main results). Here the effects through $t+4$ are more cleanly in line with the patterns in the parametric results, but there is a long-run difference in insurgent attacks presumably driven by omitted variables not accounted for by the cubic time trend. This figure highlights the fact that while the exact shape of the semiparametric results depends on how we match, as they should, the substantive results are similar over four to five weeks across a range of specifications. There is a clear increase in insurgent attacks after Coalition-caused casualties for several weeks and a less dramatic one-week decrease in attacks after insurgent-caused casualties.

What's the Mechanism?

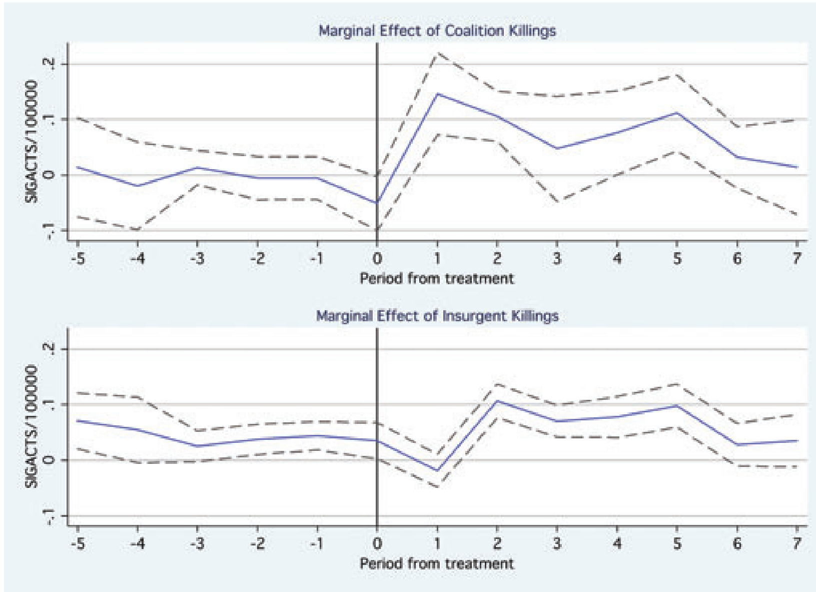
This section considers two sets of explanations for our findings. The first set consists of theories about the nature of counterinsurgent warfare that imply a positive correlation between insurgent attacks and past civilian casualties as a consequence of Coalition unit organization and tactics. The second set consists of explanations residing in the ways in which collateral damage impacts noncombatant preferences.

Counterinsurgent Organization and Tactics

Two mechanisms implied by prominent theories of counterinsurgent tactical behavior imply results similar to ours. The first mechanism is based on arguments about the impact of Coalition unit tactical decisions—whether

[38] Even if our approach matches district-weeks correctly on the motivations to mistreat civilians, the mechanical correlation between attacks and the probability of civilians being killed may create a residual positive correlation in $t = 0$.

FIGURE 5 Effect of Civilian Killings on Insurgent Violence in Matched Strata (Matching on Four-Period History of SIGACTs/100,000, and Average Insurgent Attacks)



soldiers are on foot or mounted in vehicles.[39] Lyall and Wilson (2009) reason that mounted patrols, as opposed to foot patrols, are less able to foster relationships with the population and gather valuable intelligence information about local activity. Furthermore, these units are more likely to breed enmity among civilians because of the inconvenience posed to civilians and the disruption of their daily lives by mechanized patrols. These factors combined to lead to higher levels of insurgent attacks in areas patrolled by mounted vehicles.

Lyall and Wilson's (2009) theoretical logic suggests two dynamics by which civilian casualties would increase in areas with more mounted patrols. First, in response to mounted patrols, insurgents could substitute into larger explosives, meaning that insurgent-caused civilian casualties would increase. Second, mounted patrols have access to heavier weaponry, which are more likely to cause civilian casualties even if aimed accurately. Suppose that more mechanized units tend to get attacked more because they have less information. The first dynamic would create a spurious positive correlation between killings by the insurgents and attacks because the kinds of units that were being attacked more would also be the units being attacked with weapons most likely to lead to insurgent-caused casualties. The second dynamic would create a similar spurious correlation between killings by the Coalition and attacks because the kinds of units that were being attacked more would also be the units equipped with weapons most likely to lead to Coalition-caused casualties.

