State Violence and Wartime Civilian Agency: Evidence from Peru

Livia Isabella Schubiger

Department of Political Science, Duke University. livia.schubiger@duke.edu

Abstract

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 diversity of grassroots collective action during war.

Keywords: State Violence, Civil War, Civilians, Counterinsurgency, Civilian Victimization

Supplementary materials are available in the online appendix. Replication files are available in the JOP Data Archive on the Dataverse. This research was supported by the Zurich University Alumni Association, the 'Fakultaeres Mentoring' Program at UZH, and the Swiss National Science Foundation. It has also benefited from generous research support at the LSE and at Duke.

How civilians respond to wartime violence has profound implications for conflict dynamics and post conflict 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 (Petersen, 2001; Wood, 2003; Kalyvas, 2006). 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 countries 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. Though research has explored the incentives of state actors to outsource violence to affiliated non-state 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 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 con-

sequences 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., Mason and Krane, 1989; Goodwin, 2001; Wood, 2003; Kalyvas and Kocher, 2007; Lyall, 2009; Kocher, Pepinsky and Kalyvas, 2011; Condra and Shapiro, 2012; Lyall, Blair and Imai, 2013; Toft and Zhukov, 2015). 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 *counter*insurgent 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 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 non-affiliation 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 mo-

bilization (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 often been shown to mitigate the insurgents' collective action problem (Olson, 1971; Lichbach, 1995) 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 (Mason and Krane, 1989; Goodwin, 2001; Kalyvas and Kocher, 2007; Humphreys and Weinstein, 2008).

On the other hand, 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.

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

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 impact behavioral outcomes in significant ways (Steele, 2009; Gutiérrez-Sanín and Wood, 2017). Specifically, most authors follow Kalyvas (2006, 141ff.) 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 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 being extreme or the result of particular intentions, strategies, or orders (Gutiíerrez-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 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, Mitchell and Lowe, 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 shall 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 non-selective yet non-random, and imperfectly tied to collective attributes, should significantly increase the incentives for 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 non-allegiance to insurgent groups (see also Lyall, 2009, 337, and Kalyvas 2006, 167f.). 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' non-affiliation 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, that it is 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 based on 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 towards 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 intra-state conflicts, previous research has primarily focused on insurgents, state forces, or external and supra-locally organized paramilitary groups as providers of order and authority during war

(Weinstein, 2007; Mampilly, 2011; Arjona, 2014). 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 re-organization of local life, by reconfiguring traditional institutions, and by transforming the norms, skills, and preferences of those residents left behind in targeted areas. 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, Blattman, Chytilová, Henrich, Miguel and Mitts, 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 will promote counterinsurgent collective action through the mechanism of signaling, an effect that might persist through the militarization of local institutions and the transformation of local preferences towards the autonomous provision of order and security in a context of violent threats.

Notably, however, none of these mechanisms requires 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, others migrate to non-targeted 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. Peru 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* (PCP-SL, henceforth *Sendero Luminoso* or Shining Path) launched its armed struggle against the Pe-

ruvian 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 about 70,000 people died in Peru as a result of political violence in the 1980s and 1990s (Ball, Asher, Sulmont and Manrique, 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 (Comisión de la Verdad y Reconciliación, 2003, tomo I, 59ff.; tomo IV, 34ff.; tomo VIII, 245ff.).[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 (Comisión de la Verdad y Reconciliación, 2003, tomo I, 59ff.; tomo IV, 34ff.): 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 of 1983, 1984, and 1985. The armed forces lacked adequate intelligence and links to the civilian population (e.g., Coronel, 1996; Degregori, 1998; Comisión de la Verdad y Reconciliación, 2003, tomo VIII). 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 (Tapia, 1997: 31; Degregori 1998, 146). While this strategy aimed to prevent subversive

[2]See also Coronel (1996). References to the report of the Comisión de la Verdad y Reconciliación (2003) are based on the digital book version.

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 (Degregori, 1998, 141, 146; Comisión de la Verdad y Reconciliación, part VIII, chapter 2). 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: Comisión de la Verdad y Reconciliación, part VIII, chapter 2, 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 based on the assumption that *Sendero Luminoso* primarily recruited people from more indigenous backgrounds (e.g., Coronel, 1996; Del Pino, 1996; Comisión de la Verdad y Reconciliación, 2003, part VIII, chapter 2). As one of the most prominent experts covering the conflict writes,

> "the Armed Forces were blind, or, rather, color-blind. (...) [W]hen they saw dark skin, they fired" (Degregori, 1998, 143f.).

