

State Violence and Wartime Civilian Agency: Evidence from Peru

Livia Isabella Schubiger, Duke University

How civilians respond to political violence profoundly shapes conflict processes and the legacies of civil war. Yet influential patterns of wartime civilian agency remain strikingly unexplored. This study investigates how exposure to state violence influences the organization of ordinary citizens into civil defense forces, a common and consequential type of mobilization that is still poorly understood. I argue that state violence marked by direct and collective targeting promotes community-based armed mobilization through the mechanisms of signaling and the militarization of local governance in irregular civil war. The analysis focuses on the Peruvian armed conflict during the 1980s. Based on an instrumental variable and a difference-in-differences approach, the results suggest that communities victimized by state forces were more likely to rise up against the insurgents at later stages. These counterintuitive findings underscore the relevance and complexity of grassroots collective action during war.

How civilians respond to wartime violence has profound implications for conflict dynamics and postconflict recovery. And yet, the determinants and types of wartime civilian agency remain poorly understood. Previous studies have focused primarily on the choices of noncombatants to flee violence-affected regions in the hope of reaching safer areas (Steele 2009), as well as their strategies of joining, submitting to, or collaborating with state forces or rebel groups (Kalyvas 2006; Petersen 2001; Wood 2003). However, many civilians stay behind in contested areas without aligning with either side and actively try to shield their communities from the turmoils of war. Puzzlingly, sometimes these ordinary citizens—generally poorly armed and trained—rise up against full-blown insurgencies, despite receiving little or no support from the state. Our limited understanding of these choices is striking, given the central assumptions about civilian behavior on which our theories of conflict processes build.

This article investigates grassroots collective action in the form of organized and armed resistance to insurgent groups that is not directed or imposed by the state. Community-based counterinsurgent mobilization often has profound consequences for subsequent dynamics of political violence, the reconfiguration of social networks, and wartime institutional change. Indeed, counterinsurgent actors operating outside the realm or at the margins of state control have dramatically influenced patterns of order and violence in contexts as diverse as Sierra Leone (Humphreys and Weinstein 2008), Iraq (Clayton and Thomson 2014), Mozambique (Jentzsch 2013), and Southern Sudan (Blocq 2014)—to name just a few. Although research has explored the incentives of state actors to outsource violence to affiliated nonstate armed groups (Carey, Mitchell, and Lowe 2013) as well as the incentives of individuals to join insurgent or counterinsurgent armed groups (Humphreys and Weinstein 2008), we know little about

Livia Isabella Schubiger (livia.schubiger@duke.edu) is the Douglas and Ellen Lowey Assistant Professor, Department of Political Science, Duke University, Durham, NC 27708.

This research was supported by the Zurich University Alumni Association, the Fakultäeres Mentoring Program at the University of Zurich, and the Swiss National Science Foundation. It has also benefited from generous research support at the London School of Economics and Political Science and at Duke University. Data and supporting materials necessary to reproduce the numerical results in the article are available in the *JOP* Dataverse (<https://dataverse.harvard.edu/dataverse/jop>). An online appendix with supplementary material is available at <https://doi.org/10.1086/711720>.

Published online June 14, 2021.

The Journal of Politics, volume 83, number 4, October 2021. © 2021 Southern Political Science Association. All rights reserved. Published by The University of Chicago Press for the Southern Political Science Association. <https://doi.org/10.1086/711720>

the determinants of community-driven, autonomous forms of counterinsurgent mobilization in war and why some segments of the civilian population engage in this type of mobilization while others do not. Rather than presenting an overarching theory of this form of community-based mobilization, this study investigates the effect of one particular causal variable: the state-led victimization of civilians in war.

State violence against civilians remains a common feature of armed conflict, and the consequences of this type of violence have been subject to intense scholarly debate. However, most studies on the effects of state-led civilian targeting have focused on its impact on the insurgent side of conflicts, in particular the insurgents' capacity to marshal civilian collaboration, to mobilize followers, and to gather military momentum and strength (e.g., Condra and Shapiro 2012; Goodwin 2001; Kalyvas and Kocher 2007; Kocher, Pepinsky, and Kalyvas 2011; Lyall 2009; Lyall, Blair, and Imai 2013; Mason and Krane 1989; Toft and Zhukov 2015; Wood 2003). As a result, the repertoire of civilian agency remains theoretically truncated in this literature, and the effects of state violence on consequential forms of noncombatant mobilization are left unexplored.

This study finds that exposure to one common type of state violence—the direct and collective targeting of civilians—promotes community-based counterinsurgent collective action in irregular civil war. Based on fine-grained data on the Peruvian armed conflict, the empirical analysis uses an instrumental variable (IV) approach that exploits spatial variation in the counterinsurgency approach of the state to assess the causal effect of state violence. Specifically, the research design exploits the fact that state repression in the mid-1980s in Peru was largely confined—de jure and de facto—to the government-imposed emergency zones, the borders of which were strictly defined by the boundaries of specific administrative units. The results consistently suggest a substantial and positive effect of state violence on counterrevolutionary collective action in Peruvian villages and towns, an effect that is robust to difference-in-differences (DiD) analysis as an alternative empirical strategy, as well as substantial levels of unobserved confounding. Several mechanisms are plausibly underlying this relationship, in particular attempts of victimized communities to demonstrate their nonaffiliation with the insurgents (*signaling*) and the institutionalization of armed governance in the longer run (*militarization of local governance*).

Although the analysis of this study is restricted to one conflict, the findings yield broader implications for the study of collective action in civil war. Specifically, while the results are counterintuitive and striking, and at odds with much of the literature on mobilization in war, the presented finding is compatible with a positive effect of state violence on *pro*-insurgent mobilization (e.g., Goodwin 2001; Wood 2003), a

compatibility that remains obscured if victimized communities are portrayed as homogeneous social groups.¹ To the extent that insurgent and counterinsurgent mobilization are driven by different segments of targeted groups, both dynamics can be affected by state violence at the same time. Moreover, by theorizing mechanisms underlying the militarization of civil society that do not imply communities' allegiance or subordination to either the insurgents or the state, this study also contributes to a better understanding of armed mobilization by ordinary citizens in contexts outside of civil wars where the provision of security by state actors is scarce.

REPRESSION AND MOBILIZATION IN CIVIL WAR

That wartime exposure to state violence could trigger civilian resistance to insurgent groups seems puzzling from a prominent theoretical perspective, given that this type of violence has been shown to mitigate the insurgents' collective action problem (Lichbach 1995; Olson 1971) through several mechanisms. Indiscriminate state violence in particular has been shown to play into the hands of the insurgents by triggering moral outrage (Wood 2003), by forging and reinforcing grievances (Goodwin 2001), by fueling dynamics of revenge (Kalyvas 2006; Wood 2008), and by strengthening the collective identity of the targeted group (Khawaja 1993). When state violence is indiscriminate, insurgents may also be able to enlarge their ranks by signaling their willingness to protect their constituency (Goodwin 2001; Humphreys and Weinstein 2008; Kalyvas and Kocher 2007; Mason and Krane 1989).

Yet, scholars have also argued that indiscriminate state violence may help to turn civilians against the insurgents at the local level—for example, when insurgents are blamed for violence endured at the hand of state forces (Lyall 2009) or when civilians feel forced to defect to the stronger side (Kalyvas 2006). Whether and under what circumstances these dynamics result in *organized* and *armed* resistance, however, remains unclear.

Furthermore, the literature has only paid scant attention to the fact that state violence is often not completely indiscriminate and that even limited forms of screening and profiling might affect behavioral outcomes in significant ways (Gutiérrez-Sanín and Wood 2017; Steele 2009). Specifically, most authors follow Kalyvas (2006) in simply distinguishing between selective (individualized) and indiscriminate (non-individualized) targeting. Often, however, more fine-grained distinctions are needed, as violence observed in armed conflict often affects groups of individuals sharing particular

1. Exploring insurgent mobilization is beyond the scope of this study, as it would require a different framework and measurement strategy.

attributes such as language, geography, or ethnicity in distinct ways. This article focuses on the effects of *collective targeting* as the empirically observed disproportionate exposure of certain segments of the population to violence, without implying this pattern to be extreme or the result of particular intentions, strategies, or orders (Gutiérrez-Sanín and Wood 2017, 24; see also Steele 2009).

Moreover, I consider the interpersonal context of state violence: direct violence is inflicted in face-to-face interactions between perpetrators and victims, while indirect violence is not (Balcells 2010). In this article I show that exposure to direct and imperfect collective targeting—conventionally implicitly subsumed under the category of indiscriminate violence—can foster civilian mobilization against insurgent forces, an effect that is theoretically compatible with a positive effect on insurgent recruitment among the same communities and social groups.

