Aerial Bombing and Counterinsurgency in the Vietnam War

Matthew Adam Kocher Yale University
Thomas B. Pepinsky Cornell University
Stathis N. Kalyvas Yale University

Aerial bombardment has been an important component of counterinsurgency practice since shortly after it became a viable military technology in the early twentieth century. Due to the nature of insurgency, bombing frequently occurs in and around settled areas, and consequently it tends to generate many civilian casualties. However, the effectiveness of bombing civilian areas as a military tactic remains disputed. Using data disaggregated to the level of the smallest population unit and measured at multiple points in time, this article examines the effect of aerial bombardment on the pattern of local control in the Vietnam War. A variety of estimation methods, including instrumental variables and genetic matching, show that bombing civilians systematically shifted control in favor of the Viet Cong insurgents.

In her reflections on violence, Hannah Arendt famously remarked that "violence can destroy power; it is utterly incapable of creating it" (1970, 56). This is a powerful insight which clashes, however, with an equally powerful idea: that violence and repression "work" to generate compliance. This article analyzes uniquely detailed local data from the Vietnam War to adjudicate between these contradictory insights. Specifically, it examines how aerial bombing against civilian targets affected the Viet Cong (VC) insurgency's ability to control the population of South Vietnam.

A military strategy that depends heavily upon popular mobilization, insurgency makes the control of civilians a central objective. Consequently, military engagements frequently occur in populated areas, leading to "collateral" civilian casualties. Insurgents' failure to clearly identify themselves as combatants makes it difficult for counterinsurgents to discriminate in the use of lethal violence. For these reasons, counterinsurgency tends to kill many civilians. Valentino, Huth, and Balch-Lindsay (2004) show that the mass killing of civilians has been significantly more common since World War II in guerrilla wars than in wars fought by conventional means.[1]

Aerial bombing has been an important counterinsurgency practice since shortly after it became a viable military technology. Bombing was used by the British to suppress Kurdish uprisings in Iraq following World War I (McDowall 1996) and against rebel tribes in Waziristan during the 1920s (Peck 1928). Within the last decade, bombing has been used extensively against insurgents by the United States in Iraq and Afghanistan; by Israel in the Gaza Strip and Lebanon; by Russia in Chechnya; and most recently by Pakistan in its anti-Taliban campaign in the Swat Valley. The U.S. military continues to stress the vital role of aerial operations as a component of counterinsurgency (Department of the Army 2007, E1).

The global prevalence of counterinsurgent violence against civilians points to its utility. Yet the conventional wisdom among contemporary scholars says that victimizing civilians is futile, at best, or even counterproductive. Some practitioners have embraced this logic as well; for

Matthew Adam Kocher is a Lecturer in the Department of Political Science and The Jackson Institute, Yale University, P.O. Box 208301, New Haven, CT 06520-8301 (matthew.kocher@yale.edu). Thomas B. Pepinsky is Assistant Professor of Government, Cornell University, 322 White Hall, Ithaca, NY 14853 (pepinsky@cornell.edu). Stathis N. Kalyvas is the Arnold Wolfers Professor of Political Science, Yale University, P.O. Box 208301, New Haven, CT 06520-8301 (stathis.kalyvas@yale.edu).

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[1] A replication of this finding may be found as Table 8 of Appendix D in the Supporting Information. Data for the replication of all results presented in this article and the Supporting Information may be found at http://courses.cit.cornell.edu/tp253/research.html.

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instance, General Stanley McChrystal made the reduction of civilian casualties a major focus of his counterinsurgency strategy as commander of the ISAF in Afghanistan (Hastings 2010). This apparent consensus is belied by important disagreements at the level of concepts and mechanisms. Studies that directly analyze the consequences of targeting civilians in counterinsurgency warfare either dispute the conventional wisdom or introduce important caveats. Downes (2008, 37–39) points to several cases, including multiple counterinsurgency campaigns, in which civilians were victimized in war by the winning side. Kalyvas (2006) maintains that violence against civilians is an effective means to consolidate control in civil wars *if* it is used selectively and only against suspected collaborators; indiscriminate violence is generally counterproductive, but not always. Stoll (1993) argues that brutal reprisals by the Guatemalan military against entire peasant communities suspected of harboring or sympathizing with the communist EGP were an effective counterinsurgency method. In the most careful quantitative study of civilian targeting to date, Lyall (2009) finds that quasi-random shelling by Russian forces in Chechnya effectively suppressed rebel attacks.

Even among analysts who embrace the conventional wisdom, there is a significant indeterminacy about when and how targeting civilians is thought to become counterproductive. One argument holds that killing an enemy's civilians toughens the resistance of their entire society (Pape 1996). Abrahms (2006) provides a psychological microfoundation for civilian resistance in fear, arguing that the intentional targeting of civilians is interpreted by its targets as an attempt to destroy their entire society rather than to extract specific concessions. A related argument says that targeting civilians provokes retaliation in kind that obviates the military value of the original action (Carr 2002). Both resistance and retaliation may be intensified by the solidarity of nationalism (Arreguín-Toft 2001). These arguments imply that killing civilians may have a positive military value in the short run, but that it is offset by macroscale processes that react back on the perpetrators over indeterminate time frames.

Empirically, much of what we know is subject to problems of aggregation. Common proxies for the utility of victimizing civilians, such as victory or defeat, war duration, or costs, depend on many factors. Victimization of civilians by the state may damage insurgent movements that emerge victorious in the long run, or it may push civilians to ally with insurgents who lose for other reasons. Most worrisome, states or insurgents may opt to target civilians precisely when escalating costs, lengthening war duration, or the course of events begin to put victory in doubt.[2] Reviewing a vast body of descriptive literature on civil wars, Kalyvas (2006, 146–72) concludes that the indiscriminate targeting of civilians is usually counterproductive, but he admits this conclusion "is not based on systematic research" (167).

This article complements the literature on civilian victimization in counterinsurgency warfare, focusing on a particular type of military technology, aerial bombing, and an exact metric of "success," whether it increases the ability of insurgents to control populated places. Specifically, we study the immediate effects of bombing against civilians on the ability of insurgents to control very small local population units over a short time frame. To do this, we assemble a new dataset on bombing and control during the Vietnam War. To measure the relative success of the insurgent and counterinsurgent forces, we use detailed microdata on the control of populated places by either forces loyal to the Republic of Vietnam (RVN; sometimes referred to here as "the incumbents") or the Viet Cong ("the insurgents"). We examine how patterns of insurgent control responded to bombing, which the incumbents used extensively in heavily populated areas amidst a bitter struggle for population control.

Among studies of violence, our data are unparalleled in their coverage. We analyze over 10,000 hamlets, covering the entire territory of the RVN, and we have repeated measures of control along with a rich set of important control variables. Moreover, our data on bombing are remarkably precise. We know the location of virtually every payload of munitions dropped over the RVN between 1965 and 1975. The data show that both bombing and control varied spatially and temporally throughout the war. The combination of scale, variation, and unsurpassed coverage makes Vietnam an ideal case to test whether targeting civilians helps or hinders insurgents' ability to control the civilian population.

The results consistently show that bombing was counterproductive as a counterinsurgency practice. Higher frequencies of bombing correspond unambiguously to higher levels of downstream control by the Viet Cong. These findings hold up across a range of models and specifications designed specifically to address problems of unobserved heterogeneity and endogeneity.

