Into the Arms of the Rebels? Aerial Bombardment, Indiscriminate Violence, and Territorial Control in the Vietnam War *

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Abstract

Indiscriminate violence is a common feature of counterinsurgency warfare. Such violence is frequently purposive, designed to crush insurgent movements, to terrorize civilians into submission, even to depopulate insurgent-held territory. To date, there has been almost no systematic research on whether indiscriminate violence accomplishes the tasks it is used for. We assemble a new and unique dataset comprised of repeated measures of territorial control in over ten thousand individual hamlets in South Vietnam and location data on virtually every bombing mission flown by the air forces of the United States and the Republic of Vietnam south of the seventeenth parallel. Employing instrumental variables and genetic matching, we find that indiscriminate violence through aerial bombing dramatically increased the ability of the National Front for the Liberation of South Vietnam (Viet Cong) to control populated places.

In her reflections on violence, Hannah Arendt (1970:56) famously remarked that "violence can destroy power; it is utterly incapable of creating it." This is a powerful insight which clashes, however, with an equally powerful idea, namely that violence and repression "work." In this paper, we use a uniquely detailed data from Vietnam to try to adjudicate between these contradictory insights. More specifically, we focus on the phenomenon of "indiscriminate" violence against civilians in civil wars.

Indiscriminate violence, understood as the practice of targeting civilians on the basis of a collective attribute (Kalyvas 2006), is a common feature of many conflicts, especially insurgencies. For instance, collective punishment was a staple of counterinsurgency policy during the Second World War as the Nazis faced resistance movements in occupied countries. Serb forces used indiscriminate violence against Kosovar Albanians in 1999, while the Sudanese government's counterinsurgent campaign in Darfur depends mainly upon large-scale attacks on unarmed civilians. In the Vietnam War, the United States and its allies deployed massive firepower in "free fire zones" where every individual was presumed to be an enemy. Even today, coalition forces have ramped up aerial bombardments as part of the "surge" strategy in Iraq, and the United States military continues to stress the vital role of aerial operations as a component of counterinsurgency doctrine (Cordesman 2008; United States Department of the Army 2006: paragraph E1). To observers and victims alike, indiscriminate violence is repugnant. But a belief in the immorality of indiscriminate violence is not incompatible with a conviction about its efficacy.

Recent studies of mass violence and similar phenomena focus on their determinants (Downes 2006; Humphreys and Weinstein 2006; Valentino et al. 2004). The literature on the consequences of these strategies is much less developed. Reviewing a vast body of literature on civil wars, Kalyvas (2006:146-172) argues that even though indiscriminate violence is frequently counterproductive, it may be successful under some conditions—a conclusion also reached by Downes (2007). Nevertheless, much of what we know is subject to a problem of aggregation. "Success" is defined as victory or defeat in war, but many factors beyond the treatment of civilians influence war outcomes. Indiscriminate violence may cripple insurgent movements that emerge victorious in the long run. Alternatively, indiscriminate state violence may push civilians to ally with insurgents, but other factors may hamper insurgent movements as a whole. Politics is often held to explain such discontinuities between the local results of indiscriminate violence and the final outcomes of wars: Merom (2003) argues that indiscriminate violence is effective, but that democracies find it unacceptable, while Arreguín-Toft (2001) argues that indiscriminate violence cripples insurgencies, but is unpalatable to local elites. Even looking at subnational data might not be sufficient to tease apart the relationship between indiscriminate violence and the success of counterinsurgency campaigns, for strategies of violence are endogenous to the results of previous strategies. Indeed, indiscriminate violence may signal the failure of counterinsurgency, rather than causing it.

The aggregation problem suggests a need for systematic research on the effects of indiscriminate violence in local contexts, while the risk of endogeneity makes the task of untangling causal relationships a key concern. Our paper begins with the insight that within civil wars, indiscriminate violence is unevenly distributed across time and space, so that research on the violence itself may hold the key to testing its effectiveness. To this end, we assemble a new dataset on incumbent indiscriminate violence and insurgent territorial control during the Vietnam War, one of the largest insurgencies in modern history. We use detailed micro-data on the control of territory by either forces loyal to the Republic of Vietnam or the insurgency represented by the National Front for the Liberation of South Vietnam—the Viet Cong. Our proxy for indiscriminate violence is aerial bombing. While much of the war's aerial bombardment was directed at North Vietnam or at concentrations of communist forces operating in remote and relatively uninhabited countryside, bombing was also used extensively in heavily populated territory where a bitter struggle for population control played out between the incumbents and the Viet Cong. The very inability of aerial counterinsurgency tactics to discriminate between desired targets and loyal civilians was at the time considered a major justification for its use. Bombing, in fact, was one of the most common forms of indiscriminate violence used against civilians during the twentieth century. A focus on bombing's effects in Vietnam can therefore shed light on the question of the effects of indiscriminate violence in general.

Among studies of violence, our data are unparalleled in their coverage. We analyze over ten thousand hamlets, covering the entire territory of the Republic of Vietnam, and we have repeated measures of territorial control along with a rich set of important control variables. Moreover, our data on aerial bombing are remarkably precise. We know the location of virtually every payload of munitions dropped over the Republic of Vietnam between 1965 and 1975. Our data show that both bombing and control varied spatially and temporally throughout the war: some hamlets were bombed scores or even hundreds of times over a few months; others were rarely or never bombed. The combination of scale, variation, and unsurpassed data coverage makes Vietnam an ideal case to test the effect of indiscriminate violence on insurgent control.

We find that indiscriminate violence in the form of aerial bombing was wholly counterproductive as a counterinsurgency strategy in Vietnam. We find unambiguous evidence that higher frequencies of bombing correspond to higher levels of downstream territorial control by the Viet Cong. Recognizing that the relationship between bombing and insurgent control is complex, plagued by unobserved heterogeneity and endogeneity, we build our causal story through a variety of methods, employing instrumental variables and matching as supplements to our baseline statistical models. Consistent results from these methods show that aerial bombing resulted in increased Viet Cong control.

Our findings are the most rigorous evidence available that indiscriminate violence drives its victims to side against its perpetrators. They demonstrate just how much the success of counterinsurgency depends on the methods with which it is fought. In Vietnam, the indiscriminate slaughter of civilians through aerial bombardment undermined counterinsurgency efforts. Our findings also contribute a new within-country focus to the cross-national literature on the use of strategic bombing to achieve political goals (Pape 1996; Horowitz and Reiter 2001; Allen 2007). 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 paper illustrates the utility—and the challenges—of within-country research strategies in studying violence in civil wars (Sambanis 2004; Kalyvas 2006). 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 proceed as follows. We discuss the microfoundations of indiscriminate violence in the context of insurgency and underline the indeterminacy of the theoretical debate about its effects. We then discuss the data and proceed inductively, by identifying the determinants of aerial bombing in South Vietnam. We analyze the effects of bombing and conclude.

Insurgency and Indiscriminate Violence

War rarely fails to have some effect on civilians within the combatant states. Insurgency, however, implicates civilians more directly in the process of warfare than conventional forms of conflict.[1] Descriptively, the key 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-scale or decisive ones, are rare or absent, and front lines do not form. Insurgents attempt to organize and control the civilian population, mainly in the countryside, as a means to gradually build up forces sufficient to take over the state or detach a portion of the state's territory. Civilians are exploited for manpower, tax revenue, and information security. Given their inability to draw insurgents into sustained combat, the state is forced to compete with insurgents for control over the population.

Violence against civilians is a central feature of such conflicts. In Kalyvas's (2006) equilibrium framework, combatants use violence selectively in a process of competitive terror. Since indiscriminate violence punishes individuals regardless of their actions, it is strictly

[1] The term "insurgency" refers to a cluster of ideas for which the terms "guerrilla warfare," "asymmetric warfare," and "unconventional warfare" are also sometimes used. Although there are some subtle differences between the extension and intension of these terms, for the purposes of this paper they may be treated as synonymous. Likewise, we use terms like "rebel" and "insurgent" interchangeably.

counterproductive, while selective violence (individually targeted homicide) furthers the goals of combatants. However, state forces—and sometimes also insurgents—nevertheless often commit face-to-face massacres, destroy the physical and ecological infrastructure that supports both life and livelihood, or kill on the basis of broad organizational or identity profiles. A particularly common form of indiscriminate violence is the intentional use of military technologies like artillery, car bombs, or aerial bombardment that cannot discriminate effectively between combatants and noncombatants.