STATE VIOLENCE AND COUNTERINSURGENT COLLECTIVE ACTION

Counterinsurgent collective action refers to the establishment, active participation in, or support of groups and organizations that are not part of the official security forces of a state but are organized in armed self-defense and open resistance against—or protection from—insurgent groups. Counterinsurgent mobilization thus implies the public opposition to insurgent actors and can be state orchestrated or community based (Jentzsch, Kalyvas, and Schubiger 2015). While the incentives for state actors to create, control, and sustain counterinsurgent militias outside the formal security apparatus are numerous (Carey et al. 2013), this article is concerned with the more puzzling phenomenon of *autonomous* or *community-based* mobilization, which refers to processes through which self-defense organizations are established or sustained independently from state actors and through the bottom-up initiative of local residents themselves.

Rather than equating certain forms of wartime mobilization with particular private allegiances, this article assumes that the incentives for ordinary citizens to engage in armed self-defense are strong enough only if there is no alternative actor or institution that guarantees order and security and if alternative forms of protection are foreclosed. In remote rural areas, for instance, the option of migration is typically less available to economically marginalized residents who are heavily dependent on their land and on agricultural forms of production and who are only weakly embedded into geographically dispersed social networks. Similarly, the option of joining highly mobile guerrilla units is generally not only restricted to the young and healthy but also conditional on the recruitment and screening strategies of rebel groups (Kalyvas and Kocher 2007).

While it is beyond the scope of this article to provide an overarching theory of counterinsurgent mobilization, how should we think about the impact of state violence on this type of mobilization in war? Put differently, for those civilians left behind in contested areas, what effect does exposure to state violence have?

First of all, state violence, if perceived as nonselective yet norandom, and imperfectly tied to collective attributes, should significantly increase the incentives for members of victimized communities to engage in armed mobilization against the insurgents as a strategy of signaling, understood as the purposeful display of features or actions with the intention to raise the probability of the receiver interpreting them in a given way (Gambetta 2009, 170). For those segments of the civilian population that fit the profiling scheme of state actors, and for whom options such as flight or joining insurgent forces are foreclosed, counterinsurgent mobilization can be one of the few strategies available to avoid further state violence through the very public and costly display of their nonallegiance to insurgent groups (see also Kalyvas 2006, 167–68; Lyall 2009, 337). Counterinsurgent collective action is thus one way targeted communities may choose to publicly convey their alignment with the stronger side to escape the victim category.

Importantly, not all forms of state violence should trigger counterinsurgent mobilization as a signaling response. The logic of signaling is based on the perceived responsiveness of the perpetrator, that is, the perceived probability that behavioral signals will be received, and that state agents will have sufficient incentives to discriminate between the “guilty” and the “innocent” if provided with the opportunity to update their beliefs. It follows that the public display of civilians’ nonaffiliation with the insurgents will only be pursued as a strategy of self-protection if state violence is, first, collective yet neither genocidal nor otherwise perfectly tied to collective attributes and, second, not exclusively inflicted in indirect attacks. If violence is completely arbitrary or extremely well predicted by identity traits (as in the case of genocidal violence or ethnic cleansing), noncombatants will expect that the perpetrator lacks the willingness to discriminate on the basis of behavioral information. If state violence is exclusively indirect, as in the case of aerial bombings, civilians will anticipate that behavioral signs may simply not be received. However, in many if not most civil wars, state violence—while often based on a mix of selective, collective, and arbitrary targeting—is neither entirely random nor genocidal, nor exclusively indirect, as state agents have incentives to discriminate between rebel supporters and their own potential allies (Kalyvas 2006).

Once initiated, community-based armed mobilization is likely to endure as long as high levels of perceived insecurity persist. Thus, the need to self-protect through signaling might

be regarded as necessary long after state violence subsides. Insurgent retaliation following the public defection of communities to the state may further incentivize communities to stay mobilized and armed and, thus, ready to fight back. Once mobilized, community-based counterinsurgent forces are thus likely to become part of a more comprehensive and more durable process of institutional change oriented toward communities' self-reliant provision of order and security in an environment of high volatility and risk. While the militarization of local governance—"the supplanting of local forms of governance with new forms that reflect the influence of armed actors" (Wood 2008, 550)—is an almost ubiquitous feature of violent intrastate conflicts, previous research has primarily focused on insurgents, state forces, or external and supralocally organized paramilitary groups as providers of order and authority during war (Arjona 2014; Mampilly 2011; Weinstein 2007). However, ordinary citizens may themselves be the primary agents of wartime institutional change, including the reconfiguration of local modes of governance that maximize the capability to self-protect. Armed mobilization in response to wartime violence is likely to promote the longer-term institutionalization of armed collective action by prompting the coordination-intensive reorganization of local life, by reconfiguring traditional institutions, and by transforming the norms, skills, and preferences of those residents left behind in targeted areas toward the autonomous provision of order and security in a context of violent threats. Indeed, numerous studies have found that exposure to wartime violence more generally shapes local norms and patterns of collective action in the long run, potentially by fostering in-group cooperation (Bauer et al. 2016), enhanced activism in the wake of victimization (Wood 2003), and the transformation of local institutions (Wood 2008). Bateson (2013), for example, shows how exposure to wartime violence in Guatemala durably transformed threat perceptions, norms, and institutions in affected communities, explaining the widespread contemporary policing of communities by civil patrols—decades after the civil war came to an end. In summary, I argue that state violence marked by direct and collective targeting will promote counterinsurgent mobilization through the mechanism of signaling, and this effect is likely to persist through the militarization of local institutions in a context of violent threats.

Notably, however, none of these mechanisms require the existence of private loyalties or allegiances to the state, instead being theoretically compatible with preference falsification (Kuran 1995) among victimized groups. Moreover, insurgent and counterinsurgent mobilization may well be fostered simultaneously by state violence even at the local level, such as when some individuals join insurgent columns in a bid to increase their safety, and others migrate to nontargeted areas,

while for yet other community members both of these options are foreclosed.

Of course, the proposed impact of state violence is subject to external constraints. In particular, insurgents should be able to crush, hinder, or prevent counterinsurgent mobilization where they manage to maintain high levels of local territorial control. The outlined mechanisms should therefore be most pervasive under conditions of irregular civil war marked by steep military asymmetries between state forces and insurgent groups (Kalyvas and Balcells 2010). Under these conditions, insurgent territorial control will—at least temporarily—be disrupted as a result of direct state violence, as insurgents will try to avoid head-on confrontations with militarily superior state troops. In other words, in irregular civil wars, direct state violence should weaken insurgent territorial control even where it previously was predominant or complete (Kalyvas 2006). The scope conditions of the signaling mechanism are additionally limited to areas where state-society links are weak for linguistic, cultural, institutional, or other reasons and where intelligence is poor as a result, even if incumbent territorial control is high. Moreover, state violence should trigger counterinsurgent mobilization primarily under conditions of high uncertainty—conditions that also imply an absence of incentives for preemptive mobilization. Put differently, it is under conditions where incumbent targeting reveals information about a community's vulnerability and risk of further victimization that reactive signaling should occur. This also implies that state violence need not be extreme in intensity to trigger signaling as a response.

In summary, the theoretical expectation is that exposure to state violence in the form of direct collective targeting will increase the propensity of communities to engage in counterinsurgent mobilization in irregular civil war, an effect that should persist once this type of state violence ends. The empirical test of this expectation is based on a subnational study of the first decade of the armed conflict between the insurgent group Shining Path and the Peruvian government that ravaged the country from the early 1980s through the first half of the 1990s. The Peruvian armed conflict provides an ideal setting to test the implications of the proposed argument, as it classifies as an irregular war and displays wide variation in exposure to the theorized form of state violence over space and time.

EMPIRICAL CONTEXT: THE PERUVIAN CIVIL WAR

The revolutionary armed group Partido Comunista del Perú-Sendero Luminoso (henceforth Sendero Luminoso, or Shining Path) launched its armed struggle against the Peruvian government in May 1980 in the department of Ayacucho, at a time when Peru was returning to civilian rule after more than a decade of military government. The years of armed conflict,

insurgent terror, and state repression that followed caused immense human suffering. It has been estimated that 69,280 people died in Peru as a result of political violence in the 1980s and 1990s (Ball et al. 2003).