Simple physics dictate that the dynamics above would operate most strongly in areas of higher population density where the consequences of an errant .50 caliber round or large IED are more likely to kill civilians. Thus, if there is a spurious positive correlation between killings and insurgent violence that is driven by mechanization—which we cannot directly test because reliable data do not exist on units' areas of operation (AO) in Iraq or on the extent to which tactical behavior correlated with unit equipment (i.e., armored units patrolling mounted to a greater extent)—we should also find that the ratio of civilian casualties to attacks should be higher in more urban and more densely populated districts. We test this logic by regressing ratios of civilian casualties to insurgent attacks on the percent of the district that is urbanized and on the district's population density.[40] These ratios are intended to capture how precisely the different parties employ

[39]See discussion in supporting evidence for more details on these alternative hypotheses and how we ruled them out.

[40]One would be concerned that these regressions are hopelessly endogenous if more mechanized units were sent to areas experiencing higher levels of violence. We think this concern is largely unfounded. There was no deliberate effort to match more mechanized BCTs to more violent areas. Private communication, LTC (Ret.) Douglas Ollivant, Ph.D., September 8, 2009. From October 2006 to December 2007, Ollivant was Chief of Plans for Multi-

violence with respect to civilians.[41] We find no evidence the ratios are higher in areas of denser population or with a higher percentage of urban populations (SE Table 3). The link between unit characteristics and civilian casualties is unlikely to be driving the results.

The second mechanism is that civilian killings by Coalition forces would correlate positively with insurgent attacks because Coalition units that engage less with the local community in their AO both kill more civilians and suffer more insurgent attacks. One proxy for community engagement by U.S. forces is the initiation of small-scale reconstruction projects by military units under the Commander's Emergency Response Program (CERP).[42] If better information flowing from engagement with communities allows units to be more discriminate, we should see a drop in the ratio of civilians killed by the Coalition per attack. If better information makes it harder for insurgents to operate, we should see an increase in the ratio of civilians killed by insurgents per attack. Put formally, if a relationship between engagement (i.e., interacting directly and repeatedly with civilians) and precision (ability to engage insurgents without causing collateral damage) were driving our results, then the ratio of Coalition-caused civilian casualties per attack should be negatively correlated with the number of CERP projects initiated and the ratio of insurgent-caused civilian casualties per attack should be positively correlated with the number of projects initiated.

We test for this alternative explanation using two proxies for engagement, the number of CERP projects started in a given district-quarter and the total value of those projects in millions of dollars.[43] Regressing casualty ratios on these proxies, we find that neither the number of projects nor levels of spending are associated with overall casualty ratios, or casualty ratios for any specific actor (SE Table 4). This increases our confidence that the relationships we observe are not driven by the fact that units which do not engage with the communities kill more civilians and suffer more attacks.

Civilian Agency and the Informational Mechanism

There are many ways in which noncombatant reactions to collateral damage might impact subsequent levels of violence. The simplest is that communities might clamor for revenge when the Coalition causes civilian casualties, leading insurgents to conduct more attacks, and might put pressure on insurgents to rein in attacks after insurgent-caused casualties.[44] This explanation seems unlikely to generate the patterns above for three reasons. First, for there to be an increase in insurgent attacks because the population clamors for revenge, insurgents would have to be producing fewer attacks than they were capable of perpetrating before the civilian casualty incident(s). That seems unlikely as a general trend, though it might be true in some places at some times. Second, this "revenge" mechanism does not have a clear prediction for variation across more or less urban districts. Third, if insurgents responded to calls to be more discriminate after they caused casualties, we would expect the reduction in attacks to be strongest for indirect fire attacks (those involving mortars and rockets), which are the least discriminate form of insurgent attack. It is, in fact, positive and statistically insignificant (SE Table 2G).[45]

Given this and the findings above, we need a mechanism that (1) assumes insurgents produce at capacity (or at least at the limit of what the population will tolerate as in the Berman, Shapiro, and Felter [forthcoming] "hearts and minds" model); (2) predicts the impact of civilian casualties will be strongest in urban areas and mixed sectarian regions; and (3) implies a differential response for indirect fire and suicide attacks, which are less sensitive to information leakage than other forms of attacks.