Our findings are the most rigorous evidence available that the victimization of civilians undermines

[2] Downes (2006, 2008) shows that precisely such attrition dynamics drive states' decisions to victimize civilians in interstate wars. Similarly, Valentino, Huth, and Balch-Lindsay (2004) show that mass killing in guerrilla wars becomes more prevalent as the military threat to the state grows. The difficulties of identifying causal effects in this context imply that it is unrealistic to assume military actors can accurately predict the consequences of targeting civilians.

counterinsurgency efforts. They demonstrate just how much the success of counterinsurgency depends on the methods with which it is fought; tactics that run a high risk of victimizing civilians are likely to rebound against their users. Our findings also contribute a new within-country focus to the cross-national and macroscale literature on the use of strategic bombing to achieve political goals (Allen 2007; Horowitz and Reiter 2001; Pape 1996). The local effects of bombing are poorly understood in this literature, yet they play a central role in the literature's causal claims. Methodologically, our article illustrates the utility—and the challenges—of within-country research strategies in studying violence in civil wars (Kalyvas 2006; Sambanis 2004). Case studies alone are insufficient, for insurgents and incumbents alike respond to changing conditions, and strategies of violence are both the product of previous developments and the causes of subsequent ones. Untangling the direction of causality is no easy task, but we show that careful attention to the logic of inference yields powerful findings.

We begin by discussing the microfoundations of civilian targeting in the context of insurgency and the theoretical debate about its effects. We then provide historical context for our analysis and present our data. We analyze the effects of bombing using several approaches, and we conclude.

Insurgency and Indiscriminate Violence

Most wars generate civilian casualties. Insurgency, however, is a technology of rebellion that implicates civilians more directly in the process of warfare than conventional forms of conflict (Kalyvas and Balcells 2010). The key descriptive feature of insurgency is asymmetry: the state fields large, relatively well-equipped, regular military forces against a smaller force of less well-equipped rebels organized as irregulars. The rebels avoid large-scale and sustained confrontations due to the state's material advantages. Consequently, battles, especially large or decisive ones, are rare; front lines do not form. Insurgents attempt to organize the civilian population, mainly in the countryside, as a means to gradually build up forces sufficient to take over the state or to detach a portion of its territory. Given their inability to draw insurgents into sustained combat, the state must compete with insurgents for control over the population. Active collaboration by civilians is a vital resource for both insurgents and state forces. The behavior of civilians is highly constrained in insurgencies, but they often have substantial scope for choice as to the extent of their collaboration with the dominant local actor (Weinstein 2006; Wood 2003).

One way to influence civilian behavior is through violence, which sets insurgency apart from normal politics and constitutes it as a form of warfare. Violence against civilians is a central feature of such conflicts. It is generally assumed that civilians will try to avoid being victimized, but this insight alone entails indeterminate predictions. Violence may plausibly terrorize civilians into compliance, or it may push them to align with the insurgency. To overcome this indeterminacy, Kalyvas (2006, 141–45) distinguishes between "selective" violence, in which targets are chosen on the basis of *individualized* suspicion, and "indiscriminate" violence, in which targeting is based on collective attributes (including non-ascriptive characteristics like place of residence). Both selective and indiscriminate violence target civilians, but they do so in different ways.[3] Military practices that cannot effectively discriminate among individual victims (such as Vietnam-era bombing) are indiscriminate according to this definition because they target areas where civilians are known to reside.[4]

When violence against insurgents is selective, individuals can mitigate their odds of becoming victims by minimizing participation in the insurgency; selective violence exacerbates the "rebel's dilemma" (Lichbach 1995) by creating effective deterrence. By contrast, indiscriminate violence tends to generate a perverse structure of incentives, leading to a failure of deterrence. When violence is indiscriminate, targeting whole collectivities, individuals cannot avoid being victimized simply by refusing to participate in the insurgency. Because it approximately equalizes the probability of victimization for participants and nonparticipants, indiscriminate violence increases participation in insurgencies by raising its payoff vis-à-vis nonparticipation. Even when counterinsurgent actors want to use violence with discrimination, perverse selection mechanisms can generate a high proportion of innocent victims; noncombatants may be victimized disproportionately because they are easier than insurgents to find and kill (Kalyvas and Kocher 2007). If insurgents

[3] In what follows, we complicate this distinction by discussing the discriminate targeting of collectives (e.g., attacking some communities but not others). To avoid confusion, and to maintain consistency with an evolving research program, "selective" and "indiscriminate" are used here in the precise sense outlined by Kalyvas, such that *any* targeting of collectives (no matter how small) is termed "indiscriminate."

[4] Downes (2008, 15–16) makes a similar move in treating intentional targeting and the failure to discriminate as instances of "civilian victimization."

are more likely to survive than are civilians, indiscriminate violence may endogenously reinforce participation in insurgencies (Goodwin 2001; Kalyvas and Kocher 2007; Mason and Krane 1989). According to this logic, indiscriminate violence is counterproductive.[5]

Note, however, that collective targets of indiscriminate violence may be carefully selected from a still larger population (Steele 2009, 422–23). Individual neighborhoods, villages, political parties, unions, or tribes whose members are, on average, more likely to collaborate with one side may be targeted collectively in a process of "profiling." This method of targeting—selective at the collective level, but indiscriminate at the individual level—is likewise intended to deter collaboration with opposing forces by altering its expected payoff. Lyall argues that such collective targeting "reshapes the relationship between insurgents and populace by underscoring that the insurgency cannot credibly protect the population and, moreover, that its continued presence endangers noncombatants" (2009, 10), potentially leading to locally organized collective action against the insurgents or collective defection to the state (see Stoll 1993 for a similar argument about the civil war in Guatemala). This line of argument recapitulates the logic used to rationalize area bombing in interstate warfare since Giulio Douhet (1942, 58; see Pape 1996 for a discussion and critique). The critical distinction is that individuals cannot defect alone: in order reliably to protect themselves, they must overcome barriers to collective action in order to defeat the rebels who dominate their community or they require channels of influence that can be used to persuade insurgents to change tactics. If these requirements are met, indiscriminate violence may be an effective counterinsurgent method. Differences in the capacity for local collective action vis-à-vis insurgents may help to account for the divergence of findings between our study and Lyall's (2009) comparable and extremely careful microcomparative analysis of indiscriminate shelling by artillery in Chechnya, which he finds suppressed attacks by Chechen insurgents. The decentralized and highly localized character of the Chechen insurgency may have made it more easily subject to community pressure or less capable of resisting locally organized resistance. The VC, organized along party-hierarchical lines and able to call upon formidable translocal sources of support, would have been tougher to challenge or influence.

[5] This general process may be reinforced by (1) the perception of arbitrariness in the application of violence; (2) powerful emotional reactions among victims or observers that lead to identification with the insurgents' cause; or (3) the active use of protection as a selective incentive by the insurgents. See Kalyvas (2006, 151–60) for an extensive discussion of these issues.

The tension between these two competing logics can only be resolved empirically. Yet evaluating the effects of violence empirically is daunting, given that both of the mechanisms we identify may be at work simultaneously or sequentially within a single conflict. Complicating the picture even further, both state forces and insurgents often combine attacks against civilians with other tactics. Aggregating over long time periods (the duration of wars, for instance), or large geographical or political units, will in general make it difficult to disentangle the range of causal processes that may be at work in a given conflict.

Our approach has two major advantages that complement existing studies. First, we systematically examine the local effects of indiscriminate violence on very small population units that were continuously measured over a relatively short duration (six months). Second, we measure local control, which is the immediate objective of rebel and state forces, as our dependent variable, rather than insurgent violence. Low levels of insurgent violence in a given area are consistent with both complete insurgent control and complete incumbent control (in the former case, because rebels have no need for it; in the latter case, because their access is limited). High levels of insurgent violence may indicate an insurgency that is gaining or losing strength. Our use of control as the dependent variable is a second plausible reason why our results diverge from those of Lyall (2009), who instead examines the effect of indiscriminate violence on subsequent insurgent attacks. We believe our variable is more appropriate to the task at hand. The exercise of territorial control over a community is a much more precise way of assessing insurgents' strength than their ability to launch attacks in the vicinity of that community.