Throughout the conflict, the counterinsurgency strategy of the Peruvian government and the armed forces underwent several major transformations. In particular, the first decade of the war saw three distinct counterinsurgency approaches sequentially adopted by the state (CVR 2003, 1:58–66, 8:49–52; see also vol. 4).² The first period encompasses the time between the initiation of armed struggle by Sendero Luminoso in May 1980 and the launch of the state’s counterinsurgency campaign in the emergency zones in December 1982. Underestimating the growing size and influence of the armed movement in the country’s remote highlands, the administration of Fernando Belaúnde at first tried to fight the rebels mainly by relying on police forces. By the end of 1982, however, it had become obvious that the police had no reasonable chance of success against the elusive insurgents, which used guerrilla tactics and did not wear uniforms. The government decided to fight the rebels by military means and launched a fierce counterinsurgency campaign, which marks the onset of the second period (CVR 2003, 1:58–66; see also vol. 4). In December 1982, the armed forces were sent into the designated emergency zones, which were placed under political-military command. Most of the state forces’ gravest human rights violations fall into this period, which roughly spans the years 1983, 1984, and 1985. The armed forces lacked adequate intelligence and links to the civilian population (e.g., Coronel 1996; CVR 2003, 8:49–52; Degregori 1998). Officers and soldiers deployed to fight the insurgency in the predominantly indigenous highlands were heavily drawn from the primarily Spanish-speaking population in Lima and other coastal cities (Degregori 1998, 146; Tapia 1997, 31). While this strategy aimed to prevent subversive infiltration of the armed forces, the fact that most of the deployed soldiers did not speak the dominant language of the local population in the suspected insurgent strongholds had disastrous consequences for the quality of intelligence (CVR 2003, 8:101–4; Degregori 1998, 141, 146). A statement by Adrián Huamán Centeno, chief commander in Ayacucho in 1984, illustrates the quandary of the armed forces: “We were used to conventional warfare, that is, to know where the enemy is. . . . But in this case, no. Where is the enemy? Invisible. Whom are we going to attack? The manual of revolutionary and counterrevolutionary warfare has been available to all members of the armed forces since the 1960s, but in this

specific case, the difficulty for those who do not speak Quechua is that it is another culture, so it is difficult to communicate with people—with the *indio* that constitutes the revolutionary force” (Adrián Huamán Centeno, quoted in CVR 2003, 8:102; author translation).

Civilians bore the bulk of the violence, as the armed forces were unable to effectively distinguish between insurgents and the ordinary population and often operated on the basis of the assumption that Sendero Luminoso primarily recruited people from more indigenous backgrounds (e.g., Coronel 1996; CVR 2003, 8:101–4; Del Pino 1996). As one of the most prominent experts covering the conflict writes, “the Armed Forces were blind, or, rather, color-blind. . . . When they saw dark skin, they fired” (Degregori 1998, 143–44).

State violence was mainly inflicted directly, through massacres, kidnappings, torture, extrajudicial executions, and “disappearances” (as opposed to indirect strategies such as aerial bombings). In November 1984, many dozens of unarmed men, women, and children died at the hands of the armed forces in the infamous massacre of Putis. Because of insurgent activities in the area, they had been classified as terrorist supporters (CVR 2003, 7:95–102). State violence did not affect a majority of villages, nor did it always result in high numbers of casualties. Yet just a few villagers being targeted or “disappeared” revealed information about the vulnerability of one’s own community to state suspicion and violence. Often, individuals were taken into detention, allegedly accused of terrorism or of collaborating with the insurgents, but never sentenced, released, or seen by their families again, such as 20-year-old Antonio from the community of Ucmay in May 1984,³ who was taken into custody by members of the navy while working at a market with his wife, or 24-year-old teacher Maria from San Jose de Secce,⁴ who was taken away by soldiers in the middle of the night in December 1983. While both state forces and the insurgents—themselves notoriously brutal—committed a wide range of human rights abuses during that time, violence perpetrated by the rebels during this early phase of the war was more selective because of their superior access to local information (e.g., Degregori 1998).⁵ At the same time, the insurgents were clearly outmatched by the state in military strength. Moreover, the Senderistas seemed neither willing nor capable of protecting the population from state violence. Instead, when communities were attacked, Sendero cadres usually retreated to the mountains (e.g., Degregori 1998, 141; Fumerton 2001, 482, 484;

2. See also Coronel (1996). References to the report of the Peruvian Truth and Reconciliation Commission (Comisión de la Verdad y Reconciliación; CVR) are based on the digital book version (CVR 2003).

3. Testimony 205284 to the CVR; name changed.

4. Testimony 200337 to the CVR; name changed.

5. Insurgent targeting became increasingly indiscriminate during later periods of the war.

2002, 114; Isbell 1992, 90; McClintock 1989, 90; Weinstein 2007, 191–92). Security was not among the “goods” that the rebels could provide, not even for their alleged civilian allies (e.g., Weinstein 2007).

The state’s counterinsurgency approach changed again in the wake of Alan García assuming office mid-1985. The ensuing period (1986–88) was characterized by strategic transformations and strained relations between the political leadership and the armed forces. García aimed to radically reorient the struggle against the insurgents and at least partially succeeded in curbing human rights abuses (Fumerton 2002). However, while levels of state violence against civilians were reduced, so were the general efforts of the armed forces, which now tended toward passivity in many places. Communities were often left to their own devices, for better or worse (e.g., Del Pino 1996, 149; Fumerton 2002, 98–102, 120–23; García-Godos 2006, 152–56; 2008, 69).

Throughout the whole conflict, some communities engaged in open and armed resistance to the insurgents, forming civil defense groups that became known as *rondas campesinas* or *comités de autodefensa* (subsequently also *rondas* or *committees*). Qualitative evidence consistently points to three principal functions of these community-based forces: resistance against rebel violence and insurgent rule (e.g., Degregori 1998; Fumerton 2002; La Serna 2012), protection from state suspicion (e.g., Fumerton 2002; Weinstein 2007), and community governance (e.g., Fumerton 2001; García-Godos 2006).

Indeed, qualitative case descriptions support the notion that the civil defense forces were not only a response to insurgent intrusion and rebel violence but also an attempt to avoid victimization by state troops (Fumerton 2002, 117–18; García-Godos 2006, 273; 2008, 69; Weinstein 2007, 248, 250). “Regardless of their loyalties,” writes Weinstein (2007, 250) on the residents of the *zonas altas* in Ayacucho, “active resistance [to the rebels] was the only way . . . to avoid death at the hands of the government forces.” Typically, the *rondas* organized fellow villagers into self-defense activities such as local patrols and lookout posts. To varying degrees, the committees started to expand their roles and assumed governing functions. García-Godos (2006, 125–26, 128, 151, 273), for instance, argues for the case of the district of Tambo that the local militias’ functions were as much geared toward the provision of community governance as toward the protection from state repression and rebel incursions. In some communities, they became the “organizing principle of everyday life” (García-Godos 2008, 69). Some committees also pursued offensive activities and engaged in human rights violations themselves (CVR 2003; Fumerton 2002; García-Godos 2006). While the self-defense committees would eventually be legally recognized and officially supported by the state in the 1990s, this study

focuses on the period before 1989. During this period, the relations between state actors and the *rondas* were ambivalent at best, despite the public alignment of the latter against Shining Path (e.g., Fumerton 2002).

Regarding the impact of exposure to state violence on counterinsurgent mobilization in Peru, qualitative work from anthropologists and historians yields diverging conclusions. Some studies imply that the campaign of 1983–85 delayed civilian resistance against the insurgents (Degregori 1998, 141–42; Starn 1995, 552)—who themselves proved to be increasingly abusive and indiscriminate against civilians as the war progressed—while others suggest the opposite (Fumerton 2002, 113–14, but see 92).

RESEARCH DESIGN

What effect did exposure to state violence during the counterinsurgency campaign of 1983–85 have on counterinsurgent mobilization in the subsequent period (1986–88)? The core challenge to answering this question lies in the fact that even though state violence was highly unpredictable during the counterinsurgency campaign of 1983–85, targeting did not occur at random, thus being potentially related to other important determinants of communities’ propensity for counterinsurgent collective action. This study exploits the fact that state violence during the 1980s in Peru was largely confined to the emergency zones, the borders of which were set by administrative boundaries, leading to differences in exposure to state violence that were largely exogenous to the dynamics of the conflict in certain regions.

The emergency zones were imposed via legal decrees, which were issued by the government in Lima. Because of their legal character, they were explicitly restricted to particular districts and provinces. Thus, while the emergency zones were nonrandomly assigned, their boundaries were entirely determined by the borders of administrative units. The official major determinant of the emergency zone assignment was terrorist activity, although the government’s and the armed forces’ assessment of where these activities were most severe did not always coincide (e.g., CPHEP 2010, 133). Nevertheless, the repression campaign of the Peruvian armed forces 1983–85 was de jure least constrained, and de facto most active, in these particular administrative units. Within the emergency zones, civil liberties were severely restricted, and the population was at the mercy of the armed forces, which were officially in charge of restoring the internal order (CVR 2003). Outside the emergency zones, the armed forces did not have the same responsibilities and freedoms, and the presence of state forces as well as the risk of state-led civilian victimization was dramatically lower. By contrast, the operations of the insurgents clearly were not confined to these boundaries (e.g., Tapia 1997,

58–59; see also Fumerton 2002, 110–11). To quote the leader of a counterinsurgent *ronda* in the Apurímac river valley, where the emergency zone border coincided with a river: “This was our worst dilemma, the biggest problem, as we would say: that some areas were under emergency and others were not. Because in the Ayacucho emergency zone we were deprived of all our rights. As everyone knows, there were massacres by the Navy, the Police, the Army. However, close from here was an area not under emergency, and this is where the *Senderistas* took refuge. . . . Sendero could just cross the river to be in a liberated zone, not in the emergency area” (Huillca 1993, 44; author translation).