Table 1 provides an initial picture of the prevalence of mass civilian slaughter since the Second World War. Using data from Valentino et al. (2004), it tabulates the presence of indiscriminate slaughter (termed "mass killings") by the type of civil war being fought. For our purposes, guerilla wars are nearly equivalent to insurgent wars. Fully one third of such wars produced mass killings, and Fisher's exact test rejects the null of no association between guerrilla wars and mass killings at the $p<.001$ level.

-- Table 1 about here --

The data show that the indiscriminate slaughter of civilians is a common feature of warfare in insurgencies. Yet there remains no consensus as to whether the tactics of indiscriminate violence accomplish their central goal of crushing insurgencies. Kalyvas (2006: 167) cites a large number of studies that point to the futility of such tactics, but he admits that "[T]he conjecture that indiscriminate violence is counterproductive is not based on systematic research."

Several types of microfoundations support the intuition that indiscriminate violence does not pay for its users. First, violence can provoke rage and hatred toward the perpetrators, which can act as powerful emotional motivators for revenge. According to this logic, killing more people, whether selectively or indiscriminately, creates more enemies for the perpetrator. Rubin (New York Times Magazine, February 24, 2008) shows nicely how this process has played out in the Korengal Valley of contemporary Afghanistan:

> In the predawn light Bone — the nickname for the B-1 bomber that seemed to be the soldiers' favorite — winged in and dropped two 2,000-pound bombs above the village. Finally, around dawn, a weary [Capt. Dan] Kearney, succumbing to gallows humor, adrenaline and exhaustion, said: "O.K., I've done my killing for the week. I'm ready to go home…." Kearney estimated that they killed about 20 people, adding: "I'm not gonna lie. Some are probably civilians…." Killing women and children was tragedy enough. But civilian casualties are also a political issue. If he didn't manage to explain his actions to the Yaka China villagers and get them to understand his intentions, he could lose them to the enemy.

Second, such emotions may be intensified by a sense of injustice: death may not be what militants and collaborators deserve, but it is a risk they are thought willingly to entertain. Innocent civilians killed in air strikes or massacres die in spite of their decision not to participate, adding the insult of injustice to the injury of violence. Third, when participants in violent collective action are the only or the principal victims, violence acts as a deterrent to militancy. Individuals may calculate that non-participation increases their chances of survival, exacerbating the "rebel's dilemma" by intensifying their collective action problem (Lichbach 1995). Violence that is truly indiscriminate, in the sense that the probability of victimization for participants and non-participants is equal, should increase participation by shifting its payoff vis-à-vis non-participation. Even when actors want to use violence with discrimination, perverse selection mechanisms can generate a high proportion of innocent victims; in fact, noncombatants may be victimized disproportionately because they are easier than insurgents to find and kill. At the extreme, if militant survival prospects are better than those of civilians, indiscriminate violence has the potential to endogenously reinforce individual participation in insurgencies (Kalyvas and Kocher 2007).

While these arguments are powerful, compelling logics and some important evidence point in the opposite direction. First, civilians living in rebel-controlled areas may reduce their risk by persuading the militants to decrease provocative behavior or to leave the area entirely. At an extreme, civilians may use alternative forms of solidarity to fight the insurgents. These mechanisms have been used to justify or rationalize strategic bombing or blockade as tools of coercion in international politics (see Pape 1996 for discussions and a critique). If they hold in those settings, there is reason to suppose they might work in contexts of insurgency.

Second, if civilians are fully rational and entirely self-regarding actors, then there is no reason to suppose that indiscriminate violence by one side will induce them to prefer the other side. Instead, civilians should choose the side with the best expected payoff, which could very well be the more violent party. Guatemala's civil war appears to have followed something like this process in the late 1970s and the early 1980s (Stoll 1993, 1999). Massacres of entire villages induced the men of other nearby villages to join "civil patrols" that cooperated with the military against the communist EGP (*Ejército Guerrillero de los Pobres* or Guerrilla Army of the Poor). In many less dramatic cases, civilians have fled from areas in which the state used a great deal of indiscriminate violence, but rather than seeking shelter with the guerrillas (who could not, in any case, support them), they have taken refuge in government zones. Emotional mechanisms or intuitions of justice may rationalize or even motivate these behaviors as well: civilians have been known to blame the opposition for provoking violence carried out by the state (Cole 2001).

Third, microfoundations are rarely the entire causal story. Microfoundational mechanisms are situated within meso- or macro-level processes that determine when and how the micro is activated (Derluguian 2005). The larger strategic context of insurgent warfare is one such example. While the Viet Cong relied upon classic insurgent techniques locally, they also created highly-trained and reasonably well-equipped company- and battalion-strength units that could support local guerrillas in defending their territory or launch powerful attacks against government forces. American commanders believed that unless they could defeat these large units, or at least substantially reduce the threat they posed, then small government units in the villages would risk being overrun. Once the battalions were blunted in large-scale combat, counterinsurgency could proceed on a solid foundation of macro-level military strength. The use of airpower was an essential element of this battle doctrine, even though it had the potential to kill many civilians in the process.[2]

Fourth, a good deal of evidence shows that state forces have routinely used indiscriminate violence against civilians for many years. For instance, Valentino, et al, (2004) find that insurgency is a key predictor of mass killing, as counterinsurgents attempt to "drain the sea" of civilian support that sustains insurgencies. Similarly, Downes (forthcoming) finds that "wars of attrition," that is, long-running and costly conflicts, tend to be associated with high levels of civilian victimization in war. Assuming that military establishments learn from other military establishments, it is difficult to understand why so many states have relied on collective targeting if it does not advance their military goals. Moreover, some important cases would imply that

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[2] One possibility is that aerial bombardment creates more insurgents than it kills *when bombs fall on civilians*. If aerial bombardment also kills large numbers of fighters in battle and few enough civilian areas were bombed, then indiscriminate violence (in essence, a high rate of error) could be counterproductive locally but productive globally.

militaries have failed to learn even from their own experiences. For instance, as we show below, the U.S. continued to use heavy bombing in populated areas after many years of experience in Vietnam. If indiscriminate violence is counterproductive, why is it so widely used?

Fifth, the most rigorous study of this problem to date, which uses an innovative quasi-experimental research design to examine the consequences of Russian artillery barrages on the violence of insurgents in Chechnya, finds that fewer insurgent attacks were carried out in villages that were shelled than in matched villages that were not shelled (Lyall 2007).[3]

In sum, there is no well-supported consensus in the literature as to the effect that indiscriminate violence has on civilian behavior in insurgencies. The principal obstacle to studying this phenomenon is the difficulty of obtaining high-quality, systematic data on both the

[3] Lyall's analysis has three main limitations to which we should be attentive. First, it covers a small number of villages for a short period of time; idiosyncrasies of the Chechnyan context could be driving these results. Second, he does not specify a mechanism that links the Russian military's indiscriminate violence to lower levels of insurgent reprisal *in the same areas*. The standard microfoundational logic suggests that indiscriminate violence motivates participation, not reprisal. Insurgents may respond to overwhelming indiscriminate violence with new tactics, reducing open attacks against incumbent strongholds (which they know will fail) and shifting towards local infiltration and agitation. Indiscriminate violence, in short, may lead to short-term decreases in insurgent attacks precisely because it increases insurgent mobilization. Moreover, Lyall's research design depends on the premise that Russian artillery strikes were random in this region. He posits two randomization mechanisms: drunkenness was rampant at one of the Russian fire bases he examines; at the other, the Russians employed a technique known to the American military as "harassment and interdiction fire" (H&I fire). Neither of these mechanisms is likely to produce randomization in the technical sense. Drunks may fire their weapons randomly, but they may just as well fire, for instance, at visible hilltops in order to watch the explosions. H&I fire is, as Lyall notes, intended to be random *within the barrage area*, but the definition of this area itself is typically not random. H&I is used to disrupt the activities of insurgents in their areas of strength or along lines of communication. To accept Lyall's premise, we must suppose that the Russians shelled villages they controlled or those occupied by sympathetic villagers just as often as villages with a history of insurgent violence and resistance. Lyall advances at least two additional mechanisms that have not been discussed so far. One is that indiscriminate violence terrorizes civilians out of collaborating with rebels, especially if there is an expectation of additional violence in the future. But, if violence is truly random as Lyall's analysis presupposes, then a failure to collaborate with the rebels will not help one to avoid it. Quite the contrary, it may increase the danger. Lyall also posits that indiscriminate violence may harm rebel fortunes by destroying the population base on which the rebels depend, either by actually killing the civilians or causing them to flee. At the extreme, in the form of genocide or politicide, or over the long run, this is surely true. Huntington (1968) argued that precisely such a long-term process of violence-induced population shift was underway in Vietnam, though he did not recommend either continuing or intensifying it. But given a partially random and slow-moving process like the bombardments of Chechnya and Vietnam, it is not clear why we should expect any short-run effect. The people who are likely to flee a bombarded area first are those least committed to the insurgency, possibly increasing local solidarity.

putative cause and the outcome. For most cases, we have neither. For a few cases, we have one but not the other. The Vietnam War may be the only historical case for which reasonable measures of both were constructed at the time and are no longer regarded as state secrets.