Bombing and Its Context in South Vietnam

Although the war in South Vietnam was fought in a variety of ways, at its core it was a classic insurgency. It pitted the forces of the RVN and the United States, with several allies, against a large insurgent organization (the Viet Cong) supported directly by North Vietnam. The war featured dramatic asymmetries of firepower. Massive U.S. bombing throughout Indochina was a hallmark of the conflict.

The air war in South Vietnam is often ignored by scholars, perhaps owing to the greater political emphasis on strategic bombing of the North under the Johnson and Nixon administrations, even though "[m]ost of these [air] operations were directed at targets within South Vietnam

and along the Ho Chi Minh Trail in Laos, not, as commonly believed, at North Vietnam" (Thayer 1985, 79). According to Littauer and Uphoff's (1972) wartime estimates, 62% of the bomb tonnage dropped in Indochina from 1965 to 1971 fell on South Vietnam. Schlight (1994) estimates that 75% of all aerial missions in Indochina were flown over South Vietnam.

The effects of bombing on the population of Vietnam and the intentions behind it were controversial subjects during the war, and they remain key objects of historical polemics to this day. A simple account is elusive. As with much of the U.S. approach to counterinsurgency in Vietnam, practices varied among commanders and evolved over time. Despite the lack of a systematic Air Force counterinsurgency doctrine until several years into the conflict (Drew 1998), the use of air power in Vietnam was lavish: "air forces" accounted for as much as 47% of the combined annual military budget of the United States and South Vietnam in 1969 (Thayer 1985, 25).

Several histories of the war argue that the bombing of civilians was a systematic and intentional component of U.S. strategy, designed to terrorize civilians out of collaboration with the VC (Fitzgerald 1989; Gibson 1986). Since the war, no direct evidence has come to light that supports this conclusion for South Vietnam as a whole, though some units may have practiced terror bombing unofficially. Some fairly direct evidence supports the latter conclusion, such as the text of a leaflet that was dropped in some areas of the South: "When the plane returns to sow death, you will have no more time to choose. Be sure to follow the example of 70,000 compatriots who have used the free-movement pass to return and re-establish a comfortable life in peace; or stay and die in suffering and horrible danger. All who stay will never be able to know when other bombs will fall. Be sure to be wise and don't be undecided any more. . ." (Littauer and Uphoff 1972, 60).

Anecdotal evidence suggests that many commanders welcomed the bombing of civilians even if they could not directly order it. Writes R. W. Apple, "An army general explained the idea to me as follows: 'You've got to dry up the sea the guerrillas swim in—that's the peasants and the best way to do that is to blast the hell out of their villages so they'll come into our refugee camps. No villages, no guerrillas: simple'" (1971, 449). General William Westmoreland, as head of U.S. operations in Vietnam, was famously unconcerned with civilian casualties, remarking that the air war "does deprive the enemy of population, doesn't it?" (Halberstam 1972, 550).

U.S. Rules of Engagement (ROEs) issued during the war are consistent with the view that civilians were frequently bombed in spite of the absence of a specific command-level intent to systematically kill or terrorize them. According to a summary of the ROEs prepared for congressional testimony, all U.S. airstrikes required the approval of the local Vietnamese province chief or a higher official. In practice, multiple sources confirm that approval was easy to obtain. Airstrikes close to populated hamlets had to be directed by a Forward Air Controller or an alternative observer. Unless carried out "in conjunction with an immediate ground operation," the inhabitants had to receive prior warning of the strike. The ROEs were relaxed for "Specified Strikes Zones," the official military jargon for so-called "free-fire zones," places "where no friendly forces or populace existed" (Congressional Record 1985). In short, even direct bombardment of populated places was not prohibited, though it was restricted.

How common was it? One indication is given by Thayer (1985, 130–32), who shows that about 23% of the South Vietnamese population lived within 3 km. of at least one airstrike in January 1969.[6] Although it is not possible to estimate civilian casualties from these figures, they must have been substantial. Race comments on the ROEs, "Despite these rules, however, heavy civilian casualties still occurred" (1972, 233), a conclusion echoed by Elliott (2003). Given the technology of the times, a heavy civilian death toll followed directly from the choice to use air power in populated areas. Lewy writes, "[U]ntil the perfection of the 'smart bomb' during the last stage of the Vietnam War, bombing was an inherently inaccurate process. Despite sophisticated computer equipment, the precision of the bombing was degraded by errors involving boresight, release mechanisms, bomb dispersion, aiming, and the computational system. Unknown winds at altitudes below the release point further complicated the pilots' task. All this meant that, as one Navy pilot wrote in 1969, 'it is impossible to hit a small target with bombs except by sheer luck'" (1978, 404).[7] In his postwar memoir, former Secretary of Defense McNamara (1995, 243) acknowledged that bombing was almost entirely incapable of distinguishing supporters of the Viet Cong from opponents and neutrals.

The available evidence strongly confirms that bombing in Vietnam was indiscriminate: it could not target individual VC supporters while sparing government supporters or the uncommitted, even when intelligence was good. Yet bombing could and did discriminate among

[6] Our data give similar results: about 32% of hamlets had at least one bomb sortie fall within 2 km. between July and December 1969.

[7] Lewy's (1978, 451) careful estimate puts South Vietnamese civilian deaths "from military operations" during the entire 1965–74 period at 300,000.

collectives, frequently striking areas that fell under the control of the Viet Cong, but avoiding areas of government dominance. These characteristics of bombing in South Vietnam make it an ideal laboratory for examining the consequences of indiscriminate violence in a counterinsurgency campaign.

The Data

We assemble our data using two sources collected by the United States as operational tools of warfare. The Hamlet Evaluation System (HES) was developed to provide a clear picture of control for the smallest population units: villages and hamlets.[8] The United States compiled a gazetteer of South Vietnamese hamlets, identified their geographic coordinates, and conducted a census. District Senior Advisors (DSAs), Army officers ranking major or above, were assigned to complete detailed questionnaires, some on a monthly basis, others quarterly, for every village and hamlet in their zones of operation. DSAs, together with small American staffs, were detached from U.S. units to live and work in the districts they rated. The RVN had 261 districts with a median area of 377 kilometers squared, or about one-fourth the size of the median U.S. county. The HES identified a median of 36 hamlets per district in 1969. One might analogize the problem a DSA faced to that of the sheriff of a small U.S. county trying to identify dangerous towns or neighborhoods in his or her jurisdiction. For more information about DSAs, see Bole and Kobata (1975).

The questionnaires were resolved into a series of "level 1 models," ordinal indices that were intended to capture specific dimensions of pacification. Level 1 models were combined into level 2 and higher models, eventually resulting in a single pacification rating that was widely distributed among the military and civilian leadership of the United States and the RVN. A full description of the procedures used to create theses indices, which combined Bayesian updating with a simple additive schema, is included as Appendix B of the Supporting Information. While the use of indices can raise red flags about what exactly is being measured, we believe this system was based on a fundamentally sound rationale: control in an insurgency is not directly observable, because insurgents are always partially clandestine. Recognizing these facts, as well as the difficulties of collecting data under wartime conditions, the designers of the HES opted for a measurement system that could aggregate a great deal of information, smoothing over error in the coding of individual questions.

To capture our dependent variable, local control, we use the "Enemy Military Model (2A)" (*Hamlet Control)*, which rates the presence and activity of Viet Cong military units in the vicinity of each hamlet on a 5-point scale, the categories of which we have designated "fully government controlled" (1), "moderately government controlled" (2), "contested" (3), "moderately insurgent controlled" (4), and "fully insurgent controlled" (5). The texts of all questions used in the construction of this index (Appendix A), along with a qualitative description of each zone (Appendix C), may be found in the Supporting Information. We assume that a continuous and active Viet Cong presence indicates their ability to control a hamlet.[9]

The bombing data are derived from the Combat Air Activities File (CACTA), which recorded U.S. bombing sorties by fixed-wing aircraft from October 1965 through December 1970. A bombing sortie was a composite event that typically included multiple aircraft and weapons.[10] The CACTA was assembled from postflight pilot debriefs conducted on the day of each sortie.