In short, the institutional setup of the emergency zones led to a relationship between geographic location and exposure to state violence that often appeared very haphazard on the ground, especially within the neighborhoods of these boundaries. To study the effect of state violence, I hence rely on an IV approach in which the location of a community inside or outside the emergency zone serves as an instrument for exposure to state violence. In standard geographic natural experiments and regression discontinuity designs, a nonparametric approach (local regression or local randomization) and restriction of the analysis to units within a very narrow bandwidth around the boundary is ideal (Keele and Titiunik 2016). However, this is not always possible because of data restrictions (Angrist and Pischke 2009). In this case, variation in violence exposure is limited, especially given that conditional ignorability has to be taken into account. The analysis here hence broadens the study region of interest to a bandwidth of 25–75 kilometers on each side of the border and relies on a parametric approach, approximating a standard IV design. The quantity of interest, the local average treatment effect (LATE),⁶ is estimated via a two-stage least squares (2SLS) approach (Hahn, Todd, and Van der Klaauw 2001; Imbens and Lemieux 2010; Lee and Lemieux 2010). Omitting covariates and border-segment fixed effects (described below), the endogenous regressor SV_i in (1) is replaced with fitted values from the first stage (2). This leads to the second stage (3). Also of interest is the reduced form (4).

$$Y_i = \alpha_1 + \beta_1 SV_i + \varepsilon_{1i}, \quad (1)$$

$$SV_i = \alpha_2 + \beta_2 \text{Zone}_i + \varepsilon_{2i}, \quad (2)$$

$$Y_i = \alpha_3 + \beta_3 \widehat{SV}_i + \varepsilon_{3i}, \quad (3)$$

$$Y_i = \alpha_4 + \beta_4 \text{Zone}_i + \varepsilon_{4i}, \quad (4)$$

6. LATE is the ratio of the reduced form to the first stage: $[E(Y_i | \text{Zone}_i = 1) - E(Y_i | \text{Zone}_i = 0)] / [E(SV_i | \text{Zone}_i = 1) - E(SV_i | \text{Zone}_i = 0)]$.

where Zone_i represents the location of a village inside or outside the emergency zone (0/1), Y_i the binary outcome, and SV_i exposure to state violence (0/1). The subscript i refers to *centros poblados*.

Despite the parametric approach, this strategy shares some key assumptions with a geographic natural experiment (Keele and Titiunik 2016) and the fuzzy regression discontinuity design (Hahn et al. 2001; Van der Klaauw 2002). Namely, it assumes the absence of precise precampaign sorting around the emergency zone boundary and the negligibility of compounding through the overlap with administrative boundaries. Moreover, it relies on the standard IV assumptions of monotonicity, instrument relevance, exclusion restriction, and (conditional) ignorability (Angrist and Pischke 2009). The no-sorting assumption is violated if units self-select into either side of the border in a manner that is pervasive and precise and driven by factors correlated with the outcome (Keele and Titiunik 2016; Lee and Lemieux 2010). This seems not a serious concern in the Peruvian case. Destination choices were typically driven by economic and social factors (e.g., Degregori 1998, 151; Del Pino 1996, 164). Moreover, it is unlikely that local residents were able to anticipate the boundaries of the emergency zones and whether, when, and where they would change over time. A density test is presented in the appendix (available online) and supports this notion. Monotonicity, too, is plausible, as military rule did not apply outside the emergency zones. Confounding, however, needs to be considered an issue because of imbalances arising from the nonrandom assignment of the emergency zone borders. *Conditional* ignorability is often invoked, and more credible than unconditional ignorability, in geographic designs (Keele and Titiunik 2016). The IV approach is hence combined with covariate adjustment, subset analysis, and genetic matching (Diamond and Sekhon 2013). Moreover, Rosenbaum sensitivity analysis is performed on the intention-to-treat (ITT) effect estimate to assess the robustness of the findings to unobserved confounding (Keele 2010; Rosenbaum 2002). While the exclusion restriction seems credible judging from data inspections and qualitative accounts (discussed below), violations cannot be completely ruled out. Likewise, compounding remains a concern. A related question is whether assignment around the emergency zone boundary occurs at the village level, as assumed here and in similar natural experiments (Keele and Titiunik 2015).⁷ Thus, a DiD approach is applied to check

7. The appendix reports results from an ITT analysis that relaxes this assumption and from an IV analysis with standard errors clustered by district. Both analyses support a positive effect of state violence. In the latter case, however, the instrument gets weak, with weak-instrument robust inference still supporting a positive confidence interval for the 75 km bandwidth.

for the robustness of the presented findings to an alternative empirical strategy. This latter strategy is based on a set of distinct identification assumptions.⁸ Most importantly, it does not rely on an exclusion restriction, and it also erases concerns about compounding. It does, however, introduce alternative assumptions. Most notably, it assumes that the development over time would have been the same or followed parallel trends for targeted and untargeted communities had no exposure to violence occurred. A series of robustness checks are presented in the appendix to probe the sensitivity of the results.

DATA AND MEASUREMENT

The units of analysis are *centros poblados*, that is, settlements of various sizes and types, such as villages and towns (DNCE 2004). The instrument, Emergency Zone, is a dummy variable indicating whether a given village was located inside or outside the emergency zone in 1983–85. Provinces and districts under emergency and political-military command were coded as listed in DESCO (1989, 347–51) and as specified in the *decretos supremos* republished in the *Normas legales: Revista de Legislación y Jurisprudencia* accessed in the Archivo del Congreso de la República in Lima. The analysis focuses on the region around the outer northeastern border of the emergency zone, where the boundary of the emergency zone was stable over time during the period of interest and where a clear comparison between affected and unaffected units is possible as a result.⁹ Distance measures the Euclidean distance from a given village to the nearest point on the emergency zone boundary. It is normalized to zero at the border; that is, it takes on negative values for *centros poblados* outside the emergency zone.

The outcome variable, *Autodefensa* Mobilization, is a dummy variable indicating whether a given *centro poblado* was affected by violence against or perpetrated by self-defense committees in the period after the counterinsurgency campaign (1986–88). By focusing on *autodefensa* mobilization in the period after the counterinsurgency campaign of 1983–85, the focus is placed on bottom-up mobilization, as during that period state-imposed mobilization was largely absent.¹⁰ State Violence is a dummy variable indicating whether a given *centro*

poblado was affected by violence perpetrated by agents of the state during the counterinsurgency campaign of 1983–85.

Autodefensa Mobilization and State Violence are coded based on georeferenced data on violent events (killing, disappearance, detention, forced recruitment, kidnapping, assassination, battle-related death, extrajudicial execution, torture, and sexual violence) provided by the Peruvian CVR. In addition to its final report (CVR 2003), the CVR compiled several data sets based on more than 15,000 individual testimonies and over 400 public hearings, as well as existing data collected by various human rights organizations. While the data compiled by the CVR are of exceptional detail and quality, and while they do not suffer from the typical biases that affect media-based data on political violence, the data collection procedures were vulnerable to self-selection and underreporting (Ball et al. 2003). Moreover, not all information could be geocoded at the *centro poblado* level, which creates additional missing data problems. In order to minimize the risk of inflating these biases and for theoretical reasons—specifically, the assumption that the effect of violence is not a direct function of its intensity—this study combines information from the different data sets (see the appendix) and works with binary variables of violence exposure, rather than focusing on the intensity of violence. In other words, if at least one victim or violent event was recorded in a given period and village, the CVR-based variables are coded 1 and 0 otherwise.

COVARIATES

Despite the locally haphazard assignment of the emergency zone borders, confounding may still be an issue, as the instrument is nonrandomly assigned. As outlined above, the government's main rationale to assign emergency zones was to curb insurgent violence. Prior Insurgent Violence is a binary variable of whether a community was subject to insurgent violence before the onset of the counterinsurgency campaign. This variable is based on the same sources as *Autodefensa* Mobilization and State Violence. I also include a variable, Prior Insurgent Presence, that identifies suspected pockets of strong insurgent presence and initial civilian support for the insurgents by the end of 1982. This variable is coded at the district level and based on Noel (1989), a former general who commanded an infantry division in the emergency zones. According to this source, Sendero Luminoso had allegedly established a presence in more than two dozen districts in the departments of Ayacucho, Huancavelica, and Apurímac by the end of 1982 (Noel 1989, 26; cited in Tapia 1997, 34). Another important determinant of both state repression and subsequent patterns of mobilization, in Peru and elsewhere, is the capacity of state agents and insurgent groups to control territory. In insurgencies, insurgent control is typically concentrated in remote rural

8. Both empirical strategies rely on the *stable unit treatment value assumption* (Rubin 1980, 591; 1986, 961). Diffusion processes associated with counterinsurgent mobilization are documented mainly for the period after 1988 (Fumerton 2002), which is not under study here.