The Data and its Context

Although the war in South Vietnam was fought in a variety of ways, it was at its core a classic insurgency. It pitted the incumbent forces of the Republic of Vietnam (RVN) and the United States, along with several allies, against a large and amorphous insurgent organization (the Viet Cong) supported directly by the Democratic Republic of Vietnam (North Vietnam). Like other insurgencies, the war featured dramatic asymmetries in firepower between the incumbents and the Viet Cong. Moreover, indiscriminate violence by was a hallmark of the conflict, including massive aerial bombings throughout Indochina as well as search and destroy missions and "free fire zones" in the South.

The air war in South Vietnam is frequently ignored by military historians and political scientists, perhaps owing to the greater political emphasis on strategic bombing of North Vietnam under the Johnson and Nixon administrations. As measured in tonnage, though, by 1969 South Vietnam was already one of the most heavily bombed countries in history—third only to North Vietnam and Laos (FitzGerald 1989:470). The tonnage dropped from 1965 to 1967 on South Vietnam was double that dropped on the north (McNamara 1995:243). Schlight (1994) estimates that over the course of the war, fully seventy-five percent of all aerial missions flown over Indochina were flown over South Vietnam. Indeed, among American military planners the use of aerial firepower was an integral part of counterinsurgency warfare (United States Air Force 1967).

American military planners understood that aerial bombing in South Vietnam was almost entirely incapable of distinguishing supporters of the Viet Cong from opponents and neutrals (McNamara 1995:243). They believed that such massive indiscriminate firepower from the US and RVN would terrorize Vietnamese civilians into siding against the Viet Cong, and that the threat of random death from above would push civilians to leave their homes as refugees. A RAND corporation study from 1965 argued that aerial bombing was turning civilians against the Viet Cong by demonstrating the insurgents' inability to protect them (FitzGerald 1989:208). Moreover, these perceptions were clearly communicated to South Vietnamese civilians, as demonstrated in a flyer dropped over much of the country as a component of the military's psychological operations:

> 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* [emphasis added]. Be sure to be wise and don't be undecided any more… (Littauer and Uphoff 1972:47).

These statements reflect an operational belief that massive indiscriminate firepower would further the counterinsurgency campaign in Vietnam.

Analogues can be found at the highest levels of command. Writes R.W. Apple (1971), "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.'" Bombing, like defoliation and crop destruction, was designed to displace and terrify all civilians, not only known insurgents (Gibson 1986:226-231). General William Westmoreland, as head of U.S. Military Assistance Command, Vietnam, was famously unconcerned with the civilian causalities associated with the air war over South Vietnam, remarking that the air war "does deprive the enemy of population, doesn't it?" (Halberstam 1972:550).

Given the widespread violence unleashed through aerial bombing in South Vietnam—and the fact that decision makers were both aware of its indiscriminate nature and specifically welcomed it—we use it to investigate the effect of indiscriminate violence on the fate of the Viet Cong insurgency. We assemble our data using two sources collected by the US Government as operational tools of warfare. The Hamlet Evaluation System (HES) was developed by analysts from the Southeast Asia Division of the Office of the Assistant Secretary of Defense for Systems Analysis, an agency of the Defense Department under Robert McNamara. Given the complex and shifting character of the battlefield in Vietnam, the HES was intended to provide a clearer picture of who controlled what and when.[4]

We rely on an index variable, the "HES Security Sub-Model," that describes variation in local control on a 5-point scale, the categories of which we have designated "strongly government controlled" (1), "moderately government controlled" (2), "contested" (3), "moderately insurgent controlled" (4) and "strongly insurgent controlled" (5). The index is based on a series of questions that ask about the presence (or absence) of incumbent and insurgent personnel in the hamlet (CORDS/RAD 1971). To link insurgent control to theories of indiscriminate violence, we assume that local control is a direct consequence of the degree to which insurgents and incumbents can secure the participation of local civilians.

[4] For more detailed information on the HES, see Kocher (2004). The data are fully described in CORDS/RAD (1971).

One concern about the data stems from the partiality of the actors who created them. The U.S. government, and especially the Department of Defense, had strong incentives to shape the data in ways that would justify (*ex ante*) and rationalize (*ex post*) its conduct of the war (see critiques in Gibson 1986; Kolko 1985; Thayer 1985). In spite of these concerns, we have several reasons for confidence in the data. First, even if the Department of Defense systematically inflated the number of hamlets falling under government control, we make no inferences regarding the aggregate fortunes of either party to the war. We examine only variation among cases that were coded in similar ways. This variation is substantial: many hamlets were coded as falling under partial or complete Viet Cong control for many months or years; many were regarded as stably and reliably controlled by the Vietnamese government; many shifted among control categories, in some cases several times in either direction. 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 data sets to create a misleading picture from the intersection of the two. Finally, as we demonstrate, our results do not reflect well on the decision makers of the time. Not only did the US kill hundreds of thousands of South Vietnamese civilians with airstrikes, but it appears much of this bombing was done needlessly. To produce our findings, analysts would have had to overestimate the failure of their own efforts, and then only in the hamlets that they targeted for aerial bombing.

The bombing data are derived from two systems that compiled records of post-flight pilot reports: the Combat Activities Asia file (CACTA) – active from October 1965 through December 1970 – and the Southeast Asia Database (SEADAB) – active from January 1970 through June 1975. The data were converted to a contemporary, platform-independent format by Management Support Technology Inc., of Fairfax, Virginia under contract to the Defense

Security Cooperation Agency Humanitarian Assistance and Mine Action unit (DSCA-HAMA) US Department of Defense. Included in the database are: the number of aircraft involved in each sortie, the type and number of munitions dropped, the date and geographic coordinates of the attack.

We study a full cross-section of South Vietnam during a 6-month period from July through December, 1969. Two rationales drive this choice. First, appropriate data on local control are not available for time-periods prior to July, 1969, and there is an additional gap in the data from January, 1970 – June, 1971 (with the exception of a few missing months, the bombing data are available continuously from 1965 through to 1973). Data availability accordingly constrains our analysis.

Second and perhaps more importantly, the virtue of studying this period is that it displayed greater variation in control than periods that followed, and it was a period in which counterinsurgency warfare appeared to be successful (Davidson 1988:610-612; Goodman 1970; Sorley 1999:154-160; Tran 1980:16-25). June and July of 1969 saw few Viet Cong-initiated attacks, and indeed little direct combat overall (Davidson 1988:597, 612-617; Duiker 302-307). An "Autumn Campaign" initiated by the Viet Cong in August of 1969 met with little success, and captured documents from the North Vietnamese Army's Central Office for South Vietnam (COSVN) reveal frustration with the apparent success of rural pacification efforts by the US and RVN (COSVN 1969). Still, aerial bombardment of heavily populated areas remained prevalent as allied forces increased their operations in preparation for the draw-down of forces occasioned by Nixon's "Vietnamization" policy (FitzGerald 1989: 508-511). Late 1969 is accordingly a propitious time frame to test whether or not *aerial bombing* decreased insurgent control, for this period witnessed a lull in the ground war and apparent counterinsurgency success, but still high levels of aerial bombardment.

Determinants of Aerial Bombing

We begin our analysis by exploring the determinants of bombing in South Vietnam inductively, using maps to illustrate the spatial variation of bombing. Maps 1 and 2 plot bombing sorties and populated hamlets in the northern and southern parts, respectively, of South Vietnam.[5] Two things are immediately clear. First, bombing on a fairly massive scale was carried out in every region of South Vietnam. Second, and contrary to what some historians of the war have maintained, bombing was used massively in areas that were heavily populated. For instance, over the second half of 1969, four years after the entry of U.S. ground forces into the war in Vietnam and following a full decade of advisory and combat experience against the Vietnamese communists, fully 30% of all the country's hamlets were bombed at least once. During the same period, 18.65% of all recorded bombing sorties were targeted to within two kilometers of a hamlet. Some populated areas were spared during this period, like the far northern coastal region, but most Vietnamese civilians had very intimate experience with aerial bombardment.