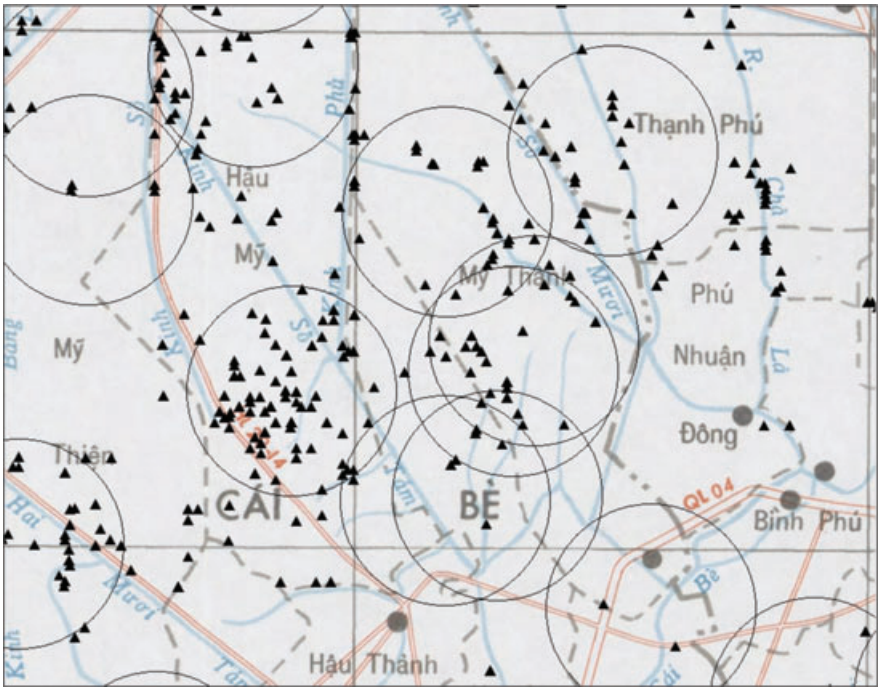
We mapped bombing sorties onto hamlets over the full territory of the country using a GIS. An illustration of the procedure we used is given in Map 1 (the base map is of Cái Bè district, in Định Tường Province). The large circles represent search radii of 2 km. around hamlet centers. The triangles are bombing sorties. We used this procedure to create a variable that counts the number of sorties that attacked within the search radius of each hamlet [*Bombed* (*count*)]; we also created a binary variable indicating whether the hamlet was bombed or not [*Bombed* (*binary*)]. This procedure entails that a single sortie may be treated as "striking" multiple hamlets. Given that our hamlet data do not describe the extension or shape of individual hamlets (all are represented as points), that bombing sorties dropped multiple weapons from multiple aircraft, and that the technology of the time was far from pinpoint accuracy, we believe the 2 km. radius is a reasonable simplification of a highly complex

[8] Vietnamese villages were bounded territorial units that covered the entire land area of the country; hamlets were clusters of dwellings. The data are described in CORDS/RAD (1971).

[9] We repeated the instrumental variables analyses presented in this article on three additional dependent variables: the "Enemy Military Activity (1B)" submodel, which is a component of index 2A, the "Enemy Political (2C)" model, and a single question text (HQB1) that directly rates the "status of the enemy infrastructure" on a 5-point scale. The results are reported in Appendix E, Tables 10–12, of the Supporting Information and strongly support our conclusions.

[10] Over 83% of bombing sorties involved more than one aircraft, while the mean number of weapons dropped per sortie was about 14.

MAP 1 Illustration of the Procedure for Mapping Bombing Sorties to Hamlets. Cái Bè District, Định Tường Province, Republic of South Vietnam, July 1969. Scale Approximately 1:50000



reality.[11] Even sorties that failed to directly strike hamlet dwellings could have had serious material and psychological effects on hamlet residents.

In our analyses, we control for several structural variables that prior work has shown to have been important cross-sectional covariates of insurgent control in Vietnam (see Kocher 2004 for details on the construction of these variables). An indicator of terrain variability was constructed from an altitude grid map by measuring the standard deviation of an eight-cell neighborhood around the cell that contains each hamlet (*Rough terrain*). We control for the log of each hamlet's distance to the closest international border (*Log of distance to international border*). We control for the *Log of hamlet population*, and in addition we use a *Development index* constructed from ordinal variables that measured the relative presence of televisions and motorized vehicles, high-value items that visibly signaled the wealth or poverty of a locality.

We study a full cross-section of South Vietnam from July through December 1969. This choice is driven both by data availability and empirical design rationales. First, the version of the HES that went online in 1967 had a highly subjective and much less sophisticated coding schema than the HES70 system we use. It also collected no information on key independent variables. Second, the data held by the National Archives are fragmentary, missing January 1970–June 1971, as well as 34 of 44 provinces for all of 1972. Third, the six months of 1971 that we do have access to cover a period with far less variation in control (most hamlets had shifted into government control), less intertemporal change in control, and virtually no bombing close to hamlets; in short, it lacks the variation we need for empirical leverage. By contrast, in 1969 control over rural South Vietnam was still sharply divided between the two sides. Counterinsurgency appeared to be succeeding (Davidson 1988, 610–12; Goodman 1970; Sorley 1999, 154–60; Tran 1980, 16–25). June and July of 1969 saw few Viet Cong–initiated attacks, and indeed little direct combat overall (Davidson 1988, 597, 612–17). An "Autumn Campaign" initiated by the Viet Cong in August 1969 met with little success, and captured documents from the North Vietnamese Army's Central Office for South Vietnam (COSVN) reveal frustration with the success of pacification efforts by the RVN (COSVN 1969). Still, bombing of populated areas remained prevalent as allied forces prepared for the drawdown of forces

[11] As a robustness test, we reran the ordered logit and instrumental variables models presented below for search radii of 1.5 km. and 2.5 km. Our findings were confirmed for all models; the effect of bombing was somewhat weaker in substantive terms for a 2.5 km. radius, but quite a bit stronger for a 1.5 km. radius. These results are available upon request to the authors.

occasioned by Nixon's "Vietnamization" policy (FitzGerald 1989, 508–11). Late 1969 is accordingly a propitious time frame to examine the effect of bombing on insurgent control, for this period witnessed (1) a lull in the ground war, (2) apparent counterinsurgency success, and (3) continued high levels of bombing.

The short time frame of our study raises two methodological issues: (1) the possibility that this period is not representative of the Vietnam War as a whole, and (2) the possibility that our findings are an artifact of bombing prior to July 1969. On the first point, Kocher (2010) finds that the entire history of bombing from 1965 strongly predicts insurgent control at the *village* level in the middle of 1971, even holding constant a prior indicator of control. To address the second point, we calculated a count of bombing sorties covering the full history of U.S. bombing from 1965 through June 1969 (the month before our study begins) for each hamlet location in our study. We replicated the ordered logit and instrumental variable models reported below with the addition of bombing history as a statistical control. All of our results were confirmed. For additional details, please contact the authors.