9. The appendix shows a map of the relevant subregion.

10. Ideally, mobilization could be measured directly, as not all *rondas* were engaged in, or affected by, violence. This type of undercounting would be particularly problematic if *autodefensa* mobilization was more or less likely associated with violence in targeted as opposed to nontargeted villages. To mitigate this concern, the outcome variable includes all recorded types of violence and *rondas* as victims as well as perpetrators.

areas where coercive state power is difficult to project. Therefore, the distance of each *centro poblado* to the nearest provincial capital (Distance to Province Capital) serves as a measure of the capacity for territorial control of insurgent as opposed to state forces. Finally, a precampaign outcome measure is included (Prior Mobilization).

RESULTS

The first-stage, reduced-form, and second-stage results from 2SLS regressions are reported in table 1 for bandwidths ranging from 25 to 75 kilometers on each side of the emergency zone border. All specifications include fixed effects at the level of four border segments (Dell 2010), which are defined by the intersection of province and emergency zone boundaries. This makes sure that villages that are compared to each other are located next to similar portions of the boundary. Each *centro poblado* is assigned to the segment of its nearest point on the emergency zone boundary.

The first-stage results, reported in the first panel of table 1, indicate a much higher risk of exposure to state violence for units located inside the emergency zone compared to those located outside of it—about a 100% increase over the average probability of being victimized. The multivariate *F*-test for the relevance of the excluded instrument passes the conventional threshold of 10 for all specifications and bandwidths, ranging from 12 to 27.¹¹ The reduced-form or ITT estimates represent the overall effect of the instrument on the dependent variable and are reported in the second panel. The results suggest a significantly higher propensity for counterinsurgent mobilization on behalf of communities located inside compared to those outside the emergency zone. Specifically, the ITT estimates indicate a 1.2–2.6 percentage point change in the probability of counterinsurgent mobilization (mean outcome, 75 km: 0.006). The second-stage results are presented in the third panel, showing the impact of state violence, once instrumented by the location of a village inside or outside the emergency zone. The IV estimates indicate a positive effect of state violence on counterinsurgent mobilization. The estimated LATE is substantively large, indicating a 26–38 percentage point change in the probability of counterinsurgent mobilization.¹²

Threats to inference and robustness tests

Despite the results reported above, several concerns remain. First, the dummy variable approach might raise questions

when it comes to the measurement of state and insurgent violence. There are two justifications for this approach, one empirical and one theoretical. Empirically, a count variable would likely aggravate biases that arise from self-selection and underreporting. For example, a higher record of victims may be the result of a higher propensity to testify to the truth commission in some communities, rather than a true indicator of the magnitude of violence. Theoretically, the hypothesized effect does not presume a high intensity of violence or large massacres in most affected communities. Even small numbers of victims can have a large effect according to the argument developed in this article, especially in predominantly rural contexts. If collective targeting is imperfect, then even one or a few victims can send strong signals regarding which particular communities are under threat. Nevertheless, it is important to rule out that the presented results are an artifact of the way violence has been measured. I hence include several alternative measures (reported in the appendix) for both state and insurgent violence, taking into account that killings and disappearances might have different effects than nonlethal forms of violence and that the Peruvian CVR had data collection criteria distinct from other human rights organizations present during the war. The findings remain substantially unchanged.

Second, there was some top-down mobilization during 1983–85, that is, state-led efforts to organize ordinary citizens into counterinsurgent groups. In the qualitative and historical literature, there is a strong indication that the overwhelming majority of the state-imposed civil defense groups were deactivated as soon as military pressure and presence decreased in the second half of the 1980s (Degregori 1996; Fumerton 2002; García-Godos 2006). Overall, these early state-orchestrated attempts to form *rondas* failed, and the strategy of top-down mobilization was subsequently abandoned (McClintock 1999, 236) for several years. Nevertheless, it is important to rule out that the identified effect is driven by communities organized into *autodefensa* units by state actors during the counterinsurgency campaign. Top-down mobilization could affect subsequent autonomous mobilization, directly, by affecting the local capacity for armed collective action and, indirectly, as local groups collaborating with the government may affect the capacity of state actors for selective targeting (Lyal 2010) or for creating a perception of the same (Kalyvas 2006). Robustness checks in the appendix indicate that the reported effects are stable if the state violence variable exclusively captures instances in villages where military forces operated on their own and where there are no indications of joint operations. This also partially addresses the concern that state activities other than direct violence could be influencing the outcome—a concern addressed again below.

11. Reported is the Kleibergen-Paap rk Wald *F*-statistic.

12. Ordinary least squares estimates are shown in the appendix.

Table 1. State Violence and Autodefensa Mobilization: Two-Stage Least Squares

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	First Stage (DV: Exposure to State Violence)											
Emergency Zone	.048*** (.009)	.044*** (.011)	.056*** (.011)	.055*** (.014)	.073*** (.016)	.072*** (.019)	.038*** (.009)	.034*** (.010)	.045*** (.011)	.050*** (.013)	.061*** (.015)	.065*** (.018)
Distance to Emergency Zone Border	.000 (.000)	.000 (.000)	-.000+ (.000)	-.000+ (.000)	-.000** (.000)	-.000* (.000)	-.000 (.000)	-.000 (.000)	-.000+ (.000)	-.000* (.000)	-.000** (.000)	-.000* (.000)
Prior Mobilization							.199 (.349)	.191 (.351)	.193 (.351)	.183 (.347)	.550*** (.164)	.759*** (.035)
Prior Insurgent Presence							.051*** (.013)	.056*** (.015)	.034* (.016)	.028 (.020)	.030 (.025)	-.003 (.041)
Distance to Province Capital							.000 (.000)	.000 (.000)	.000 (.000)	-.000* (.000)	-.000 (.000)	-.000* (.000)
Prior Insurgent Violence							.425*** (.047)	.428*** (.050)	.444*** (.060)	.432*** (.065)	.499*** (.071)	.467*** (.077)
	Reduced Form (DV: Autodefensa Mobilization)											
Emergency Zone	.015*** (.005)	.016** (.005)	.019** (.006)	.021** (.007)	.022* (.009)	.026* (.010)	.012** (.004)	.013** (.005)	.014** (.005)	.016* (.007)	.016* (.008)	.018* (.009)
Distance to Emergency Zone Border	-.000** (.000)	-.000** (.000)	-.000** (.000)	-.000** (.000)	-.000* (.000)	-.000* (.000)	-.000** (.000)	-.000** (.000)	-.000** (.000)	-.000* (.000)	-.000* (.000)	-.000* (.000)
Prior Mobilization							-.083** (.028)	-.092** (.031)	-.097** (.035)	-.103** (.037)	-.090** (.031)	-.076*** (.022)
Prior Insurgent Presence							.015** (.006)	.018** (.007)	.020* (.008)	.022* (.010)	.037* (.015)	.045+ (.025)
Distance to Province Capital							.000** (.000)	.000** (.000)	.000* (.000)	.000+ (.000)	.000+ (.000)	.000* (.000)
Prior Insurgent Violence							.083** (.028)	.095** (.031)	.097* (.038)	.099* (.044)	.088+ (.049)	.078 (.052)

Second Stage (DV: *Autodefensa* Mobilization)

State Violence ^a	.310** (.097)	.371** (.129)	.328** (.106)	.372** (.139)	.302* (.120)	.365* (.148)	.319** (.117)	.380* (.160)	.310** (.120)	.327* (.140)	.263* (.129)	.281* (.135)
Distance to Emergency Zone Border	-.000** (.000)	-.000* (.000)	-.000** (.000)	-.000* (.000)	-.000 (.000)	-.000 (.000)	-.000* (.000)	-.000* (.000)	-.000** (.000)	-.000* (.000)	-.000+ (.000)	-.000+ (.000)
Prior Mobilization							-.146 (.102)	-.164 (.122)	-.157 (.098)	-.163 (.104)	-.235** (.085)	-.289** (.112)
Prior Insurgent Presence							-.001 (.008)	-.003 (.011)	.009 (.009)	.013 (.011)	.029+ (.015)	.046+ (.025)
Distance to Province Capital							.000** (.000)	.000** (.000)	.000* (.000)	.000* (.000)	.000* (.000)	.000* (.000)
Prior Insurgent Violence							-.052 (.058)	-.068 (.079)	-.040 (.066)	-.042 (.073)	-.043 (.078)	-.053 (.078)
BS fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth (km)	75	65	55	45	35	25	75	65	55	45	35	25
N	7,295	6,383	5,276	4,243	3,335	2,419	7,295	6,383	5,276	4,243	3,335	2,419
F-statistic (first stage)	26.967	17.623	24.604	15.869	21.059	13.942	18.899	11.704	18.028	14.548	16.805	12.671

Note. Robust standard errors in parentheses. DV = dependent variable; BS = border segment.

^a Exposure to state violence instrumented by location inside/outside emergency zone.