One such mechanism is the combination of "sensible" civilian reactions to collateral damage with the critical role of information in Iraqi insurgency, what we term the "informational mechanism." Versions of this mechanism have been implicit in the long tradition of analysis that emphasizes intelligence collection as the fundamental task for counterinsurgents (e.g., Galula 1964; Kitson 1971; Thompson 1966). It seems quite apt in Iraq where the ability of Coalition forces to capture and/or kill insurgents rests not on the Coalition's ability to project combat power (they have that in spades), but on the acquisition of reliable information on the whereabouts of insurgents.

national Division-Baghdad and was lead Coalition force planner for the development and implementation of the Baghdad Security Plan in coordination with Iraqi Security Forces.

[41] See SE for a complete discussion of these ratios and the sensitivity analysis we conducted on them.

[42] This is a noisy proxy given variation in CERP allocation practices at the division, brigade, and battalion levels. Based on many interviews we believe the average correlation between CERP activity and community engagement is positive.

[43] See Berman, Shapiro, and Felter (forthcoming) for a complete discussion of these data.

[44] We thank two of our anonymous reviewers for pointing out the need to discuss this mechanism.

[45] The coefficient on the impact of lagged differences in insurgent-caused casualties on differences in indirect fire attacks is positive (.0009) and statistically insignificant ($t = .5$).

The conflict in Iraq is thus quite different from insurgencies where the government's capacity to produce violence is in question.[46]

Three commonsense assumptions about information in such settings lead directly to the core results above. First, the less information shared with government forces and their allies, the easier it is for insurgents to operate and the more attacks there will be against Coalition and Iraqi forces.[47] Second, the more civilians Coalition and Iraqi government forces kill, the less information other civilians share with them voluntarily, and symmetrically, the more civilians that insurgents kill in the course of their attacks on Coalition and Iraqi government targets, the more information civilians share. Third, Coalition and Iraqi forces want fewer insurgent attacks, and insurgents want to conduct more (given, of course, that they are not engaged in negotiations or a ceasefire). While each assumption may seem obvious, the first two merit a bit of unpacking.

With respect to the assumption that information was the key constraint on violence in Iraq, there is little qualitative evidence that the supply of fighters was an important constraint for any of the insurgent organizations. There is, however, copious evidence that insurgents were very concerned with the population's willingness to cooperate with them.[48] Even if information was not the sole constraint on insurgents' production of violence in Iraq—as it is certainly not in many conflicts—our unit of analysis naturally draws attention to it. There is little reason to expect the impact of recruits to be highly localized in time or space. Insurgent groups move personnel around for a variety of reasons, and the time lag from decision-to-join to attack can vary from days to months. The impact on violence of the population's willingness to share information, however, could be quite localized in both time and space. Because our analysis focuses on how changes in civilian casualties from one week to the next impact subsequent violence within confined geographical areas, it effectively focuses on factors that can respond almost immediately to changes in the treatment of civilians. We believe this approach zeroes in on the impact of informational processes as opposed to recruiting ones.

With respect to the second assumption about how collateral damage impacts information sharing, there is a range of reasons collateral damage could affect civilians' propensity to share information with government forces in a "sensible" manner. Adjudicating between them is not our goal, but it is useful to lay out just three of them. First, civilian casualties transmit information to the population about how a government established and organized by each armed actor would treat them. As collateral damage increases, the natural inference is that the actor responsible places relatively less value on the lives of (certain) civilians. Collateral damage therefore provides evidence about how a government organized and populated by insurgent- or Coalition-selected officials will value citizens' livelihoods in the future. If information shared by one individual can have a substantial effect on insurgents' ability to produce violence against Coalition forces, then one logical reaction of forward-looking civilians to mistreatment is to share more or less information.

Second, civilian casualties transmit information on the threat that each side poses to the civilian's physical security in a specific time and place.[49] Civilians seeking to reduce the threat to themselves and their families can therefore sensibly take actions that support the less dangerous armed actor. Certainly intimidation by one side or the other can dampen this process, but on the margins citizens caring about their personal security should become more or less willing to share information as one side or the other reveals how much of a threat they pose to civilians. The balance of opinion in the policy and military communities is that even clearly unintentional civilian casualties by government forces lead to reduced cooperation from civilians (see, e.g., Their and Ranjbar 2008).