The HES has been the object of intense criticism beginning during the war itself (see, e.g., Elliott 2003; Gibson 1986; Kolko 1985; Race 1972; Thayer 1985). The principal critique is that the data were biased in favor of the United States to justify or rationalize its conduct of the war. Nevertheless, we have several reasons for confidence in our use of the data. First, inflation in the number of hamlets under government control should not affect our results because we examine only variation among cases that were coded in similar ways; we make no inference about absolute levels of control. This variation is substantial: many hamlets were coded as falling under Viet Cong control for months or years; many were regarded as stably controlled by the Vietnamese government; and many shifted among categories, in some cases several times in either direction. In any case, general inflation would tend to bias regression coefficients toward zero, making it less likely we would find any effect. Second, the data on control and the data on bombing were not collected by the same agencies, so it would have been difficult to shape the two datasets to create a misleading picture of their intersection. Finally, our results do not reflect well on the decision makers of the time. Not only did the United States kill many civilians with airstrikes, but it also appears much of this bombing was done needlessly. To intentionally produce our findings, analysts would have had to intend to demonstrate the failure of their own efforts, and then only in the hamlets they targeted for bombing.

The Effects of Bombing

While our data provide precise estimates of both control and violence, estimating the downstream effects of violence on control is complicated by the nonrandom assignment of bombing. Our data (Kalyvas and Kocher 2009) show a strong linear association between insurgent influence and bombing, with fully insurgent-controlled hamlets bombed at about 10 times the frequency of fully incumbent-controlled hamlets and about four times the frequency of the most contested hamlets (see Appendix E of the Supporting Information, Table 9). Map 2 in the Supporting Information shows a similar pattern through a visual analysis of Định Tường province. This map plots bombing sorties, as well as hamlets by their control status in July 1969. We can see from the map that bombing was directed overwhelmingly at areas with clusters of (red) fully insurgent-controlled hamlets. Contested (light green) and moderately insurgent-controlled (pink) hamlets generally did not have bombing in their vicinity, though some are adjacent to areas of heavy bombardment. How, then, do we distinguish between locales that were bombed because they were controlled by the insurgents and locales that became insurgent controlled because they were bombed?

Table 1 offers a first cut on the consequences of bombing, using lags in a contingency table. The five major columns represent control categories in July 1969. Within each column, bombing in September is cross-tabulated with control status in the same hamlets three months later, in December 1969, thereby examining the downstream consequences of bombing while holding constant a prior measurement of control.[12] Two cells within each major column are boldfaced; the observations in these cells have the same control value in December as in July. The diagonal formed by these boldfaced cells shows that a majority (about 57%) of Vietnamese hamlets fell into the same control category in December 1969 that they occupied six months earlier.

In all five of the major columns, hamlets bombed in September were more likely to move toward insurgent control, and less likely to move toward incumbent control, by December than hamlets that were not bombed. Consider, for instance, hamlets that were under "moderate insurgent control" in July 1969. Within this group, nearly 50% of the unbombed hamlets were rated contested or under moderate/high government control six

[12] We use September as the month for bombing to maintain consistency with our parametric analyses. The contingency table results are similar for bombing measured in July and August (somewhat weaker for July, somewhat stronger for August).

TABLE 1 The Effects of Bombing in September on Control in December 1969

	Hamlet Control July 1969									
	Full Government Control		Moderate Government Control		Contested		Moderate Insurgent Control		Full Insurgent Control	
	Bombed?		Bombed?		Bombed?		Bombed?		Bombed?	
Hamlet Control December 1969	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**
Full Government	**68.80%**	**40.00%**	11.72%	0.74%	1.72%	1.20%	0.20%	0.00%	0.00%	0.00%
Control	**(752)**	**(4)**	(320)	(1)	(40)	(3)	(4)	(0)	(0)	(0)
Moderate Government	23.70%	50.00%	**63.00%**	**63.70%**	27.40%	21.69%	7.71%	6.10%	4.52%	1.38%
Control	(259)	(5)	**(1,720)**	**(86)**	(798)	(54)	(156)	(13)	(19)	(4)
Contested	6.86%	10.00%	22.09%	27.41%	**53.61%**	**55.82%**	42.14%	32.86%	11.19%	6.23%
	(75)	(1)	(603)	(37)	**(1,561)**	**(139)**	(853)	(70)	(47)	(18)
Moderate Insurgent	0.64%	0.00%	3.15%	8.15%	16.59%	20.88%	**47.38%**	**56.81%**	33.57%	18.69%
Control	(7)	(0)	(86)	(11)	(483)	(52)	**(959)**	**(121)**	(141)	(54)
Full Insurgent	0.00%	0.00%	0.04%	0.00%	0.69%	0.40%	2.57%	4.23%	**50.71%**	**73.70%**
Control	(0)	(0)	(1)	(0)	(20)	(1)	(52)	(9)	**(213)**	**(213)**
Total	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%
	(1,093)	(10)	(2,730)	(135)	(2,912)	(249)	(2,024)	(213)	(420)	(289)
Fisher's exact test probability	$= 0.141$		< 0.001		$= 0.203$		$= 0.032$		< 0.001	

"Bombed" = At least one fixed-wing air attack sortie targeted coordinates within a 2,000-meter radius of hamlet center during this time period.

TABLE 2 Ordered Logit on Control Status in December 1969

	Model 2A	Model 2B	Model 2C	Model 2D
Moderate Government Control (July)	2.655*	2.098*	2.737*	2.056*
	(0.082)	(0.087)	(0.083)	(0.089)
Contested (July)	4.550*	3.555*	4.733*	3.394*
	(0.090)	(0.101)	(0.091)	(0.104)
Moderate Insurgent Control (July)	6.063*	4.738*	6.305*	4.509*
	(0.097)	(0.115)	(0.100)	(0.118)
Full Insurgent Control (July)	9.195*	7.655*	9.417*	7.093*
	(0.141)	(0.158)	(0.143)	(0.167)
District average control (July)	–	0.779*	–	0.842*
		(0.038)		(0.039)
Δ District average control (July–Sept.)			0.590*	
			(0.041)	
Bombed (count, September)	0.066*	0.067*	0.066*	0.061*
	(0.012)	(0.012)	(0.012)	(0.012)
Development Index	–	–	–	−0.168*
				(0.020)
Log of Distance to International Border	–	–	–	0.029
				(0.025)
Rough Terrain	–	–	–	0.005
				(0.002)
Log of Hamlet Population	–	–	–	−0.095*
				(0.024)
N	10075	10075	10075	9707
Log-likelihood	−10293.4	−10077.3	−10186.0	−9622.8
Pseudo-R^2	0.291	0.306	0.299	0.302

All cutpoints are significant at the $p < 0.001$ level. $* = p < .001$. Two-tailed tests. Standard errors in parentheses.

months later, while about 39% of the bombed hamlets were similarly rated by December. Among hamlets that were strongly controlled by the insurgents in July, nearly 74% of the bombed hamlets remained in the same category, as compared with about 51% of the unbombed hamlets.

This evidence establishes that bombing is associated with a downstream failure of counterinsurgency, but it is possible that rather than causing the resulting distribution of control, bombing instead acts as an indicator for unobserved sources of insurgent strength or incumbent weakness in July, which led to gains in control (or a reduction of losses) for the insurgents down the road. Put differently, bombing may signal that the incumbents were in a tough fight, one that, on average, they were less likely to win.

One way to check this possibility is to hold constant not only the prior level of control for each hamlet, but also the prior level of control in regions around each hamlet. The rationale is that the propensity of a hamlet to change its control status in the future depends in part on the relative strength of insurgents or incumbents nearby. Our assumption is that the level of a party's regional strength should be reflected in its control over nearby hamlets. If both bombing in July and control in December are a function of regional strength that is not yet reflected in local control, then holding regional strength constant should weaken or eliminate the apparent association between bombing and downstream control.