+ $p < .1$.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

Third, a relevant historical factor to consider is the Mita system. The emergency zone boundary substantially yet imperfectly overlaps with the historical Mita boundary (map shown in the appendix), a forced labor system in effect between 1573 and 1812 in Bolivia and Peru, which has been found to have long-lasting effects on socioeconomic outcomes such as poverty (Dell 2010). A robustness test thus controls for the location of a village inside or outside the Mita boundary. While the instrument is somewhat weaker for two of the chosen bandwidths, and while for the smallest two bandwidths the significance threshold of 0.05 is just barely missed, the substantive conclusion holds (see the appendix). Finally, the results also remain stable if limited information maximum likelihood estimation is performed instead of 2SLS.

Nevertheless, as with all approaches that rely at least partially on the selection on observables assumption, an important question is whether unobserved confounders could undermine the credibility of the reported results. What if additional factors left out of the analysis affected both the allocation of state repression and the local propensity for counterinsurgent mobilization? Given the haphazard yet nonrandom assignment of the instrument, this remains a valid concern. A method for sensitivity analysis proposed by Rosenbaum (2002) allows assessing the sensitivity of the reported results based on randomization inference. Specifically, Rosenbaum sensitivity analysis estimates the extent to which the data would have to be affected by unobserved confounding for the reported effects to disappear. As the approach is matching based for observational studies, table 2 reports the results of an ITT analysis based on genetic matching (Diamond and Sekhon 2013; Mebane and Sekhon 2011; Sekhon 2011). The table also shows the results of the sensitivity analysis with regard to the statistical significance of the reported finding.

Matching is performed on Prior Insurgent Violence, Prior Insurgent Presence, Distance to Province Capital, Mita, and Prior Mobilization. Column 2 in table 2 repeats the analysis restricted to regions where the indicator for Prior Insurgent Presence is zero. This is necessary to balance this variable, which could only be measured at the district level. This subset analysis also allows evaluating the competing explanation that it was insurgent presence that motivated communities to mobilize against them. Again, the results indicate a positive and significant ITT effect for subsequent periods. The relevant sensitivity parameter is Γ , and it can be interpreted as the size of the effect an almost perfect predictor of the dependent variable would have to exhibit on the odds of a village being exposed to the emergency zones in order to eliminate the reported results (Keele 2010; Rosenbaum 2002). The Γ values in table 2 show that an unobserved, nearly perfect predictor of counterinsurgent mobilization would have to change the odds of a village

Table 2. ITT Genetic Matching and Sensitivity Analysis

	(1)	(2)
ATT (ITT matched)	.011***	.007**
Abadie-Imbens SE	.003	.002
<i>t</i>	4.14	3.22
<i>N</i> (original, total)	7,295	6,498
<i>N</i> (original, treated [ITT])	3,616	2,819
Highest Γ (McNemar's test < .05) ^a	21.8	17.1
Bandwidth (km)	75	75
Replacement	No	Yes

Note. Impact of location inside/outside the emergency zone on counterinsurgent mobilization, with 1:1 genetic matching (population size = 10,000). Column 1 reports results for full set without replacement. Column 2 reports results for regions where Prior Insurgent Presence equals 0, with replacement. Matching is performed on Prior Insurgent Violence, Prior Insurgent Presence (only col. 1), Distance to Province Capital, Mita, and Prior Mobilization. ATT = average treatment effect on the treated; ITT = intention to treat.

^a Highest value at which the McNemar's test still turns out significant ($p < .05$).

* $p < .05$.

** $p < .01$.

*** $p < .001$.

being assigned to the emergency zones by a factor of 17. This is an unusually large value indicating an exceptionally high level of robustness to hidden bias. Typical values in the social sciences range from 1 to 2 (Keele 2010), while in his sensitivity analysis of a large-scale study on the relationship between smoking and death from lung cancer (Hammond 1964), Rosenbaum (2002, 112–14) finds a Γ of 6.

A final concern relates to the exclusion restriction. It is violated if the emergency zones had an effect on *autodefensa* mobilization other than through exposure to state violence. The existing qualitative evidence, as outlined above, suggests very limited interactions between soldiers and civilians apart from violence during the treatment period. The robustness of the results to omitted variable bias and to tests that take top-down mobilization into account further increases the confidence that this assumption holds. Nevertheless, it remains an assumption, the validity of which cannot be proven. Moreover, the border-segment fixed effects approach cannot rule out that the treatment effect is compounded through the overlap of the emergency zone boundaries with administrative ones. Hence, an alternative identification strategy is pursued that overcomes these concerns. In this latter empirical approach (reported below), identification does not rely on geographic boundaries, and the exclusion restriction is irrelevant as well.

Alternative identification

This section reports the estimated average effect of state violence on *autodefensa* mobilization based on a DiD approach

with two time periods (corresponding to the conflict phases before and after the 1983–85 counterinsurgency campaign). Here, the study region, while partially overlapping with the one relevant for the IV approach, is larger and covers the three departments most heavily affected by political violence during the first years of the internal armed conflict, namely, Ayacucho, Huancavelica, and Apurímac (map shown in the appendix).¹³ In the regression framework, the time indicator $Period_t$ assumes the value 1 for the period after the counterinsurgency campaign and 0 otherwise. The coefficient on the interaction term for the state violence and period indicator is the effect estimate of interest (ATT):¹⁴

$$Y_{it} = \alpha + \beta(SV_i \times Period_t) + \gamma SV_i + \delta Period_t + \varepsilon_{it}. \quad (5)$$

As there is only one pretreatment period, pretreatment trends cannot be explored in detail (see the appendix). Thus, the DiD strategy is combined with propensity score screening to restrict the analysis to units with comparable treatment propensities (Angrist and Pischke 2009; Crump et al. 2009; Rosenbaum and Rubin 1983).¹⁵ Table 3 shows the results for all *centros poblados* inside the region of common support, the latter being defined by the propensity score using the same confounding variables as reported above.¹⁶ Shown are the results with standard errors clustered at the village (cols. 1 and 3) and district level (cols. 2 and 4), as well as with and without the same covariates used to obtain the propensity score. State violence has a positive and substantial effect on subsequent counterinsurgent mobilization in all specifications.

The appendix presents an inspection of differences in precampaign mobilization and several robustness tests, including an alternative strategy that adjusts for past outcomes. The results remain substantially unchanged.

CAVEATS AND AVENUES FOR FUTURE RESEARCH

While this article is, to my knowledge, the first to identify the effect of state-led collective targeting on counterinsurgent mobilization in war, several issues are left for future research to address. First, future work should empirically explore the validity of the theorized causal mechanisms, which was beyond the scope of this article. Second, while the type of state violence has been inferred through the distinct time periods

Table 3. Difference-in-Differences (Ordinary Least Squares): *Autodefensa* Mobilization

	(1)	(2)	(3)	(4)
Period	.001*** (.000)	.001** (.000)	.001*** (.000)	.001** (.000)
Period × State Violence	.047*** (.010)	.047** (.015)	.047*** (.010)	.047** (.015)
State Violence	.004 (.003)	.004 (.003)	-.003 (.003)	-.003 (.003)
Prior Insurgent Presence			.002 (.001)	.002 (.002)
Initial Military Rule			.003*** (.001)	.003*** (.001)
Prior Insurgent Violence			.026* (.011)	.026** (.009)
Distance to Province Capital			.000*** (.000)	.000 (.000)
Constant	.000 (.000)	.000 (.000)	-.003*** (.001)	-.003* (.001)
R ²	.029	.029	.035	.035
Clusters	11,958	285	11,958	285

Note. Clustered standard errors in parentheses. $N = 23,916$.

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

and actor strategies that characterized the Peruvian conflict, ideally the type of targeting could be measured in more direct and more fine-grained ways. The same holds true for the nature and location of insurgent strongholds. Third, *autodefensa* mobilization could ideally be measured more precisely, as the current measure likely undercounts the number of villages with *autodefensa* mobilization because of its focus on violent events. More fine-grained measures would also allow for an exploration of outliers and heterogeneous effects. Future research should explore the validity of the presented findings with more direct measures of the outcome as additional data become available. Finally, while I have speculated that the presented effect is heterogeneous and compatible with a parallel rise in insurgent recruitment at the local level—as individuals joining insurgent armed groups likely have a profile distinct from those engaging in counterinsurgent mobilization—this conjecture could not be tested in this study, as relevant individual-level measures or high-quality data on insurgent recruitment in Peru do not exist. In short, future work could build on this study and further investigate the effects of

13. The results hold if the DiD analysis is repeated in the same study region relevant for the IV analysis.

14. A more detailed exposition is provided in the appendix.

15. Out of 12,336 units, 378 “untreated” ones are outside the region of common support.

16. Note that, in this design and study region, emergency status is not an instrument and only measured at the very outset of the counterinsurgency campaign (Initial Military Rule).

state violence for community-based mobilization, by scrutinizing the presented findings with additional data and by rigorously examining the causal mechanisms in depth, both for the Peruvian case and beyond.