Map 3, a density plot of bombing sorties per square kilometer, gives a useful summary. It shows that a very narrow coastal strip along the far northern coast of the country suffered from relatively few attacks, while the coastal mountains directly east of these areas were massively

[5] Both the HES and the bombing data from SEADAB and CACTA recorded locations using the Military Grid Reference System (MGRS), a slightly modified version of the Universal Transverse Mercator projection system. For both datasets the datum is Indian 1960, based on the Everest Spheroid. Since Vietnam falls directly on the border of UTM zones 48 and 49, we re-projected the data into a non-standard Transverse Mercator projection, with a central meridian of 106.75°E.

bombed.[6] Likewise, very heavy aerial bombardments fell all around the capital, Saigon, but especially to the East, Northeast, and Southeast. The purple, roughly triangular shape that we have encircled is the capital, Saigon. Map 4 is a density plot of hamlets. Examining the two maps side-by-side confirms that while not all heavily populated areas were bombarded extensively, most were. These large-scale maps also hint that terrain and proximity to international boundaries may be somewhat associated with bombing, but they are unlikely to be very influential factors in a statistical model.

Map 5 moves us in for a much closer look at a single province, Dinh Tuong, in the northern Mekong Delta to the southeast of the capital. The underlying base map is from a series produced by the South Vietnamese government in 1970; it reflects 2nd and 3rd level administrative boundaries and other features as they were found in the late 1960s. This map plots bombing sorties, as well as hamlets by their control status in July 1969. At this point in the war, no hamlets in Dinh Tuong were regarded as fully controlled by the government. We can see from the maps that both control and bombing were highly clustered. For instance, the hamlets immediately surrounding the capital, My Tho, in the southeastern part of the province are all controlled by the government. Likewise, the district capitals are clusters of green dots. Aerial bombardments were directed overwhelmingly at areas with clusters of (red) strongly insurgent controlled hamlets. Contested (light green) and moderately insurgent controlled (pink) hamlets generally did not have bombing directly in their vicinity, though some are adjacent to areas of heavy bombardment. It is easy to see that a strip with little bombardment runs right through the

[6] The very thin purple strips along the central coast, and the Laotian and Cambodian borders, are an artifact of the procedure used to produce the density plot. Since bombing sorties inside of these two countries were excluded from the analysis, and few bombs fell at sea, the areas just adjacent to the border and the coast appear to have had a lower bombing density, when in fact bombing was if anything more massive on the other side.

center of the province, following the route of Highway 4, one of the principal motorways of South Vietnam.

The even closer look provided in Map 6 shows that, for the most part, hamlets were not directly bombarded. Rather, local roads and especially canals in the immediate vicinity of insurgent controlled hamlets were the targets. According to Elliott (2003), who worked in the area during this period and later reconstructed its history from Vietnamese-language local sources, the canals supported trees and undergrowth that was used to conceal Viet Cong bunkers and strong points from which the guerrillas operated. It appears that these were often the targets of aerial bombardment, but consequently bombs often fell quite close to populated hamlets.

In order to get a more systematic sense of the relationship between control and bombing, we mapped bombing sorties onto hamlets over the full territory of the country. Map 7 illustrates visually the procedure we used (once again using a part of Dinh Tuong Province as an example). The large circles represent search radii of 2 kilometers around the coordinates of hamlet centers; to make the map more visually tractable, we show only Viet Cong-controlled hamlets. The triangles are bombing sorties, in this case all sorties that fell in this area during July of 1969. We used this procedure over the entire territory of the RVN to create a variable that counts the number of sorties that fell within the search radius of each hamlet; we also generate a binary variable for each hamlet that indicates simply whether the hamlet was bombed or not.[7] As we can see from the map, many sorties could fall within the range of a single hamlet. In addition, a single sortie often fell within the search radius of several hamlets. The working database we created preserves this many-to-many relationship structure.

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[7] The procedure we used also recovers the distance, in meters, of each sortie from each hamlet center within the search radius. This continuous measure of bomb distance may be useful in future work.

Several important simplifications are built into this procedure. First, like all populated places, Vietnamese hamlets varied in physical extension and shape, but none of this information can be easily recovered from data or maps from the period. Second, we choose the two-kilometer search radius with the intuition that bombs dropped near (but not on) a hamlet could strike residents walking to water sources, working in the fields, or otherwise moving outside of the hamlet center. Also, given the "dumb" bombing technology of the time, many bombs did not fall where they were intended to fall. At the same time, as the search radius increases much beyond two kilometers, the number of hamlets struck by each sortie begins to increase very rapidly, making the database intractably large. Third, bombing sorties varied quite a bit in terms of their size and the destructive potential of the technology used. Individual B-52 strikes could run to many tons of explosives; A-10 attacks often involved only strafing with 20mm canon. Much of this information is preserved in the data, but it will require many hours of additional research on Vietnam-era weapons systems to be useful.[8]

Table 2 shows the relationship between control status and bombing for the six months from July – December 1969. It shows that about 10% of the hamlet observations had bombing sorties within two kilometers during this period.[9] Insurgent controlled hamlets were vastly more likely to be bombed than either contested or government controlled hamlets. In fact, a hamlet in strong insurgent control was over fourteen times more likely to be bombed in a given month than

[8] Some preliminary mapping exercises suggest that very large bombing sorties were not common in heavily populated areas of Vietnam. For instance, most B-52 strikes were directed at highland areas along the borders with Cambodia, Laos, and North Vietnam. Note also that a very small number of sorties dropped leaflets or flares. These have been excluded from the analysis.

[9] In this case, the unit of analysis is the hamlet/month. On average less than half of the hamlets bombed in a given month were bombed again in the subsequent month, while between 5 and 7% of hamlets not bombed in a month were bombed in the following month. Thus, 10% understates the percentage of hamlets that were bombed at least once over this six month period. That figure, given previously, is 30%.

a hamlet in strong government control. Each category closer to insurgent control corresponds to about double the risk of bombing.

-- Table 2 about here --

As we saw in the maps, however, bombing appears to have been influenced not only by the status of the hamlet itself, but also by the regional strength of the conflict parties. Some contested or government controlled hamlets appear to have been bombed because they were adjacent to areas where the insurgents were powerful. To simultaneously hold constant regional strength and local control, we calculated the average of the control variable within villages (which usually contained several hamlets) and also within districts.[10]

Table 3 shows a series of pooled negative binomial regressions that incorporate regional strength and several other variables into the analysis.[11] Our assumption is that the level of a party's regional strength should be reflected in its control over nearby hamlets. The dependent variable in these models is the count of bombing sorties within two kilometers of the hamlet center. Model 3A estimates the association of local control with bombing while holding constant the average control level at the village and the district. The results show that, on average, strongly insurgent controlled hamlets were attacked by about seven times as many bombing sorties as strongly government controlled hamlets. A 1-unit increase in average village-level control corresponds to about 45% more bombing sorties. District level average control is

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[10] In the Republic of Vietnam, "villages" were like American towns, with fixed territorial extent and surveyed boundaries. Hamlets were residential clusters.

[11] The decision to pool is based on the reasonable assumption that bombing itself is not state dependent from month to month, i.e. that the decision about whether or not to bomb in specific areas was based on their perceived characteristics independently each month. Note, also, that the dispersion parameter (α) is quite large and the test statistic is significant at the $p < .001$ level; given the evident overdispersion of the dependent variable, a negative binomial framework is appropriate.

important as well, predicting around 38% more bombing sorties per one-unit change. Model 3B incorporates monthly fixed effects, with no change to the key results. We can also see that the bombing of populated places declined somewhat for several months during the second half of 1969 before picking up slightly at the end of the year.[12]

-- Table 3 about here --

Model 3C incorporates several additional control variables. From the HES, we compute a development index using factor analysis on two categorical variables that measure, respectively, the presence of televisions and motorized vehicles. We found inductively that a village-level development average provided a better fit to the data. We also include a variable that measures the log of the distance to the closest international boundary.[13] A third variable measures local terrain variability. Finally, we include the log of hamlet population, taken from the HES.

All of these variables are statistically significant at the 1% level of confidence; however, none matches the substantive significance of local control. The two most influential variables are the level of development and the distance to an international border. According to these results, poorer places were bombed more heavily even holding constant local and regional control status. There is some reason to believe that the result is an artifact of proximity to major

[12] That bombing declined over the course of 1969 has important implications for our findings on the downstream consequences of bombing. We find that bombing was counterproductive. Given that it was also widely used, one might reasonably ask why the U.S. and the Republic of Vietnam were so successful at consolidating control on the ground from 1969 – 1971. Part of the answer may be that the use of bombing declined and was replaced by alternative counterinsurgency strategies.

[13] The boundary data are based on the Digital Chart of the World (DCW), with the demilitarized zone hand-drawn.

highways and waterways,[14] or that development was endogenous to prior shifts in control; these factors should be included in future analyses of the data. Hamlets located close to international borders were also bombed more heavily. Given that the interdiction of transit routes through Cambodia and Laos was a major U.S. military objective, this result is not surprising.