To simultaneously hold constant regional strength and local control, we use the average of the control variable within districts in July (*District average control*). We also examine the effects of the *difference* in district average control from July to September as a dynamic check on the direction of movement in control status in the period prior to bombing. The first column of Table 2 (Model 2A) recapitulates the results of the contingency table analysis in the form of an ordered logit; in this case, bombing is measured as a count of sorties. Positive coefficients indicate increased insurgent control. As expected, bombing in September is a statistically significant predictor of control status in December, holding constant the previous level of

TABLE 3 Bombing and Expected Changes in December 1969 Control Probabilities

	Hamlet Control in July 1969				
Hamlet Control in December 1969	**Full Government Control**	**Moderate Government Control**	**Contested**	**Moderate Insurgent Control**	**Full Insurgent Control**
Full Government Control	**−0.130**	**−0.031**	**−0.008**	**−0.002**	**−0.0002**
Moderate Government Control	**0.089**	**−0.109**	**−0.102**	**−0.047**	**−0.005**
Contested	**0.038**	**0.113**	**0.022**	**−0.092**	**−0.048**
Moderate Insurgent Control	**0.003**	**0.027**	**0.083**	**0.125**	**−0.063**
Full Insurgent Control	**0.000**	**0.001**	**0.005**	**0.017**	**0.115**

Cells contain the estimated change in the expected probability of being in a specified control level in December, by initial control level in July, as bombing sortie count changes from 0 to 10: $\Pr(Y = j \mid \text{bombed} = 10, \text{control} = k) - \Pr(Y = j \mid \text{bombed} = 0, \text{control} = k)$, where j indexes control in December, k indexes control in July. Estimates whose 95% confidence interval excludes zero are bold. Negative changes in probability are shaded. All other variables are held at their means. The estimates are from Model 2D.

control. The second column (Model 2B) introduces the district-level average control status in July, which is an extremely powerful predictor of control six months later, confirming our intuition that regional control reflects the capacity to influence local control over time. Model 2C substitutes the change in district average control between July and September, which also has a powerful association with downstream control. Model 2D introduces several structural control variables. Development level, rough terrain, and hamlet size are also statistically significant predictors of control status. The coefficient for bombing remains highly and statistically significant in all models.

Table 3, generated using Tomz, Wittenberg, and King (2003), gives the predicted differences in the probability that a hamlet bombed 10 times versus one not bombed at all fell into each one of the control categories in December, conditional on its control status in July (the probabilities are derived from Model 2D). Negative differences are shaded, and estimates that are statistically significant at the 95% confidence level are bold. The overall pattern is consistent with bombing having decreased the RVN's ability to control hamlets. The negative signs on the first differences on the upper right-hand side of the matrix indicate that bombed hamlets that were previously under government control were more likely to move toward insurgent control than were unbombed hamlets, while the positive signs on the lower left-hand side of the matrix show that bombed hamlets that were previously under insurgent control were more likely than unbombed hamlets to remain under insurgent control six months later. The cells on the diagonal reflect the difference between bombed and unbombed hamlets in their rate of stasis. If bombing damages counterinsurgent prospects, the incumbents should have a harder time holding on to bombed than unbombed hamlets, while the insurgents would have exactly the opposite experience. We find precisely this pattern: the upper two elements of the diagonal are negative, while the lower two elements have a positive sign.

Testing for Causality: Instrumental Variables and Genetic Matching

Despite the strength of our evidence, we must still worry that our initial findings reflect more complex dynamics. It is possible that unobserved factors both increased the frequency with which some hamlets were bombed and facilitated VC control in those same hamlets. Likewise, it is possible that bombing increased in places where the incumbents anticipated insurgent offensives. In either case, it would be incorrect to attribute shifts in control to the bombing itself.

We employ two techniques to tackle these problems: instrumental variables and matching. Both confront the inferential problems that arise when observational data are used to derive causal inferences, but in different ways. Consistent results using both methods will indicate that neither endogeneity nor unobserved variables drive our results.

Strategy One: Instrumental Variables

Our first strategy exploits the temporal dimension of our data to untangle the direction of causality between bombing and control. We use past values of insurgent control as instruments for bombing, and then study the effect of

bombing on downstream insurgent control. Since we have monthly data on control from July to December 1969, we use insurgent control in July and August as instruments for bombing in September, and we investigate the effect of bombing in September on insurgent control in December. Using lags of dependent variables as instruments for endogenous covariates is common in labor and housing economics (Poterba 1991). Our methodology is roughly akin to a generalized method-of-moments (GMM) estimator (Hansen 1982), although our model is a simple cross-section.

To be valid, our instruments must satisfy two requirements: excludability and relevance. The instruments are excludable if they are conditionally independent from the error term in the (unobserved) true regression. In our application, this requires that there be no unobserved relationship between insurgent control in July and August (the instruments) and insurgent control in December (the outcome). While there are certainly unobserved hamlet characteristics that jointly determine insurgent control in these two months, we can use data on insurgent control in September as a control variable to recover conditional independence. Because instrumental variables require only *conditional* independence between instruments and the error term, we need only assume that there are no unobserved hamlet-specific variables that affected insurgent control in July, August, and December 1969, but *not* in September of that year as well. Moreover, since we have two instruments and one endogenous variable, we are able to test the model's overidentifying restrictions, and we do so below.

Second, to be relevant, our instruments must actually explain our endogenous variable. We have argued above that this is likely to be the case, but we show below in a number of diagnostic tests that insurgent control in July and August are highly relevant instruments for bombing in September. Formally, if the excludability and relevance conditions are met, then the instrumental variable estimator is a consistent estimator of the effect of bombing on insurgent control. Crucially, instrumental variables regression does not require a full model of the process by which hamlets were selected for bombing.

We begin by examining the first-stage relationship between our instruments and the endogenous variable, bombing. Table 4 contains the results from two models, the first a baseline model with no substantive controls, and the second including a series of control variables. The results indicate that past insurgent control is a good instrument for bombing in September 1969. All four of the coefficients measuring this relationship are significant at the $p < .01$ level. Shea's partial R^2, a common method to assess the explanatory power of the instruments, is low but sufficiently large given the sample size: *F* tests show the instruments are highly jointly significant.[13]

Table 4 First-Stage IV Results

	MODEL 4A	MODEL 4B
INSTRUMENTS		
Hamlet Control (July)	0.190*	0.168*
	(0.035)	(0.034)
Hamlet Control (August)	0.155*	0.133*
	(0.037)	(0.036)
CONTROLS		
Hamlet Control (September)	0.068	0.071
	(0.029)	(0.029)
Development Index	–	−0.154*
		(0.038)
Log of Distance to International Border	–	−0.123*
		(0.023)
Rough Terrain	–	0.013*
		(0.002)
Log of Hamlet Population	–	0.014
		(0.023)
Constant	−0.819*	0.480
	(0.059)	(0.293)
Shea's Partial R^2	0.012*	0.009*

* = $p < .001$. Two-tailed tests. Standard errors in parentheses.

Table 5 presents the second-stage results from seven instrumental variables regressions. The first two models (Models 5A and 5B) are the second stages of the models in Table 4; the next three models include a binary indicator for bombing, dummy out the measure of insurgent control in September, and include an additional control for the average level of insurgent control at the district level. The sixth and seventh models (5F and 5G) investigate the effect of bombing on *changes* in insurgent control between September and December 1969. This is the strongest possible test of the argument that bombing changes the degree of insurgent control. In Model 5G, we make an additional change. We use the first differences in control from July to August and from August to September as our instruments, instead of the levels of control in these months, and we hold constant changes in control from September to October in the second stage. If bombing were systematically directed against hamlets in the process of moving toward insurgent control, then bombing would reflect the anticipation of changes in control rather than causing them. We take this possibility explicitly into account in Model 5G.

[13] In Model 4A, $F(2, 10071) = 59.40$. In Model 4B, $F(2, 9699) = 44.17$.