DISCUSSION AND CONCLUSION

Based on a study of targeted and spared communities in Peru, this article finds that exposure to state violence increased the probability that communities would autonomously rise up against the insurgents. This finding is consistent with the proposed theoretical argument, which holds that state violence in the form of direct and collective targeting promotes the armed mobilization of ordinary citizens against insurgent groups in irregular civil war.

By identifying the effect of state violence on counterinsurgent mobilization at the community level, this study provides novel insight into the impact of state-led civilian victimization on subsequent conflict dynamics and contributes to the growing body of research on the consequences of wartime violence for local collective action and institutional change. While more research is needed to empirically probe the theorized causal mechanisms, the results resonate with recent research suggesting that wartime victimization can translate into an increased local capacity for collective action, an effect that has been traced to the strengthening of prosocial norms in communities (Gilligan, Pasquale, and Samii 2014) as well as transformed preferences at the individual level (e.g., Voors et al. 2012), developments that may well be in-group orientated rather than inclusive (Bauer et al. 2016). Moving beyond these important findings and insights, this study sheds light on the actual behavioral choices of civilians while conflict wears on.

Importantly, the presented argument and results do not imply that collective state violence against civilians is an “effective tool” of counterinsurgency, not even for actors who might be willing to shrug off the enormous human costs of such strategies. Counterinsurgent mobilization does not imply any form of private loyalty or allegiance to the state. Indeed, and despite the fact that resistant civilians might help to suppress insurgent activity in the short term, civil militias are often difficult to control and develop their own agendas. Moreover, the militarization of civil society is very difficult to reverse, potentially undermining democratic principles and the rule of law for decades to come (Bateson 2013). The presented findings are also compatible with existing evidence of a positive impact of indiscriminate or collective state violence against civilians on the mobilization capacity of rebel groups (e.g., Goodwin 2001; Wood 2003). The civil war literature has tended to overlook that state violence may incite insurgent and counterinsurgent collective action simultaneously, partially because of the canonical conceptualization of victimized civilians as homoge-

neous social groups. However, these processes can—and often do—occur in parallel at the local level. This implies not only that state violence against noncombatants is morally wrong and counterproductive but that it may also have deeply polarizing and socially disruptive effects at the local level. Moreover, it highlights the requirement to study wartime mobilization in reaction to violence with consideration of the diversity of individuals it affects.

In sum, the presented findings point to the need to scrutinize and test the myriad of assumptions about grassroots collective action that underlie current studies of political violence and to pay close attention to forms of mobilization that do not fit neatly into dichotomous conceptualizations of civil wars. Moreover, the article elucidates how wartime violence shapes collective action in ways that are likely to reconfigure local networks and institutions, and to leave lasting marks on social and political life, long after civil wars end.

ACKNOWLEDGMENTS

I am very grateful to Lars-Erik Cederman, Stathis Kalyvas, Luke Keele, Dieter Ruloff, and Elisabeth Wood for excellent advice at various stages of this project. Previous versions of this article were presented at the OCV Yale speaker series, the Conference on Militias at Yale University, the MPSA and APSA meetings, the ENCoRe Istanbul meeting, the Annual International Conference on Development Economics and Policy, the Uppsala Department of Peace and Conflict Research, the University of Rochester, and the University of Konstanz. I thank the participants of these events for helpful comments. Christian Davenport, Mario Fumerton, John Griffin, Fabrizio Gilardi, Kalle Moene, Hillel Soifer, Abbey Steele, and Julian Wucherpfennig also provided very valuable feedback. I thank David Sulmont for his supervision at PUCP, Juan-Carlos Guerrero, Gustavo Gorriti, Daniel Manrique, Ponciano del Pino, Ronald Schmidt, and Orin Starn for answering my questions, as well as Gabriel Salazar Borja, Valeria Zarama Duque, Paolo André Rivas Legua, Gabby Levy, Christian Mueller, Emily Myers, Cesar Cornejo Roman, Diego Jose Romero, Marta Talevi, and Ximena Minan Vega for their excellent research assistance. Finally, I thank four anonymous reviewers and the editors for their constructive guidance and feedback.

REFERENCES

- Angrist, Joshua D., and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ: Princeton University Press.
- Arjona, Ana. 2014. “Wartime Institutions: A Research Agenda.” *Journal of Conflict Resolution* 58 (8): 1360–89.
- Balcells, Laia. 2010. “Rivalry and Revenge: Violence against Civilians in Conventional Civil Wars.” *International Studies Quarterly* 54 (2): 291–313.

- Ball, Patrick, Jana Asher, David Sulmont, and Daniel Manrique. 2003. "How Many Peruvians Have Died? An Estimate of the Total Number of Victims Killed or Disappeared in the Armed Internal Conflict between 1980 and 2000." Report. American Association for the Advancement of Science, Washington, DC.
- Bateson, Regina. 2013. "Order and Violence in Postwar Guatemala." PhD diss., Yale University.
- Bauer, Michal, Christopher Blattman, Julie Chytilová, Joseph Henrich, Edward Miguel, and Tamar Mitts. 2016. "Can War Foster Cooperation?" NBER Working paper no. 22312, National Bureau of Economic Research, Cambridge, MA.
- Blocq, Daniel S. 2014. "The Grassroots Nature of Counterinsurgent Tribal Militia Formation: The Case of the Fertit in Southern Sudan, 1985–1989." *Journal of Eastern African Studies* 8 (4): 710–24.
- Carey, Sabine, Neil J. Mitchell, and Will Lowe. 2013. "States, the Security Sector and the Monopoly of Violence: A New Database on Pro-government Militias." *Journal of Peace Research* 50 (2): 249–58.
- Clayton, Govinda, and Andrew Thomson. 2014. "The Enemy of My Enemy Is My Friend. . . . The Dynamics of Self-Defense Forces in Irregular War: The Case of the Sons of Iraq." *Studies in Conflict and Terrorism* 37 (11): 920–35.
- Condra, Luke N., and Jacob N. Shapiro. 2012. "Who Takes the Blame? The Strategic Effects of Collateral Damage." *American Journal of Political Science* 56 (1): 167–87.
- Coronel, José. 1996. "Violencia política y repuestas campesinas en Huanta." In C. I. Degregori, J. Coronel, P. del Pino, and O. Starn, eds., *Las rondas campesinas y la derrota de Sendero Luminoso*. Estudios de la Sociedad Rural 15. Lima: Instituto de Estudios Peruanos, 29–116.
- CPHEP (Comisión Permanente de Historia del Ejército del Perú). 2010. *En Honor a la Verdad: Version del Ejército sobre su participación en la defensa del sistema democrático contra las organizaciones terroristas*. Lima: CPHEP.
- Crump, Richard, Joseph Hotz, Guido Imbens, and Oscar Mitnik. 2009. "Dealing with Limited Overlap in Estimation of Average Treatment Effects." *Biometrika* 96 (1): 187–99.
- CVR (Comisión de la Verdad y Reconciliación). 2003. "Informe Final." Lima: CVR.
- Degregori, Carlos Iván. 1996. "Ayacucho, después de la violencia." In C. I. Degregori, J. Coronel, P. del Pino, and O. Starn, eds., *Las rondas campesinas y la derrota de Sendero Luminoso*. Estudios de la Sociedad Rural 15. Lima: Instituto de Estudios Peruanos.
- Degregori, Carlos Iván. 1998. "Harvesting Storms: Peasant Rondas and the Defeat of Sendero Luminoso in Ayacucho." In Steve J. Stern, ed., *Shining and Other Paths: War and Society in Peru, 1980–1995*. Durham, NC: Duke University Press.
- Dell, Melissa. 2010. "The Persistent Effects of Peru's Mining Mita." *Econometrica* 78 (6): 1863–903.
- Del Pino, Ponciano. 1996. "Tiempos de guerra y de dioses: Ronderos, evangélicos y senderistas en el valle del río Apurímac." In C. I. Degregori, J. Coronel, P. del Pino, and O. Starn, eds., *Las rondas campesinas y la derrota de Sendero Luminoso*. Estudios de la Sociedad Rural 15. Lima: Instituto de Estudios Peruanos.
- DESCO (Centro de Estudios y Promoción del Desarrollo). 1989. *Violencia política en el Perú, 1980–1988*, vol. 1. Lima: DESCO.
- Diamond, Alexis, and Jasjeet S. Sekhon. 2013. "Genetic Matching for Estimating Causal Effects: A General Multivariate Matching Method for Achieving Balance in Observational Studies." *Review of Economics and Statistics* 95 (3): 932–45.
- DNCE (Dirección Nacional de Censos y Encuestas). 2004. "Marco Conceptual de Precenso: Censos Nacionales de Población y Vivienda." DNCE, Lima.
- Fumerton, Mario. 2001. "Rondas Campesinas in the Peruvian Civil War: Peasant Self-Defence Organisations in Ayacucho." *Bulletin of Latin American Studies* 20 (4): 470–97.
- Fumerton, Mario. 2002. "From Victims to Heroes: Peasant Counter-rebellion and Civil War in Ayacucho." PhD diss., University of Utrecht.
- Gambetta, Diego. 2009. "Signaling." In Peter Hedstroem and Peter Bearman, eds., *The Oxford Handbook of Analytical Sociology*. Oxford: Oxford University Press.
- García-Godos, Jemima. 2006. "Citizenship, Conflict and Reconstruction: A Case-Study of the Effects of Armed Conflict on Peasant-State Relations in Tambo, Peru." PhD diss., University of Oslo.
- García-Godos, Jemima. 2008. "Victim Reparations in the Peruvian Truth Commission and the Challenge of Historical Interpretation." *International Journal of Transitional Justice* 2:63–82.
- Gilligan, Michael J., Benjamin J. Pasquale, and Cyrus Samii. 2014. "Civil War and Social Cohesion: Lab-in-the-Field Evidence from Nepal." *American Journal of Political Science* 58 (3): 604–19.
- Goodwin, Jeff. 2001. *No Other Way Out: States and Revolutionary Movements, 1945–1991*. Cambridge: Cambridge University Press.
- Gutiérrez-Sanín, Francisco, and Elisabeth Jean Wood. 2017. "What Should We Mean by 'Pattern of Political Violence'? Repertoire, Targeting, Frequency, and Technique." *Perspectives on Politics* 15 (1): 20–41.
- Hahn, Jinyong, Petra Todd, and Wilbert Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69 (1): 201–9.
- Hammond, E. Cuyler. 1964. "Smoking in Relation to Mortality and Morbidity: Findings in First Thirty-Four Months of Follow-Up in a Prospective Study Started in 1959." *Journal of the National Cancer Institute* 32 (5): 1161–88.
- Huillca, Hugo. 1993. "El diálogo con Sendero es imposible." In O. Starn, ed., *Hablan con Ronderos: La búsqueda por la paz en los Andes*. Lima: Instituto de Estudios Peruanos, 44–45.
- Humphreys, Macartan, and Jeremy Weinstein. 2008. "Who Fights? The Determinants of Participation in Civil War." *American Journal of Political Science* 52 (2): 436–55.
- Imbens, Guido W., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48:281–355.
- Isbell, Billie Jean. 1992. "Shining Path and Peasant Responses in Rural Ayacucho." In David Scott Palmer, ed., *The Shining Path of Peru*. New York: St. Martin's.
- Jentsch, Corinna. 2013. "'Sharpen Your Hoes and Picks': Peasant Mobilization for Self-Defense during Mozambique's Post-independence War, 1976–1992." Prepared for the Order, Conflict, and Violence Workshop, April 9.
- Jentsch, Corinna, Stathis N. Kalyvas, and Livia Isabella Schubiger. 2015. "Militias in Civil War." *Journal of Conflict Resolution* 59 (5): 755–69.
- Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War*. Cambridge: Cambridge University Press.
- Kalyvas, Stathis N., and Laia Balcels. 2010. "International System and Technologies of Rebellion: How the End of the Cold War Shaped Internal Conflict." *American Political Science Review* 104 (3): 415–29.
- Kalyvas, Stathis N., and Matthew Adam Kocher. 2007. "How 'Free' Is Free Riding in Civil Wars? Violence, Insurgency, and the Collective Action Problem." *World Politics* 59 (2): 177–216.
- Keele, Luke. 2010. "An Overview of Rbounds: An R Package for Rosenbaum Bounds Sensitivity Analysis with Matched Data." Unpublished manuscript.
- Keele, Luke, and Rocio Titiunik. 2015. "Geographic Boundaries as Regression Discontinuities." *Political Analysis* 23 (1): 127–55.
- Keele, Luke, and Rocio Titiunik. 2016. "Natural Experiments Based on Geography." *Political Science Research Methods* 4 (1): 65–95.