Effects of bombing

The analysis in the previous section establishes that U.S. forces bombed in the environs of insurgent-controlled hamlets much more frequently than they did in incumbent-controlled areas. This creates a serious problem for any attempt to evaluate the *effects* of bombing (or any other form of indiscriminate violence): how 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? Ideally, we would begin with a random selection of bomb targets and compare the posterior control status of those cases with the unbombed control group. But, as we have seen, bombing was far from random in the Vietnam War: it was systematically targeted to places where the Viet Cong was powerful.

Table 4 shows the results of a simple tabulation of control status and bombing. The five major columns of the table represent control categories in July of 1969. For instance, the first column contains only hamlets that were rated as fully controlled by the government in July, the second column contains only observations that were partially controlled by the government in July, and so forth. Within each column, we cross-tabulated bombing with control status in the same hamlets six months later, in December of 1969. In this way, we can examine the

[14] An ongoing goal of our project is to georeference all of the information contained in the 1970 map series (Nha đia-dư quó□c-gia 1970), which will permit us to incorporate data on road and canal density and proximity into the analysis as well to analyze the bombing data at alternative levels of aggregation.

downstream consequences of bombing while holding constant a prior measurement of control. We have bolded two cells within each major column; all of the observations in these cells have the same control value in December that they had in July. The diagonal formed by these bolded cells show that unsurprisingly, a majority (about 59.5%) of Vietnamese hamlets fell into the same control category in December of 1969 that they occupied six months earlier.

-- Table 4 about here --

In four of the five major columns, hamlets that were bombed in July were more likely to move toward insurgent control, and correspondingly less likely to move toward incumbent control, by December than hamlets that were not bombed in July. In the remaining column, the highest level of incumbent control in July, the data show that a slightly higher proportion of bombed than unbombed hamlets remained strongly controlled by the government (though this sub-table is not statistically significant). Consider, for instance, hamlets that were coded as being "contested" in July 1969. Within this group, about 40.0% of the non-bombed hamlets were rated as under moderate or high government control six months later, while about 25% of the bombed hamlets were similarly rated by December. For the same group, 22.9% of the bombed hamlets moved into one of the two insurgent-controlled categories, while only 14.1% of the non-bombed hamlets did the same. Among hamlets that were strongly controlled by the insurgents in July, 75% of the bombed hamlets remained in the same category, while about 50% of the non-bombed hamlets moved in the direction of government control.

This evidence establishes that bombing is strongly 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 otherwise unobserved sources of insurgent strength or incumbent weakness in July which led to gains in control for the insurgents. Put differently, bombing signals that the incumbents are in a tough fight, either because they are pushing aggressively into rebel-held territory or because the insurgents are seeking to expand their own zones of control. Still, this only damages our results if tougher fights systematically favored the insurgents, and equally in areas where the incumbents were defending places they already controlled. This also would require that incumbents were comparatively successful in expanding their control and defending previous gains in precisely those places where they did not fight hard (per hypothesis, in the areas where there was less bombing). This is especially puzzling for hamlets that were in strong insurgent control in July 1969 but moved toward government control by December. Another possibility is that the incumbents used alternative counterinsurgency strategies in places that appeared to them propitious for expansion of their zones of control, while they used bombing to “hold the line” elsewhere. An implication is that these areas would have been worse for the incumbents (i.e. more of them would have moved further into insurgent control) than they were in the absence of bombing.

One way to check both possibilities is to hold constant not only the prior level of control for each hamlet, but also the prior level of control in variously-defined 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 is 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 once again use the average of the control variable within villages and within districts in July. The first column of Table 5 recapitulates the results of the contingency table analysis in the form of an ordered logit. Positive coefficients indicate increased insurgent control. As expected, bombing in July is a statistically significant predictor of control status in December, holding constant the previous level of control. The second column introduces the village- and district-level average control status in July. Both are extremely powerful predictors of control six months later, confirming the intuition that regional control reflects the capacity to influence local control over time. The coefficient for bombing is slightly reduced, but it remains a significant predictor of downstream rebel control and counterinsurgent weakness. The third column introduces the same set of control variables found in Model 3C. Unsurprisingly, development level, distance to international boundaries, and rough terrain are all statistically significant predictors of control status 6 months later. The coefficient for bombing falls very slightly, but remains significant.

-- Table 5 about here --

Table 6, generated using Imai et al. (2007), 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 (the probabilities are derived from Model 5B). Negative differences are shaded, while statistical significance is indicated in bold. The overall pattern is about what we should expect to see if bombing made it tougher for the incumbents to gain control of hamlets. The negative sign of the first differences on the upper-right hand side of the matrix tell us that bombed hamlets were more likely to move out of government control that unbombed hamlets, while the positive signs on the lower-right hand side of the matrix reflect that bombed hamlets were more likely that unbombed hamlets to move into insurgent control 6 months downstream. Only two of twenty-five cells do not fit the expected pattern.

-- Table 6 about here --

Testing for causality: instrumental variables and genetic matching

Despite the strength of the evidence that U.S. aerial bombardment favored Viet Cong control, we must still worry that our initial findings reflect more complex dynamics of violence. On one hand, developments in Viet Cong tactics reflect the mechanisms that we expect to be at play. The failure of the Autumn Campaign prompted COSVN to recommend fewer attacks against US or ARVN installations, and renewed guerilla tactics and agitation to gain control of local territory (COSVN 1969; Davidson 1988:599-601). But on the other hand, it is possible that an unobserved factor or factors both increased the frequency with which some hamlets were bombed and facilitated Viet Cong control in those same hamlets. Likewise, it is possible that bombing increased in places where the incumbents anticipated a future upsurge in insurgent control. In either case, it would be incorrect to attribute the changes in control status in the hamlets that experienced aerial bombing 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 aerial bombing and insurgent control. We have shown that aerial bombing is anything but random, and we can use this very fact to identify its effects on insurgent 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 insurgent control between July and 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 (see e.g. 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. We employ GMM on our entire panel as a robustness test below.

In order to untangle the direction of causality in this fashion, our instruments must satisfy two requirements: excludability and relevance. First, to be valid, our instruments must be "excludable," meaning that they are conditionally independent from the error term in the (unobserved) true regression. In our application, this means that there is no unobserved relationship between insurgent control in June 1969 and insurgent control in December 1969. While there are certainly unobserved characteristics of individual hamlets that jointly determine insurgent control in these two months, we can use data on insurgent control in September 1969 as a control variable to correct for this possibility and 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—it must be the case that variation in insurgent control in July and August 1969 explains variation in aerial bombing in September 1969. We have shown above that this is likely to be the case, but we examine this question again in the context of our instrumental variables models in order to confirm that insurgent control is a relevant instrument for bombing. As we show below with a number of diagnostic tests, insurgent control in July and August is a highly relevant instrument for bombing in September.

We begin by examining the first stage relationship between our instruments and the endogenous variable, insurgent control. Table 7 contains the results from two models, the first a baseline model with no substantive controls, and the second including a series of hamlet- and village-level control variables.

-- Table 7 about here --

The results are the first pieces of evidence that past insurgent control is a good instrument for aerial bombing in September of 1969. All four of the coefficients measuring this relationship are significant at the $p < .01$ level (three of the four meet the more demanding threshold of $p < .001$). 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.[15] These are not formal tests of our instrument's relevance, but they are encouraging first steps in demonstrating that, as we expect, past insurgent control is a good predictor of aerial bombing.

In Table 8, we present the second stage results from seven instrumental variables regressions. The first two models (Models 8A and 8B) are the second stages of the models in Table 7; the next three models include a binary indicator variable for bombing, break apart the measure of insurgent control in September (as above), and include two additional controls for the average level of insurgent control at the village and district levels. The sixth and seventh models (8F and 8G) 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, and also most closely mirrors the use of lagged dependent variables as instruments in labor and housing economics. In Model 8G, 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. Recall that one worry we had about estimating the direction of causality involved the possibility that bombing was systematically directed against hamlets that were *in the process* of moving toward insurgent control. Were this the case, then bombing would reflect the anticipation of changes in control, rather than causing them. We take this possibility explicitly into account in Model 8G.

-- Table 8 about here --

[15] In Model A6, $F(2,10771) = 49.85$. In Model B6, $F(2,9656) = 34.64$.