TABLE 5 Second-Stage IV Results

	MODEL 5A	MODEL 5B	MODEL 5C	MODEL 5D	MODEL 5E	MODEL 5F	MODEL 5G
Dependent Variable	***Control in December***	***Control in December***	***Control in December***	***Control in December***	***Control in December***	***Change in Control***	***Change in Control***
Bombed (count)	1.369*	1.464*	–	1.330*	1.440*	1.440*	1.248*
	(0.127)	(0.158)		(0.133)	(0.154)	(0.154)	(0.209)
Bombed (binary)	–	–	9.239*	–	–	–	–
			(0.877)				
Hamlet Control (September)	0.264*	0.250*	0.238*	–	0.164	−0.836*	−0.641*
	(0.048)	(0.052)	(0.047)		(0.065)	(0.065)	(0.081)
Δ Control (September to October)	–	–	–	–	–	–	0.159
							(0.070)
Moderate Government Control (September)	–	–	–	0.753*	–	–	–
				(0.096)			
Contested (September)	–	–	–	1.401*	–	–	–
				(0.095)			
Moderate Insurgent Control (September)	–	–	–	1.618*	–	–	–
				(0.114)			
Full Insurgent Control (September)	–	–	–	0.642*	–	–	–
				(0.243)			
Development Index	–	0.135	0.133	0.139	0.120	0.120	0.106
		(0.066)	(0.058)	(0.060)	(0.064)	(0.064)	(0.062)
Log of Distance to International Border	–	0.179*	0.073	0.154*	0.165*	0.165*	0.152*
		(0.039)	(0.031)	(0.034)	(0.038)	(0.038)	(0.038)
Rough Terrain	–	−0.016*	−0.003	−0.016*	−0.016*	−0.016*	−0.014*
		(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Log of Hamlet Population	–	−0.057	−0.006	−0.072	−0.058	−0.058	−0.053
		(0.034)	(0.030)	(0.031)	(0.034)	(0.034)	(0.030)
District average control (September)	–	–	–	–	0.147	0.147	–
					(0.055)	(0.055)	
Constant	1.493*	0.019	0.541	0.096	0.008	0.008	0.042
	(0.105)	(0.452)	(0.389)	(0.423)	(0.445)	(0.445)	(0.390)
N	10075	9707	9707	9707	9707	9707	9707
LM Statistic	117.45**	87.62**	113.14**	102.68**	89.40**	89.40**	37.38**
Wald F Statistic	59.40**	44.17***	57.19**	51.83**	45.08**	45.08**	18.75*
Hansen's J Statistic	0.200	0.053	0.440	0.443	0.018	0.018	1.748
p-value	0.654	0.818	0.507	0.506	0.893	0.893	0.186

All models are IV-OLS regressions. *= $p < .001$. Two-tailed tests. Standard errors in parentheses. For Wald F statistics, ** = $p < .05$ less than 10% IV bias, * = $p < .05$ less than 15% IV bias.

In all models, the effect of bombing on insurgent control is positive, and this coefficient is very precisely estimated. These results mirror those from the ordered logits above, with the caveat that since we have modeled the dependent variable as a continuous rather than ordinal variable, the coefficients have different substantive interpretations.[14] As expected, our index of insurgent control in September is strongly related to insurgent control in

[14] We are not aware of any method that consistently estimates ordered logistic regression models with endogenous independent variables.

December, and Model 5F shows that insurgent control in September is a strong predictor of *changes* in control in December. Finally, Model 5G controls not only for prior levels of local control, but also for the prior *changes* in local control. Each model confirms that our results do not depend on how we model insurgent control or how we proxy the dependent variable. While not the primary focus of this article, coefficient estimates for several control variables are no longer significant. In particular, variables measuring village-level development and the log of hamlet population appear only weakly related to insurgent control in these models. A large body of macroscale evidence supports the claim that per capita GDP and population size are associated with civil war onset and duration (Hegre and Sambanis 2006). Our analysis suggests that these factors play less of a role in shaping the dynamics of local territorial control in insurgencies.

The diagnostics presented in the final four rows of Table 5 allow us to test some of the key assumptions of our identification strategy. The LM statistic tests the hypothesis that our instruments predict bombing in September 1969. The high chi-square test statistics allow us to confidently reject the null hypothesis that our model is not identified. The Wald F test statistic tests the joint significance of the excluded instruments in predicting bombing in September to guard against "weak identification." All models well surpass the conventional benchmarks identified by Stock and Yogo (2005). Finally, we test the overidentifying restrictions of our two instruments using Hansen's J test.[15] Our test statistics are small, and their p-values are far from conventional levels, so we cannot reject the null hypothesis that the instruments are valid. Together, these specification tests support our contention that the instruments for bombing are both excludable and relevant.[16]

Strategy Two: Genetic Matching

Matching techniques facilitate the estimation of causal effects using observational data by choosing observations that approximate the counterfactual quantities of interest. In our application, we wish to compare the level of insurgent control in hamlets that were bombed to the level of control in hamlets that were not bombed. Since bombing cannot be randomly assigned to hamlets, we create a "control" group of hamlets that are as comparable as possible to the "treated" (i.e., bombed) hamlets. We do this by using observed data about each hamlet to create a sample of hamlets that differ as little as possible aside from having been bombed or not. We then evaluate the effects of bombing on insurgent control using the matched dataset. Importantly, using this procedure we sacrifice some precision, for we are only able to estimate the effect of a binary treatment (bombed or not) on insurgent control.

There is a lively debate about the proper techniques for matching, so best practices are difficult to determine. Yet we identify two key methodological suggestions. First, Ho et al. (2007a) caution against "controlling for consequences"—including covariates in the matching or estimation procedures that are intermediate consequences of a treatment variable. We are careful to match (and estimate) only models where any covariate is not possibly a consequence of bombing in September. We therefore use data on hamlet population, village-level development, district-level insurgent control, and hamlet-level insurgent control lagged to July 1969. Distance to an international border and rough terrain are constants for each hamlet.

Second, we check to see if our matching procedure fares adequately in creating a truly balanced matched dataset. Diamond and Sekhon (2005) show that matching based on standard distance metrics, such as propensity scores or Mahanalobis distance, can increase bias on some covariates rather than reducing it.[17] We examined balance statistics (available upon request) obtained by matching based on simple "nearest neighbor" propensity score methods as implemented in Ho et al. (2007b) and found that balance worsened notably after matching on some covariates. To avoid this problem, we use Diamond and Sekhon's genetic matching procedure, which searches across the space of all distance metrics to achieve balance. Our results indicate dramatic improvements in balance between "treated" and "control" hamlets, which suggest that the genetic matching algorithm provides data that approximate the counterfactual question: what would the control status of bombed hamlets have been had they not been bombed?[18,19]

[15] Equivalent results were obtained from Sargan's C statistic.

[16] Our IV strategy is uncommon in political science, so we exploit the full panel in a series of robustness tests. All models confirm the findings we present here. These results, together with a discussion, are included as Table 13 of Appendix D in the Supporting Information.

[17] This occurs because basic distributional assumptions about the covariates do not hold. This issue may be relevant in our case, as neither the log of distance to an international border nor terrain variability is distributed normally.

[18] Balance statistics and a brief discussion are provided as Table 14 in Appendix D of the Supporting Information.

[19] We obtained nearly identical estimates of the effect of bombing on control using nearest neighbor matching, as well as such

Table 6 Bombing and Insurgent Control, Matched Data

	Coefficient	Standard Error	T Score
Bombed (binary)	0.536*	0.112	4.806
Rough Terrain	0.014	0.005	2.690
Log of Distance to International Border	−0.266*	0.073	−3.639
Development Index	0.094	0.121	0.778
Log of Hamlet Population	−0.297*	0.066	−4.510
Hamlet control	1.710*	0.088	19.356
District average control	0.584*	0.112	5.197
Cut Point 1	−2.529	0.937	−2.700
Cut Point 2	1.332	0.909	1.465
Cut Point 3	3.737*	0.912	4.098
Cut Point 4	6.180*	0.927	6.666
N	1273		

Ordered logistic regression. *= $p < .001$. Two-tailed tests.