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. Due to insurgent activities in the area, they had been classified as terrorist supporters (Comisión de la Verdad y Reconciliación, 2003, part VII, chapter 7). 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 due to 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., McClintock, 1989, 90; Isbell, 1992, 90; Degregori 1998, 141; Fumerton, 2001, 482, 484; Fumerton, 2002, 114; Weinstein, 2007, 191f.). 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 towards passivity. Communities were largely left to their own devices, for better or worse (e.g., Del Pino, 1996, 149; García-Godos, 2008, 69; García-Godos, 2006, 152ff., Fumerton, 2002, 98ff.; 120ff.).

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

[3]Testimony 205284 to the Truth and Reconciliation commission; name changed.

[4]Testimony 200337 to the Truth and Reconciliation commission; name changed.

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

violence and insurgent rule (e.g., Degregori, 1998; La Serna 2012; Fumerton, 2002), protection from state suspicion (e.g., Weinstein, 2007; Fumerton, 2002), 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 (Weinstein, 2007, 248, 250; Fumerton, 2002, 117f.; García-Godos, 2008, 69; 2006, 273). "Regardless of their loyalties," writes for example 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, 125f., 128, 151, 273) for instance argues for the case of the district of Tambo that the local militias' functions were as much geared towards the provision of community governance as towards 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 (Fumerton, 2002; Comisión de la Verdad y Reconciliación, 2003; 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 prior to 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, 141f.; 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, 113f. – but see Fumerton 2002, 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 (1986-1988)? 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 degrees, which were issued by the government in Lima. Due to their legal character, they were explicitly restricted to particular districts and provinces. Thus, while the emergency zones were non-randomly 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., Comisión Permanente de Historia del Ejército del Perú, 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 (Comisión de la Verdad y Reconciliación, 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, 58f., see also Fumerton, 2002, 110f.). 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 instrumental variable 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 non-parametric 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 due to 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 instrumental variables design.[6] The quantity of interest, the local average treatment effect (LATE) is estimated via a 2SLS approach (Hahn, Todd and Van der Klaauw, 2001; Imbens and Lemieux, 2010; Lee and Lemieux, 2010). Omitting covariates, 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).

$$Y_i = \alpha_1 + \beta_1 SV_i + \epsilon_{1i} \tag{1}$$

[6]The effect of interest is the impact of state violence SV_i on counterinsurgent collective action Y_i, exploiting the haphazard placement of communities inside or outside the emergency zone $Zone_i$: $\frac{E[Y_i|Zone_i=1]-E[Y_i|Zone_i=0]}{E[SV_i|Zone_i=1]-E[SV_i|Zone_i=0]}$.

$$SV_i = \delta + \gamma Zone_i + \eta_i \tag{2}$$

$$Y_i = \alpha_2 + \beta_2 \widehat{SV}_i + \epsilon_{2i} \tag{3}$$

$$Y_i = \kappa + \lambda Zone_i + \mu_i \tag{4}$$

$Zone_i$ 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, Todd and Van der Klaauw, 2001; Van der Klaauw, 2002). Namely, it assumes the absence of precise pre-campaign sorting around the emergency zone boundary and the negligibility of compounding through the overlap with administrative boundaries. Moreover, it relies on the standard instrumental variable 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 (Lee and Lemieux, 2010; Keele and Titiunik, 2016). This seems not a serious concern in the Peruvian case. Destination choices were typically driven by economic and social factors (e.g., Del Pino, 1996, 164; Degregori, 1998, 151). 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 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 due to imbalances arising from the non-random assignment of the emergency zone borders. *Conditional* ignorability is often invoked, and more credible, 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 effect to assess the robustness of the findings to unobserved confounding (Rosenbaum, 2002; Keele, 2010). 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 difference-in-differences approach is applied to check 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.

[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.