We find in all models that the effect of bombing on insurgent control is positive, and that this coefficient is very precisely estimated. These results mirror those from the ordered logistic regressions above, with the caveat that since we have modeled the dependent variable as a continuous rather than ordinal variable, coefficients have different substantive interpretations. Curiously, our ordinal index of insurgent control in September appears only weakly related to insurgent control in December. Model 8D shows that this is an effect of assuming a linear functional form, and Model 8F shows that insurgent control in September is a strong predictor of *changes* in control in December. Finally, Model 8G 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 of interest. Note also that the estimated effect of the prior change in insurgent control is insignificant. This suggests that conditional on bombing and other covariates, there is no secular trend towards insurgent control in our data.

While not the primary focus of this paper, 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. Previous work on these data has found strong evidence that development and hamlet population size affected local control; those results are consistent with robust macro-scale evidence that per capita GDP is associated with civil war onset and duration. This analysis undermines the micro-scale findings and suggests that we consider the macro-scale evidence with greater skepticism. Moreover, rather curiously, the effect of average insurgent control in the district in which the hamlet is located is significant and *negative*.

The diagnostics presented in the final four rows of Table 8 allow us to test some of the most important assumptions of our instrumental variables strategy. The LM statistic tests the hypothesis that our instruments predict bombing in September 1969. The high Chi-square test statistics for all of them allow us to reject confidently the null hypothesis that our model is not identified. The Wald F test statistic tests the joint significance of the two excluded instruments in predicting bombing in September to guard against "weak identification," which occurs when instruments do not explain much variation in the endogenous covariate (Murray 2006). When instruments are weak, instrumental variables estimates are biased (Bound et al. 1995). We compare these statistics to the critical values identified by Stock and Yogo (2005) and find that all models well surpass conventional benchmarks, indicating that we are unlikely to suffer from bias due to weak instruments. Finally, we test the overidentifying restrictions of our two instruments using Hansen's J test. Under the null, the instruments are valid ("excludable"); rejecting the null indicates that there is some subset of instruments that is correlated with the error term in the second stage regression. Our test statistics are small, and their *p*-values are far from conventional levels. Together, these specification tests demonstrate that we have identified instruments for bombing that are both excludable and relevant.

Finally, we recognize that our instrumental variables strategy is uncommon in political science, so we exploit the full panel to present a series of robustness tests specifically designed to correct for the unobserved hamlet-level factors that might impugn our cross-sectional instrumental variables identification strategy. We estimate fixed effects regressions, both with and without instrumental variables, and dynamic panel data models. The dependent variable in all models is the *change* in insurgent control. The results appear in Table 9.

-- Table 9 about here --

Model 9A is a fixed effects model that controls for unobserved hamlet-level factors, and uses the lag of bombing to predict subsequent changes in control. Model 9B is an instrumental variables model that uses the lag of the difference between hamlet-level control scores and the village-level mean of insurgent control as an instrument for bombing in the current period. Both models confirm the results from the cross-sectional analysis: controlling for unobserved hamlet-level characteristics, bombing increased insurgent control. Models 9C-9E present results from dynamic panel data models. When lagged dependent variables appear on the right hand side of a panel data model, the lag of the dependent variable is correlated by construction with the fixed effects. Arellano and Bond (1991) propose a GMM estimator to correct for this problem—these models, which use lagged values of the dependent variable as instruments, were the inspiration for our cross-sectional identification strategy above. Model 9D explicitly models bombing as endogenous, and instruments with two-month lags of bombing and insurgent control. Model 9E adds the difference between the mean village control and hamlet-level control as an additional instrument. In all three dynamic models, bombing increases subsequent insurgent control—and the coefficient on the lagged dependent variable is now positive, as it should be. Unfortunately, common specification tests for the Arellano-Bond models are undefined given that the log of distance to an international border is constant across time. Still, we are less concerned with model specification here than with confirming that our estimates hold up to a variety of different estimators. Our results show that they do, even after explicitly controlling for the unobserved hamlet-level heterogeneity that would invalidate our cross-sectional instrumental variables strategy. Using instrumental variables to untangle the direction of causality, we find that aerial bombing increased Viet Cong control.

Strategy Two: Genetic Matching

Matching techniques facilitate the estimation of causal effects using observational data by choosing observations that will 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 we cannot randomly assign hamlets to be bombed, and since we cannot simultaneously observe the effect of bombing and not bombing in the same hamlet,[16] we create a "control" group of hamlets that are as comparable as possible to the hamlets that were "treated" (i.e., bombed). 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. The literature glosses the measure of similarity among treatment and control hamlets as "distance." We then evaluate the effects of bombing on insurgent control using this 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. Ho et al. (2007) caution against "controlling for consequences"—including covariates in the matching or estimation procedures that are intermediate consequences of a treatment variable. Quite naturally, we will mask the effect of bombing in September on insurgent control in December if we control for insurgent control in November. We are careful to match (and estimate) only models where any covariate is not possibly a consequence of bombing in September. We

[16] This is sometimes known as the "fundamental problem of causal inference" (Holland 1986).

therefore use data on hamlet population, village-level development, village-level insurgent control, district-level insurgent control, and hamlet-level insurgent control from June of 1969. Only distance to an international border and rough terrain are unaffected by bombing.

We also check to see whether or not our matching procedure fares adequately in creating a truly balanced matched dataset. Diamond and Sekhon (2005) show that in most common applications, matching based on standard distance metrics (the two most prominent being propensity scores and Mahanalobis distance) can increase bias on some covariates rather than reducing it.[17] They propose a genetic matching algorithm that searches across the space of all distance metrics to achieve balance. We examine balance statistics (available upon request) obtained by matching based on simple "nearest neighbor" propensity score methods as implemented in Ho et al. (2004), and find that balance worsens notably after matching for some covariates. Given this, we employ Diamond and Sekhon's genetic matching procedure. Our results regarding the effect of bombing on insurgent control, however, are nearly identical when using nearest-neighbor matching techniques.

We first present the balance statistics obtained after running the genetic matching algorithm (Table 10). The goal of matching is to minimize the differences between all variables (and their distances) across treatment and control groups.

-- Table 10 about here --

These statistics indicate that balance increases dramatically for nearly every covariate after matching, so that inferences behind models that analyze all data may be misleading. As a

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

simple first step, Ho et al. (2007) recommend comparing means between matched control hamlets and all hamlets across all variables and the distance measure. The smaller the differences between treatment and control hamlets, the greater the balance.[18] The first four columns in Table 10 show that the balance between treatment and control improves markedly after matching—improvement exceeds 99% for most. Ho et al. (2007) prefer to compare summary statistics from empirical quantile-quantile plots—these plots array "the quantiles of a variable of the treatment group against that of the control group in a square plot." Tight clustering around a 45 degree line indicates greater balance. Comparing percent improvement in medians, means, and maxima of deviations from the 45 degree line for all variables and the distance measure, we find great increases in balance after matching. These statistics suggest that the genetic matching algorithm has provided us with data that can approximate the counterfactual question at hand: are hamlets that are bombed in September 1969 subsequently more likely to be controlled by the Viet Cong in December than hamlets that were not bombed?

Using the matched data, we investigate this counterfactual by estimating an ordered logistic regression similar to the ones presented above. The results are in Table 11.

-- Table 11 about here --

The coefficient on bombing is positive and highly 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

[18] There is no metric for what counts as a "small" difference. Cochran (1968) suggests that the differences should be less than a quarter of the standard deviation of the control; our differences are well below this cutoff point for all variables.

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 12, which was generated using the same procedures as Table 6 above. 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.

-- Table 12 about here --

The pattern of estimated probability changes confirms this relationship. The negative changes in probabilities appear in the northeast half of Table 12, 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 than zero. Furthermore, the pattern of substantive changes is sensible, decreasing in all columns as the difference in control from July and December increases. These substantive changes confirm that aerial bombing had dramatic consequences for insurgent control in Vietnam, leading 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 omitted variable bias or measurement error. These are the strengths of instrumental variables regressions such as those shown previously.[19] Matching's strength is in reducing the model-dependence of the counterfactual inferences made about the effect of a treatment of interest on an outcome of

[19] Omitted variables do affect instrumental variables regressions in the event that they are not conditionally independent from the instruments. But this is merely a component requirement of the assumption that instruments are conditionally independent of the error term in the true regression.

interest when data are not generated via randomized experiment. We are interested in the effect of bombing on insurgent control, and matching allows us to make tighter inferences about this quantity by pruning away irrelevant hamlets that were not bombed. Combined with our results from instrumental variables regressions, though, matching helps us to confirm that aerial bombing increased Viet Cong control in Vietnam.