Third, civilian casualties can create motivations for revenge on the part of noncombatants. If civilians are aware that sharing information with counterinsurgents is costly for insurgents, then one way in which they can exact retribution for harm caused by insurgents is to share information with government forces and their allies, and vice versa for harm caused by government forces. Calling

[46]The massive superiority of government forces is, however, common to many counterinsurgency operations through history, so understanding what impacts the supply of information has broad relevance.

[47]Note that we focus on information shared with government forces. The relationship between information shared with insurgents and the number of attacks is less clear, as we might expect insurgents lacking precise information on their enemies to engage in more imprecise attacks, shooting rockets into the Green Zone, for example. Information shared with the Coalition, however, leads to raids and arrests, which we expect to directly affect both the number and the quality of attacks insurgents can conduct.

[48]See, for example, the copious collection of Iraqi insurgents' internal documents cited in Fishman (2009).

[49]Violence in Iraq was highly spatially clustered, with clusters of violence shifting dramatically over time. Civilian casualties could thus provide new information on the current threat. The bivariate correlations between the count of Coalition-caused civilian casualty events in a given district-week and the numbers 5 and 10 weeks before in the same district are .25 and .18. The correlation between the number of Coalition-caused casualties in the present week and in past weeks is even lower, approximately .03 at both 5 and 10 weeks back.

in tips requires less of a commitment, in terms of time and risk, than joining a pro- or antigovernment militia and so we might expect the marginal effect of civilian casualties on information sharing to be relatively large compared to the effect on participation—a slightly different revenge mechanism whose predictions we discussed above.

A natural concern here is that the marginal impact on subsequent insurgent violence of any one civilian's decision to share information should be trivial, so that substantial civilian coordination would be necessary to meaningfully affect insurgent violence. If true, there would clearly be a sizable collective action problem to overcome in the context of civil conflict, and the reactions described above would not be "sensible." In Iraq, however, individual decisions could have substantial impact. As a general rule, Coalition forces would conduct raids based on two independent sources of information and would take even single-sourced information into account when planning patrols and the like.[50] As even one successful raid could dramatically reduce insurgent capacity in a given area for weeks or even months, the decisions of just one or two civilians could have massive implications for levels of insurgent attacks.[51] Over time, Coalition forces became very effective at collecting information in ways that minimized risks for informants (Anderson 2007). This meant that sharing information in response to civilian casualties was indeed sensible in that it carried relatively little risk and could have large impacts.

Unfortunately, we cannot directly assess the impact of collateral damage on information flow to counterinsurgents because no unclassified data exist on such information transfers. Intelligence from human sources (HUMINT) is among the most highly classified types of information held by the U.S. military, and no data on tips provided to Coalition forces in Iraq exist in unclassified form. Instead, as is often the case in political science, what we can do is make a cautious case that the results are consistent with the observable implications of the information mechanism.

That case rests on four findings. First, increases in Coalition-caused civilian casualties at time t predict increased insurgent attacks at time $t+1$ in that district and increases in insurgent-caused casualties predict reduced insurgent attacks at $t+1$.[52] This symmetric average response is a prediction of the informational mechanism that is not shared by the revenge mechanism. While other mechanisms could predict such a response, they are unlikely to be driving the results, as discussed in the previous subsection. Second, the effects come from predominantly urban districts. In urban areas there are more people around who can observe what insurgents are doing, which means that (a) the number of people who could share operationally relevant information is greater and (b) it is harder for insurgents to identify informants and thus the sensitivity of information sharing to casualties should be higher. Third, the effects are strongest in mixed sect areas, exactly the places where ingroup policing is harder. In such areas intimidation of civilians to prevent them from responding "sensibly" was likely harder given the presence of multiple competing militias.[53] Moreover, in mixed areas there were, due to the nature of the war, both people strongly opposed to the Coalition and people strongly opposed to the insurgency. Fourth, the impact of civilian casualties on indirect fire attacks is in the opposite direction of the main effect. If insurgents substitute into tactics which are less sensitive to information sharing when the population becomes more willing to talk to counterinsurgents, that is exactly what we should see.