- Khawaja, Marwan. 1993. "Repression and Popular Collective Action: Evidence from the West Bank." *Sociological Forum* 8:47–71.
- Kocher, Matthew Adam, Thomas B. Pepinsky, and Stathis N. Kalyvas. 2011. "Aerial Bombing and Counterinsurgency in the Vietnam War." *American Journal of Political Science* 55:201–18.
- Kuran, Timur. 1995. *Private Truths, Public Lies*. Cambridge, MA: Harvard University Press.
- La Serna, Miguel. 2012. *The Corner of the Living: Ayacucho on the Eve of the Shining Path Insurgency*. Chapel Hill: University of North Carolina Press.
- Lee, David S., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48:281–355.
- Lichbach, Mark I. 1995. *The Rebel's Dilemma*. Ann Arbor: University of Michigan Press.
- Lyll, Jason. 2009. "Does Indiscriminate Violence Incite Insurgent Attacks? Evidence from Chechnya." *Journal of Conflict Resolution* 53 (3): 331–62.
- Lyll, Jason. 2010. "Are Co-ethnics More Effective Counter-insurgents? Evidence from the Second Chechen War." *American Political Science Review* 104 (1): 1–20.
- Lyll, Jason, Graeme Blair, and Kosuke Imai. 2013. "Explaining Support for Combatants during Wartime: A Survey Experiment in Afghanistan." *American Political Science Review* 107 (4): 679–705.
- Mampilly, Zachariah Cherian. 2011. *Rebel Rulers: Insurgent Governance and Civilian Life during War*. Ithaca, NY: Cornell University Press.
- Mason, T. David, and Dale A. Krane. 1989. "The Political Economy of Death Squads: Toward a Theory of the Impact of State-Sanctioned Terror." *International Studies Quarterly* 33:175–98.
- McClintock, Cynthia. 1989. "Peru's Sendero Luminoso Rebellion: Origins and Trajectory." In S. Eckstein, ed., *Power and Popular Protest*. Berkeley: University of California Press.
- McClintock, Cynthia. 1999. "The Decimation of Peru's Sendero Luminoso." In Cynthia J. Arnson, ed., *Comparative Peace Processes in Latin America*. Washington, DC: Woodrow Wilson Center, 223–50.
- Mebane, Walter, and Jasjeet S. Sekhon. 2011. "Genetic Optimization Using Derivatives: The rgenoud package for R." *Journal of Statistical Software* 42 (11): 1–26.
- Noel, Clemente. 1989. *Ayacucho: Testimonio de un Soldado*. Lima: Publinor.
- Olson, Mancur. 1971. *The Logic of Collective Action*. Cambridge, MA: Harvard University Press.
- Petersen, Roger D. 2001. *Resistance and Rebellion: Lessons from Eastern Europe*. Cambridge: Cambridge University Press.
- Rosenbaum, Paul R. 2002. *Observational Studies*. 2nd ed. Berlin: Springer.
- Rosenbaum, Paul R., and Donald B. Rubin. 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika* 70 (1): 41–55.
- Rubin, Donald B. 1980. "Comment on 'Randomization Analysis of Experimental Data in the Fisher Randomization Test' by D. Basu." *Journal of the American Statistical Association* 75 (371): 591–93.
- Rubin, Donald B. 1986. "Which Ifs Have Causal Answers?" *Journal of the American Statistical Association* 81:961–62.
- Sekhon, Jasjeet S. 2011. "Multivariate and Propensity Score Matching Software with Automated Balance Optimization: The Matching Package for R." *Journal of Statistical Software* 42 (7): 1–52.
- Starn, Orin. 1995. "To Revolt against the Revolution: War and Resistance in Peru's Andes." *Cultural Anthropology* 10 (4): 547–80.
- Steele, Abbey. 2009. "Seeking Safety: Avoiding Displacement and Choosing Destinations in Civil Wars." *Journal of Peace Research* 46:419–30.
- Tapia, Carlos. 1997. *Las Fuerzas Armadas y Sendero Luminoso: Dos Estrategias y un Final*. Lima: Instituto de Estudios Peruanos.
- Toft, Monica Duffy, and Yuri M. Zhukov. 2015. "Islamists and Nationalists: Rebel Motivation and Counterinsurgency in Russia's North Caucasus." *American Political Science Review* 109 (2): 222–38.
- Van der Klaauw, Wilbur. 2002. "Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach." *International Economic Review* 43 (4): 1249–87.
- Voors, Maarten J., Eleonora E. M. Nillesen, Philip Verwimp, Erwin H. Bulte, Robert Lensink, and Daan P. Van Soest. 2012. "Violent Conflict and Behavior: A Field Experiment in Burundi." *American Economic Review* 102 (2): 941–64.
- Weinstein, Jeremy M. 2007. *Inside Rebellion: The Politics of Insurgent Violence*. Cambridge: Cambridge University Press.
- Wood, Elisabeth Jean. 2003. *Insurgent Collective Action and Civil War in El Salvador*. Cambridge: Cambridge University Press.
- Wood, Elisabeth Jean. 2008. "The Social Processes of Civil War: The Wartime Transformation of Social Networks." *Annual Review of Political Science* 11:539–61.