Conclusion

It is appealing to believe that what we think is right is also instrumentally rational, but this is often not the case. Indiscriminate violence, used as a tool to repress and terrorize civilians, is repugnant, yet many governments with the power to inflict it believe that it serves their interests. The data left behind by the Vietnam-era U.S. Government are extraordinary: a systematic and carefully collected record of an atrocity. The irony is that the atrocity appears to have been self-defeating, much as Hannah Arendt thought.

We are not the first to have criticized the air war over South Vietnam, nor the first to have claimed that it was counterproductive. But critics have been unable to marshal systematic evidence about the air war's impact on the South Vietnamese population. In fact, many appear to believe that aerial bombing failed because it was "bound to fail"—it was a pathology of a military apparatus that could not (Gibson 1986) or would not (Krepinevich 1986) understand its counterinsurgency mission. We turn the literature's attention to the victims of the air war, using the most detailed data ever to gauge the effectiveness of the strategy that victimized them. South Vietnamese civilians suffered from aerial bombing, and this bombing harmed the strategic goals of the US and RVN.

Our findings speak to long-running debates about the effectiveness of American strategy in Vietnam. We complement a large body of revisionist scholarship (e.g. Sorley 1999) that holds that the counterinsurgency campaign waged by the United States and allied forces in the wake of the Tet Offensive was more successful than commonly thought. The success was due to the shift away from indiscriminate violence, and towards hamlet-level security. Our analysis covers the period in which American and South Vietnamese forces began to shift their attention to ensuring the physical security of hamlets rather than attempting to crush the Viet Cong through massive firepower. But even in this period of what appears to be growing counterinsurgency success, we find that indiscriminate violence through aerial bombing remained quite prevalent. The Viet Cong's success against counterinsurgency operations was not the result of American timidity that prevented the *expansion* of aerial bombardment (i.e. Warner 1978). This aerial bombardment *itself* hampered the counterinsurgency campaign. More of it would likely have hastened the victory of the Viet Cong.

In a broader context, our results reinforce and extend to the context of counterinsurgency a growing consensus on the limitations of airpower as a coercive instrument. We also help to explain why counterinsurgency remains such a challenging task with a long average duration, even for sophisticated and modern armies. Massive advantages in technology and firepower may confer no advantage whatsoever, and may even reinforce ongoing insurgencies. Methodologically, we point to the need for caution in drawing conclusions from the analysis of aggregated data and case studies.

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Table 1: *Mass Killings and Civil Wars*

		Guerilla War		
		No	Yes	Total
Mass Killings	No	83 (92.22)	51 (68.00)	134 (81.21)
	Yes	7 (7.78)	24 (32.00)	31 (18.79)
	Total	90 (100)	75 (100)	165 (100)

Table 2: *Control and Bombing, July - December 1969*

	Bombed	**Not bombed**	**Total**
High Government Control	2.69% (272)	97.31% (9842)	100.00% (10114)
Moderate Government Control	4.91% (1196)	95.09% (23142)	100.00% (24338)
Contested	8.44% (1352)	91.56% (14661)	100.00% (16013)
Moderate Insurgent Control	14.76% (1527)	85.24% (8820)	100.00% (10347)
High Insurgent Control	37.29% (1431)	62.71% (2407)	100.00% (3838)
Total	9.48% (5778)	90.52% (58872)	100.00% (64650)

$X^2(4) = 0.0012$, Pr = 0.000. "Bombed" = Air attack sortie targeted coordinates within 2000 meter radius of hamlet center.

Table 3: *Negative Binomial Regression on Bomb Sortie Count*

	Model 2A	Factor Δ	Model 2B	Factor Δ	Model 2C	Factor Δ
Moderate Government Control	0.286** (0.086)	1.33	0.303*** (0.086)	1.35	0.176 (0.093)	1.19
Contested	0.713*** (0.120)	2.04	0.727*** (0.120)	2.07	0.425** (0.128)	1.53
Moderate Insurgent Control	1.118*** (0.155)	3.06	1.154*** (0.155)	3.17	0.847*** (0.165)	2.33
High Insurgent Control	1.946*** (0.202)	7.00	1.965*** (0.201)	7.13	1.742*** (0.217)	5.71
Village average control	0.370*** (0.054)	1.45	0.364*** (0.054)	1.44	0.262*** (0.057)	1.30
District average control	0.316*** (0.050)	1.37	0.328 (0.038)	1.39	0.331*** (0.040)	1.39
August			-0.037 (0.062)	0.96	-0.144* (0.068)	0.87
September			-0.556*** (0.064)	0.57	-0.680*** (0.069)	0.51
October			-0.742*** (0.065)	0.48	-0.894*** (0.069)	0.41
November			-0.466*** (0.064)	0.63	-0.746*** (0.068)	0.47
December			-0.454*** (0.064)	0.64	-0.623*** (0.068)	0.54
Development Index (Village level)					-0.473*** (0.038)	0.62
Log of Distance to International Border					-0.433*** (0.025)	0.65
Rough Terrain					0.005** (0.022)	1.005
Log of Hamlet Population					0.073** (0.022)	1.076
Constant	-3.923*** (0.095)	--	-3.618*** (0.102)	--	1.173*** (0.322)	--
N	64650		64650		56134	
Log likelihood	-31036		-30929		-26782	
α	16.51		16.12		15.68	
LR test α = 0	0.0007		0.0007		0.006	
p-value	0.000		0.000		0.000	

*** = $p < .001$, ** = $p < .01$, * = $p < .05$.

Table 4: *The Effects of Bombing in July on Control in December, 1969*

	Control July 1969									
	High Government Control		**Moderate Government Control**		**Contested**		**Moderate Insurgent Control**		**High Insurgent Control**	
	Bombed?		**Bombed?**		**Bombed?**		**Bombed?**		**Bombed?**	
Control December 1969	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**
High Government Control	**77.14% (54)**	**74.74% (1169)**	7.98% (21)	15.71% (591)	0.85% (2)	2.31% (51)	0.00% (0)	0.18% (3)	0.00% (0)	0.51% (2)
Moderate Government Control	21.34% (15)	21.99% (344)	**68.06% (179)**	**67.96% (2556)**	24.15% (57)	37.58% (829)	9.09% (30)	14.74% (244)	0.68% (2)	3.81% (15)
Contested	1.43% (1)	3.13% (49)	19.01% (50)	13.11% (493)	**52.12% (123)**	**46.66% (1015)**	33.64% (111)	38.31% (634)	3.38% (10)	11.93% (47)
Moderate Insurgent Control	0.00% (0)	0.13% (2)	4.94% (13)	3.08% (116)	20.34% (48)	13.69 (302)	**53.65% (177)**	**44.53% (739)**	20.95% (62)	33.76% (133)
High Insurgent Control	0.00% (0)	0.00% (0)	0.00% (0)	0.13% (5)	2.54% (6)	0.41% (9)	3.64% (12)	2.11% (35)	**75.00% (222)**	**50.00% (197)**
Total	100.00% (70)	100.00% (1564)	100.00% (263)	100.00% (3761)	100.00% (236)	100.00% (2206)	100.00 (330)	100.00 (1655)	100.00 (296)	100.00 (394)
$X^2(4)$	0.7869 (prob. = 0.853)		19.0023 (prob. = 0.001)		36.7941 (prob. = 0.000)		16.0687 (prob. = 0.003)		50.3995 (prob. = 0.000)	

"Bombed" = At least one air attack sortie targeted coordinates within a 2000 meter radius of hamlet center during this time period.

Table 5: *Ordered Logit on Control Status in December, 1969*

	Model 4A	Model 4B	Model 4C
Moderate Government	2.75***	1.53***	1.02***
Control	(0.070)	(0.090)	(0.094)
Contested	4.72***	2.43***	1.78***
	(0.083)	(0.132)	(0.135)
Moderate Insurgent	6.17***	3.09***	2.30***
Control	(0.090)	(0.166)	(0.170)
High Insurgent Control	9.41	5.43***	4.39***
	(0.139)	(0.227)	(0.234)
District average control		0.650***	0.70***
	--	(0.040)	(0.059)
Village average control		0.68***	1.31***
	--	(0.058)	(0.043)
Bombed	0.60***	1.21***	0.48***
	(0.063)	(0.040)	(0.067)
Development Index			-0.51***
(Village level)	--	--	(0.041)
Log of Distance to			-0.14***
International Border	--	--	(0.025)
Rough Terrain			0.008***
	--	--	(0.002)
Log of Hamlet			-0.17***
Population	--	--	(0.025)
N	10775	10775	9664
Log-likelihood	-10491	-9753	-8924
Pseudo-R^2	0.322	0.369	0.349

Note: all cutpoints are significant at the p < 0.01 level. *** = $p < .001$, ** = $p < .01$, * = $p < .05$

Table 6: *Bombing and Expected Changes in December 1969 Control Probabilities*

Hamlet Control in December 1969	Hamlet Control in July 1969: High Government Control	Moderate Government Control	Contested	Moderate Insurgent Control	High Insurgent Control
High Government Control	**-0.097**	**-0.033**	**-0.015**	**-0.009**	**-0.001**
Moderate Government Control	**0.049**	**-0.084**	**-0.120**	**-0.111**	**-0.022**
Contested	**0.041**	**0.090**	**0.079**	**0.036**	**-0.085**
Moderate Insurgent Control	**0.006**	**0.026**	**0.053**	**0.078**	**0.054**
High Insurgent Control	**0.000**	**0.001**	**0.003**	**0.006**	**0.053**

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* | bombed = 1, control = *k*) – Pr(Y = *j* | bombed = 0, control = *k*), where *j* indexes control in December, *k* indexes control in July. Estimates for the 95% confidence interval excludes zero are bold. Negative changes in probability are shaded.