Using the matched data, we investigate this counterfactual by estimating an ordered logistic regression similar to Model 2D above, with the exception that the treatment variable is our binary indicator of bombing. The results are in Table 6. The coefficient on bombing is positive and statistically significant, confirming once again our results from the baseline and instrumental variables models. All other variables are again significant in the expected directions, with the exception of our village-level measure of development. This is further evidence that development does not appear to predict downstream insurgent control. To see the substantive effects of bombing on insurgent control, we turn to Table 7, which was generated using Imai, King, and Lau (2007) and gives the predicted differences in the probability that a bombed versus an unbombed hamlet fell into each one of the control categories in December, conditional on its control status in July. We expect that bombing in September increases the probability that a hamlet is at a higher level of insurgent control in December and decreases the probability that a hamlet is at a higher level of government control in December. The pattern of estimated probability changes confirms this relationship. The negative changes in probabilities appear in the northeast half of Table 7, and the boundary between shaded and unshaded describes roughly a 45-degree line separating the northeast from the southwest halves. Nearly every estimate is significantly different from zero, and the pattern of substantive changes is sensible, decreasing in all columns as the difference in control from July and December increases. These results confirm our assertion that bombing led to increased insurgent control regardless of previous levels of insurgent control.

We conclude with the caveat that matching is no panacea: it cannot correct for endogeneity or measurement error. These are the strengths of instrumental variables regressions such as those shown previously. Nor can matching correct for omitted variable bias. Matching's strength is in reducing the model-dependence of the counterfactual inferences about the effect of a treatment on an outcome when data are not generated via a randomized experiment. Combined with our instrumental variables regressions, matching helps us to confirm that bombing increased Viet Cong control in Vietnam.

In both our analysis using instrumental variables and our matching analysis, the most worrying omitted variable is activity by ground forces, which certainly shaped the outcome of counterinsurgency in the hamlets during the period of our study.[20] Two scenarios are of particular concern. First, if bombing acted as a substitute for ground assault by U.S. or RVN forces, then we might come to believe that bombing was counterproductive when in fact it was simply somewhat less effective than ground assault. Second, if bombing were always accompanied by ground assault, then our findings might reflect shortcomings in the counterinsurgency tactics of ground forces rather than those of bombing. We believe both scenarios are implausible. As we discussed previously, bombing in South Vietnam was tightly restricted by ROEs that appear to have required the presence of a ground-based Forward Air Controller or an ongoing ground assault to justify a sortie. The ROEs themselves were "conditioned by the fact that in-country air activity was directed towards close air support (CAS) of ground forces" (Congressional Record 1985; also see Sams et al. 1970). Thus, we doubt that bombing was typically a substitute for ground assault. We also think it unlikely that our findings are driven by inadequacies of ground force tactics. Were this story correct, it would seem to imply that the gains in RVN control we observe during late 1969 stem from the complete absence of military effort by U.S. and RVN forces, a suggestion we regard as implausible.

alternatives as coarsened exact matching (Iacus, King, and Porro 2009), full matching, and optimal matching. A table of these results, together with a discussion, is included in the Supporting Information as Table 15 in Appendix D.

[20] It is important to note that ground force activity undermines our identification strategy only if it operated in a particular dynamic pattern, responding to control in July and August and affecting control in December conditional on control in September.

Table 7 Bombing and Expected Changes in December 1969 Control Probabilities

	Hamlet Control in July 1969				
Hamlet Control in December 1969	**Full Government Control**	**Moderate Government Control**	**Contested**	**Moderate Insurgent Control**	**Full Insurgent Control**
Full Government Control	**−0.076**	**−0.019**	**−0.004**	**−0.001**	**0.0001**
Moderate Government Control	*0.031*	**−0.103**	**−0.095**	**−0.028**	**−0.006**
Contested	**0.040**	**0.097**	0.008	**−0.099**	**−0.045**
Moderate Insurgent Control	**0.004**	**0.022**	**0.078**	**0.072**	**−0.078**
Full Insurgent Control	**0.0004**	**0.002**	**0.012**	**0.056**	**0.129**

Cells contain the estimated change in the expected probability of being in a specified control level in December, by initial control level in July: $\Pr(Y = j \mid \text{bombed} = 1, \text{control} = k) - \Pr(Y = j \mid \text{bombed} = 0, \text{control} = k)$, where j indexes control in December, k indexes control in July. Estimates whose 95% confidence interval excludes zero are bold. Estimates whose 90% confidence interval excludes zero are italicized. Negative changes in probability are shaded. All other variables are held at their means. The estimates are from Table 6.

Conclusion

Targeting civilians, whether intentionally or through a failure to adequately discriminate, is repugnant. Yet many governments believe it serves their interests; it is vital to assess the validity of their belief. The data left behind from the Vietnam War are extraordinary: a systematic record of civilian victimization. The irony is that it was self-defeating, much as Arendt thought. We are not the first to have criticized the air war over Vietnam, nor are we the first to have claimed it was counterproductive. But critics have been unable to marshal equally systematic evidence about the impact of air power on counterinsurgency efforts.

Our findings contribute to long-running debates about the effectiveness of American strategy in Vietnam. We complement a body of revisionist scholarship (e.g., Sorley 1999) that argues the "pacification" campaign waged by the United States and allied forces after the Tet Offensive was more successful than most observers believed at the time. Our results are consistent with the view that this success was due to a shift away from indiscriminate violence, and toward control operations. Our analysis covers the period in which incumbent forces began to shift their attention to the direct occupation of hamlets and away from attempts to engage the Viet Cong in decisive battles. But even in this period of counterinsurgency success, we find that indiscriminate violence through bombing remained common. Viet Cong successes against U.S. counterinsurgency efforts did not result from a timid reluctance to expand bombing (as maintained by, e.g., Warner 1978, or Senator Goldwater—see Congressional Record 1985); on the contrary, bombing hampered the pacification campaign and more of it would likely have hastened the communist victory.

In a broader context, our results reinforce and extend to the context of counterinsurgency a growing consensus on the limitations of air power as a coercive instrument, and they lend unambiguous support to General McChrystal's attempts to reduce civilian casualties in Afghanistan. We also help to explain why counterinsurgency remains such a challenging task with a long average duration, even for sophisticated, modern armies. Massive advantages in technology and firepower may confer only relatively minor advantages and may even reinforce ongoing insurgencies (Lyall and Wilson 2009). Methodologically, we stress the need for caution in drawing conclusions from the analysis of aggregated data and case studies. Last, on the theoretical front, we recognize that a general evaluation of the impact of indiscriminate violence on rebel activity requires the integration of several factors that have been overlooked by the literature so far (and are the object of recent and ongoing research): these include the characteristics of rebel organizations (Kalyvas and Balcells 2010), the cohesion of armed groups (Staniland 2010), the type of relationship that they develop with civilian populations (Arjona 2010), and the political divisions they engender within those populations (Balcells 2010). Understanding how these factors interact with strategies of violence is essential in moving this research program forward.

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Supporting Information

Additional Supporting Information may be found in the online version of this article:

Appendix A: Index construction and questionnaire text.
Appendix B: Procedure for HES Index Construction
Appendix C: Enemy Military Model (2A) Qualitative Description
Appendix D: Additional Tables and Maps
Map: Bombing and control, Định Tường Province, South Vietnam, July 1969. Scale approximately 1: 350000

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