Overall then, the informational mechanism seems to be the most likely of the explanations we examined because (1) it rests on the plausible argument that if civilians share less information with counterinsurgents, then insurgents can produce more attacks because they are losing fewer men and weapons to raids and the like; and (2) it has implications for variation in the impact of collateral damage across districts and types of attacks that are largely borne out.

Conclusion and Policy Implications

This article answers a simple question: what are the military consequences of collateral damage in intrastate conflict? Using weekly time-series data on civilian casualties and insurgent violence in each of Iraq's 104 districts from 2004 to early 2009, we show that both sides pay a cost for causing collateral damage. Coalition killings of civilians predict higher levels of insurgent violence and insurgent killings predict less violence in subsequent periods.

[50]Coalition press releases often cited information from single-sourced tips as driving raids. See, for example, Multinational Division-Baghdad (2008).

[51]See Berman (2009, 29–59) for a thorough discussion of why defection and information dramatically inhibit insurgents' capacity to produce violence.

[52]Mechanisms other than information sharing could generate similar dynamics, but we provide evidence against the most likely of those above, and two of our findings follow most clearly from an informational perspective.

[53]In ongoing work, Shapiro and Weidmann (2011) find that increasing cell phone coverage leads to reduced insurgent attacks, a finding they argue is driven by the role cell phones play in making it safer to share information with counterinsurgents.

These findings match those of Kocher, Pepinsky, and Kalyvas (2011) insofar as aerial bombings in Vietnam caused civilian casualties and therefore did more to reduce population cooperation with counterinsurgents than they did to reduce insurgents' military capacities.

The patterns we find are consistent with a theory of insurgent violence that takes civilian agency into account. In line with a long tradition of theoretical and empirical work (e.g., Berman, Shapiro and Felter forthcoming; Kalyvas 2006; Kitson 1971; Lyall and Wilson 2009), we argue that insurgents' ability to conduct attacks is limited by the degree to which the civilian population supplies valuable information to counterinsurgents. To the extent that collateral damage causes local noncombatants to effectively punish the armed group responsible by sharing less information with that group and more with its antagonist, collateral damage by Coalition forces should increase attacks by insurgents, whereas collateral damage caused by insurgents should decrease attacks. Our data are consistent with this argument and cast doubt on several alternative explanations for the results.

Critically, we find substantial variation across Iraq in the response to collateral damage. The effects are strongest in mixed areas and in areas with a largely urban population. We argue this suggests that the results are in fact driven by the impact of civilian casualties on noncombatants' propensities to share valuable information with counterinsurgents because (a) the population in mixed areas has a more heterogeneous set of political preferences and so there are more people whose behavior can be swayed by civilian casualties, and (b) in predominantly urban areas, there are more noncombatants around to observe insurgents' activities and it is harder for insurgents to wield a credible threat of retribution against informers.

Alternative mechanisms explaining variation in insurgent attacks following civilian casualties as a reaction to popular pressure or as a function of Coalition tactics or unit organization receive little support in the data. If the relationship between how Coalition units patrol and their propensity for causing collateral damage were driving our results, we should have found that the ratio of Coalition-caused casualties to insurgent attacks was greater in urban, high-density areas. If the consequences of engagement by counterinsurgents with the community were driving our results, we should have found that proxies for that engagement—CERP spending and projects initiated per capita—reduced the ratio of Coalition-caused civilian casualties to insurgent attacks and increase the insurgent casualty ratio. We found no evidence on either score, suggesting these alternative mechanisms are not driving the results.

There are at least three broad implications of our analysis. The first is that exploring civilians' strategic incentives is a profitable avenue for better understanding the dynamics of intrastate conflict. Our results strongly suggest civilians are making decisions about whom to cooperate with based on constantly changing information and that these decisions affect traditional variables of interest, such as violence directed against the state and its allies. The bulk of the literature implicitly discounts the possibility of noncombatants strategically exerting a sizable influence, focusing instead on the interaction between insurgents and incumbent forces (Stanton 2009) or between elements of their organization (Weinstein 2007). Our evidence suggests this is a potential limitation.