Table 7: *First Stage IV Results*

	MODEL 6A	MODEL 6B
INSTRUMENTS		
Index of Insurgent Control (July)	0.154*** (0.032)	0.146*** (0.034)
Index of Insurgent Control (August)	0.119*** (0.033)	0.100** (0.035)
CONTROLS		
Index of Insurgent Control (September)	0.097*** (0.025)	0.108*** (0.026)
Development Index (Village level)		-0.122** (0.039)
Log of Distance to International Border		-0.121*** (0.023)
Rough Terrain		0.012*** (0.002)
Log of Hamlet Population		0.010 (0.023)
Constant	-0.651*** (0.049)	0.578 (0.296)
Shea's Partial R^2	0.009***	0.007***

*** = p < .001, ** = p < .01.

Table 8: *Second Stage IV Results*

	MODEL 7A	MODEL 7B	MODEL 7C	MODEL 7D	MODEL 7E	MODEL 7F	MODEL 7G
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)	1.953***	1.986***		1.850***	0.593***	0.593***	1.966***
	(0.196)	(0.240)		(0.209)	(0.122)	(0.122)	(0.459)
Bombed (binary)			12.004***				
			(1.219)				
Index of Insurgent Control (September)	0.114†	0.084	0.109†		0.033	-0.967***	0.579***
	(0.065)	(0.073)	(0.060)		(0.022)	(0.022)	(0.171)
Δ Control (September to October)							-0.011
							(0.142)
Moderate Government Control				0.567***			
				(0.116)			
Contested				1.130***			
				(0.123)			
Moderate Insurgent Control				1.145***			
				(0.158)			
High Insurgent Control				-0.238			
				(0.351)			
Development Index (Village level)		0.126	0.115	0.140†	0.013	0.013	0.121
		(0.087)	(0.073)	(0.082)	(0.029)	0.029	(0.099)
Log of Distance to International Border		0.210***	0.068†	0.191***	0.059**	0.059**	0.208**
		(0.053)	(0.039)	(0.048)	(0.020)	0.020	(0.068)
Rough Terrain		-0.021***	-0.004	-0.021***	-0.006***	-0.006***	-0.021**
		(0.004)	(0.003)	(0.004)	(0.002)	0.002	(0.006)
Log of Hamlet Population		-0.066	-0.015	-0.078†	-0.024	-0.024	-0.066
		(0.046)	(0.039)	(0.043)	(0.014)	0.014	(0.046)

Second Stage IV Results (continued).

Village average control					0.768***	0.768***	
					(0.037)	0.037	
District average control					-0.051**	-0.051**	
					(0.025)	0.025	
Constant	1.551	-0.116	0.627	-0.221	-0.029	-0.029	-0.093
	(0.125)	(0.612)	(0.500)	(0.593)	(0.190)	0.190	(0.620)
N	10775	9664	9664	9664	9664	9664	9664
LM Statistic	98.824***	68.847***	97.093***	78.871***	25.630***	25.630***	18.531***
Wald F Statistic	49.851***	34.642***	48.999***	39.715***	12.836**	12.836**	9.274**
Hansen's J Statistic	0.031	0.099	0.146	0.519	1.064	1.064	0.215
p-value	0.859	0.753	0.703	0.471	0.302	0.302	0.643

*** = $p < .001$, ** = $p < .01$, * = $p < .05$, † = $p < .1$. For Wald F statistics, *** = $p < .05$ less than 10% IV bias, ** = $p < .05$ less than 15% IV bias.

Table 9: *Robustness Tests and Unobserved Heterogeneity*

	MODEL 9A	MODEL 9B	MODEL 9C	MODEL 9D	MODEL 9E
Bombed (count)[a]	0.006***	0.437***	0.008***	0.023**	0.026**
	(0.001)	(0.175)	(0.002)	(0.007)	(0.008)
Rough Terrain	0.004**	0.003	0.003	0.003	0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Log of Distance to International Border [b]	--	--	0.246***	0.249***	0.242***
			(0.013)	(0.013)	(0.014)
Development Index (Village level)	-0.166***	-0.153***	-0.111***	-0.112***	-0.117***
	(0.019)	(0.037)	(0.023)	(0.023)	(0.024)
Log of Hamlet Population	-0.074***	-0.100**	-0.041†	-0.047*	-0.062**
	(0.016)	(0.033)	(0.022)	(0.022)	(0.022)
Hamlet control (lag)	-1.055***	-1.046***	0.054***	0.051***	0.120***
	(0.005)	(0.011)	(0.010)	(0.010)	(0.010)
N	47198	47039	37486	37486	37486
Model	Fixed Effects	Fixed Effects, IV	Arellano-Bond	Arellano-Bond	Arellano-Bond
Instruments for Bombed (count)	No	Yes	No	Yes	Yes

[a] Bombed (count) is lagged one month in Model 9A. [b] Distance to an international border is constant over time, so cannot be estimated in fixed effects models.
*** = $p < .001$, ** = $p < .01$, * = $p < .05$, † = $p < .1$.

Table 10: *Balance Statistics, Unmatched Versus Matched Data*

	Means				Percent Improvement in Empirical Quantile Measures		
	Unmatched Data		Matched Data	Percent Improvement	Median	Mean	Maximum
	Treated	Control	Control				
Estimated Distance Measure	0.139	0.075	0.139	99.45	76.83	71.26	50.74
Rough Terrain	6.446	5.213	7.303	30.40	0.00	9.68	15.46
Log of Distance to International Border	11.056	11.071	11.064	43.77	60.63	62.06	92.03
Development Index (Village level)	-0.344	-0.082	-0.346	99.05	100.00	78.60	25.00
Log of Hamlet Population	6.570	6.767	6.564	96.65	58.24	48.04	74.38
Hamlet control	3.443	2.586	3.436	99.19	100.00	78.43	50.00
District average control	3.115	2.655	3.116	99.84	89.91	85.40	73.69
Village average control	3.383	2.602	3.380	99.64	84.08	76.36	38.78

Table 11: *Model G9: Bombing and Insurgent Control, Matched Data*

	Coefficient	Standard Error	T Score
Bombed (binary)	0.492***	0.115	4.284
Rough Terrain	0.015***	0.004	3.484
Log of Distance to International Border	-0.256***	0.073	-3.513
Development Index (Village level)	-0.121	0.125	-0.971
Log of Hamlet Population	-0.337***	0.069	-4.853
Hamlet control	1.262***	0.132	9.530
District average control	0.507***	0.120	4.219
Village average control	0.611***	0.150	4.074
Cut Point 1	-2.013**	0.931	-2.162
Cut Point 2	1.822*	0.915	1.99
Cut Point 3	3.945***	0.921	4.283
Cut Point 4	6.419***	0.939	6.834
N	1252		

Ordered logistic regression. *** = $p < .001$, ** = $p < .01$, * = $p < .05$

Table 12: *Bombing and Expected Changes in December 1969 Control Probabilities*

Hamlet Control in December 1969	**Hamlet Control in July 1969**				
	High Government Control	**Moderate Government Control**	**Contested**	**Moderate Insurgent Control**	**High Insurgent Control**
High Government Control	**-0.046**	**-0.015**	**-0.004**	**-0.001**	**-0.000**
Moderate Government Control	-0.022	**-0.102**	**-0.098**	**-0.046**	**-0.016**
Contested	**0.057**	**0.082**	**0.018**	**-0.075**	**-0.069**
Moderate Insurgent Control	**0.011**	**0.031**	**0.073**	**0.086**	-0.002
High Insurgent Control	**0.001**	**0.003**	**0.011**	**0.036**	**0.087**

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* | bombed = 1, control = *k*) – Pr(Y = *j* | bombed = 0, 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.