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

Data and Measurement

The units of analysis are *centros poblados*, i.e., settlements of various sizes and types, such as villages and towns (Dirección Nacional de Censos y Encuestas, 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, 345ff.) and as specified in the *decretos supremos* re-published in the *'Normas legales - Revista de Legislación y Jurisprudencia'* (Editorial Normas Legales S.A., N.d.) 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 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, i.e., 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 1983-1985, 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

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

[10]Ideally, mobilization could be measured directly, as not all *rondas* might have been 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 non-targeted villages. To mitigate this concern, the outcome variable includes all recorded types of violence, and *rondas* as victims as well as perpetrators.

of 1983-85 or not.

Data on *Autodefensa Mobilization* and *State Violence* are based on geo-referenced data on violent events (killing, disappearance, detention, forced recruitment, kidnapping, assassination, battle-related death, extrajudicial execution, torture, and sexual violence) provided by the Peruvian Truth and Reconciliation Commission (*Comisión de la Verdad y Reconciliación*, henceforth CVR). In addition to its final report (Comisión de la Verdad y Reconciliación, 2003), the CVR compiled several datasets 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 under-reporting (Ball et al., 2003). Moreover, not all information could be geo-coded 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 datasets (cf. appendix) and works with dummy 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 zero otherwise.

Covariates

Despite the locally haphazard assignment of the emergency zone borders, confounding may still be an issue, as the instrument is non-randomly 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 prior to 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 and insurgent groups to control territory. In insurgencies, insurgent control is typically concentrated in remote rural areas where coercive state power is difficult to project. Therefore, the proximity 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 pre-campaign outcome variable 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. It also, although only partially, addresses concerns about compound treatment effects, which are often a problem when natural experiments are based on borders that overlap with other boundaries (Keele and Titiunik, 2016). 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, 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 intention-to-

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

treat (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, 75km: 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 local average treatment effect (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 under-reporting. 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

[12]OLS estimates are shown in the appendix.

Table 1: State Violence and Autodefena Mobilization (2SLS)

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
						First Stage (DV: Exposure to State Violence)						
Emergency Zone	0.048***	0.044***	0.056***	0.055***	0.073***	0.072***	0.038***	0.034***	0.045***	0.050***	0.061***	0.065***
	(0.009)	(0.011)	(0.011)	(0.014)	(0.016)	(0.019)	(0.009)	(0.010)	(0.011)	(0.013)	(0.015)	(0.018)
Distance to Em. Zone Border	0.000	0.000	-0.000+	-0.000+	-0.000**	-0.000*	-0.000	-0.000	-0.000+	-0.000*	-0.000**	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization							0.199	0.191	0.193	0.183	0.550***	0.759***
							(0.349)	(0.351)	(0.351)	(0.347)	(0.164)	(0.035)
Prior Insurgent Presence							0.051***	0.056***	0.034*	0.028	0.030	-0.003
							(0.013)	(0.015)	(0.016)	(0.020)	(0.025)	(0.041)
Distance to Province Capital							0.000	0.000	0.000	-0.000*	-0.000	-0.000*
							(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence							0.425***	0.428***	0.444***	0.432***	0.499***	0.467***
							(0.047)	(0.050)	(0.060)	(0.065)	(0.071)	(0.077)
						Reduced Form (DV: Autodefensa Mobilization)						
Emergency Zone	0.015***	0.016**	0.019**	0.021**	0.022*	0.026*	0.012**	0.013**	0.014**	0.016*	0.016*	0.018*
	(0.005)	(0.005)	(0.006)	(0.007)	(0.009)	(0.010)	(0.004)	(0.005)	(0.005)	(0.007)	(0.008)	(0.009)
Distance to Em. Zone Border	-0.000**	-0.000**	-0.000**	-0.000**	-0.000*	-0.000*	-0.000**	-0.000**	-0.000**	-0.000*	-0.000*	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization							-0.083**	-0.092**	-0.097**	-0.103**	-0.090**	-0.076***
							(0.028)	(0.031)	(0.035)	(0.037)	(0.031)	(0.022)
Prior Insurgent Presence							0.015**	0.018**	0.020*	0.022*	0.037*	0.045+
							(0.006)	(0.007)	(0.008)	(0.010)	(0.015)	(0.025)
Distance to Province Capital							0.000**	0.000**	0.000*	0.000+	0.000+	0.000*
							(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence							0.083**	0.095**	0.097*	0.099*	0.088+	0.078
							(0.028)	(0.031)	(0.038)	(0.044)	(0.049)	(0.052)
						Second Stage (DV: Autodefensa Mobilization)						
State Violence (Instrumented by Em. Zone)	0.310**	0.371**	0.328**	0.372**	0.302*	0.365*	0.319**	0.380*	0.310**	0.327*	0.263*	0.281*
	(0.097)	(0.129)	(0.106)	(0.139)	(0.120)	(0.148)	(0.117)	(0.160)	(0.120)	(0.140)	(0.129)	(0.135)
Distance to Em. Zone Border	-0.000**	-0.000*	-0.000**	-0.000*	-0.000	-0.000	-0.000*	-0.000*	-0.000**	-0.000*	-0.000+	-0.000+
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization							-0.146	-0.164	-0.157	-0.163	-0.235**	-0.289**
							(0.102)	(0.122)	(0.098)	(0.104)	(0.085)	(0.112)
Prior Insurgent Presence							-0.001	-0.003	0.009	0.013	0.029+	0.046+
							(0.008)	(0.011)	(0.009)	(0.011)	(0.015)	(0.025)
Distance to Province Capital							0.000**	0.000**	0.000*	0.000*	0.000*	0.000*
							(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence							-0.052	-0.068	-0.040	-0.042	-0.043	-0.053
							(0.058)	(0.079)	(0.066)	(0.073)	(0.078)	(0.078)
BS Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	75km	65km	55km	45km	35km	25km	75km	65km	55km	45km	35km	25km
N	7295	6383	5276	4243	3335	2419	7295	6383	5276	4243	3335	2419
F-stat (1st 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