The second implication is that the dynamics of substate conflict may depend heavily on the military balance in that conflict. Unlike the modal intrastate conflict over the last 60 years, the government side in Iraq has had—with the involvement of the United States—enormous military superiority. The conflict in Iraq is not uncommon in this regard, however. Civil conflicts often showcase such asymmetries; the Mau Mau insurgency in Kenya, the Baloch insurgency in Pakistan, and the Nationalist insurgency in Northern Ireland are but a few examples of insurgents fighting militaries that do not face a challenge in projecting military power into contested territory. In these conflicts the binding constraint on insurgent violence is likely to be an informational one, not the manpower constraint normally presumed by theories designed to explain cross-national patterns of conflict.[54] When insurgent violence is constrained by manpower limitations instead of by the availability of information to counterinsurgents, there may be a very different relationship between collateral damage and subsequent insurgent violence.

Finally, the third implication stems from the core finding of the article: both Coalition forces and insurgents paid for their (mis)handling of civilians, at least in terms of subsequent violence. The argument is often made that even though terrorists or insurgents may not abide by the laws of war or seek to minimize collateral damage, abiding by those rules and taking on added risk is a moral obligation for forces representing liberal democracies. It turns out to be strategically advantageous: such behavior will be attractive to civilians. It also turns out that insurgents' sanguinary tendencies hurt them, at least in this case, where information is a key constraint on the production

[54] For the best review of models of intrastate conflict, see Blattman and Miguel (2010).

of violence.[55] In light of our results, it is no surprise that the September 2009 iteration of the Afghan Taliban "Book of Rules" includes the dictate that "The utmost steps must be taken to avoid civilian human loss in Martyrdom operations."[56] Actions that make it harder for insurgents to precisely target government forces present insurgents with a hard trade-off between accepting greater risks to their forces and triggering adverse civilian reactions and may therefore deter insurgents concerned with popular perception.

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[55]In Afghanistan, for example, where the production of insurgent violence seems to be more constrained by labor supply than by information sharing, and where the insurgency is much more rural than urban, the evidence suggests that civilians either do not or cannot punish insurgents for collateral damage to the degree exhibited in Iraq. See Condra et al. (2011).

[56]See translation at http://www.nefafoundation.org/miscellaneous/nefa˙talibancodeconduct.pdf.

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Supporting Information

Additional supporting evidence may be found in the online version of this article and provides additional information and a series of robustness checks as follows:

1A & 1B: Shows that measurement error in IBC-based civilian casualty data is unlikely to be nonrandom with respect to levels of insurgent violence.

1C: Provides descriptive statistics for the full country and Sunni, mixed, and Shiite areas.

2A: Shows core results are robust to controlling for preexisting trends in attacks and district FE to pick up predictable heterogeneity in trends.

2B: Shows core results robust to dropping Baghdad.

2C: Shows placebo test on core results.

2D: Shows results of trying to predict civilian casualties with leads of SIGACTs.

2E: Shows core results are not present if difference between lagged attacks and average over t to $t+3$ is placed on LHS.

2F: Shows core results are stronger in areas with more than the median proportion of their population (48.5%) living in urban areas.

2G: Shows core results for different kinds of insurgent attacks.

2H: Shows core results on insurgent killings are robust to population-weighting districts. Coalition results become statistically weaker.

2I: Shows core results on insurgent killings are robust to using the log of casualties on the RHS. Coalition results become statistically weaker.

2J: Shows core results in the full regression (column 5) are robust to including the count of incidents by each party on the RHS.

2K: Shows core results in the full regression (column 5) are robust to allowing a mean shift for district-weeks in which civilians are killed.

2L: Shows core results on insurgent killings are robust to including spatial lag of incidents on the RHS. Coalition results become statistically weaker.

2M: Shows core results are robust to allowing mean shift for any week that includes the first day of the month (to which we attribute killings identified through morgue reports).

2N: Shows core results on Coalition killings are robust to dropping the 7.6% of incidents involving both Coalition and insurgent killings. Insurgent results become statistically weaker.

2O: Shows difference between Sunni and mixed districts in Table 4 is robust to dropping the 7.6% of incidents involving both Coalition and insurgent killings.

2P: Shows core results with Coalition and insurgent killings per 100,000 on RHS.

3: Shows the impact of population density and urbanity on civilian casualty ratios.

4: Shows the impact of CERP projects and spending on civilian casualty ratios.

5: Shows effects of a one-SD increase in civilian casualties on rate of insurgent attacks in different periods.

Figure 5A shows an alternative matching solution to that described in the text.

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