* p<0.05, ** p<0.01, *** p<0.001; robust standard errors in parentheses.
† Exposure to State Violence instrumented by Location inside/outside Emergency Zone.

than non-lethal forms of violence, and that the Peruvian truth and reconciliation commission had distinct data collection criteria than other human rights organizations present during the war. The findings remain substantially unchanged.

Second, there was some top-down mobilization during the period 1983-1985, i.e. 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 (Lyall, 2010), or for creating a perception of the same (Kalyvas, 2006). Robustness checks in the appendix indicate that the reported effects are stable if state violence exclusively measures 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.

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 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 socio-economic 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 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 to assess 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 (Mebane, 2011; Sekhon, 2011; Diamond and Sekhon, 2013). 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. The second column 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 to evaluate the competing explanation that it was insurgent presence that motivated communities to mobilize against them. Again, the results indicate a positive and significant intention-to-treat effect for subsequent periods. Γ is the relevant sensitivity parameter and 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 instrument in order to eliminate the reported results (Rosenbaum, 2002; Keele 2010). 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 being assigned to the 'treatment' (ITT) 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, 112ff.) finds a Γ of 6.

Table 2: **ITT Genetic Matching and Sensitivity Analysis**

	I	**II**
ATT (ITT matched)	0.011***	0.007**
SE	(0.003)	(0.002)
t	4.14	3.22
N (original, total)	7295	6498
N (original, 'treated' [ITT])	3616	2819
Highest Γ (McNemar's test < 0.05)	21.8	17.1
Bandwidth	75km	75km
Replacement	No	Yes

* p<0.05, ** p<0.01, *** p<0.001. Impact of location inside/outside the emergency zone on counterinsurgent mobilization. 1:1 genetic matching (population size = 10'000). Column I reports results for full set (75km bandwidth) without replacement. Column II reports results for regions where Prior Insurgent Presence equals 0 (75km bandwidth), with replacement. Matching is performed on Prior Insurgent Violence, Prior Insurgent Presence (only column I), Distance to Province Capital, Mita, Prior Mobilization. Abadie-Imbens Standard Errors shown in parentheses. Γ refers to the highest value at which the McNemar's test still turns out significant ($p < 0.05$).

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 increase 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 difference-in-differences (DiD) approach with two time periods (corresponding to the conflict phases prior to 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 appendix).[13] In the regression framework, the time indicator $Period_t$ assumes the value of 1 for the period after the counterinsurgency campaign. The coefficient on the interaction term for the state violence and period indicator is the effect of interest; it indicates the average effect on the treated:[14]

$$Y_{it} = \alpha + \beta(SV_i \times Period_t) + \delta SV_i + \phi Period_t + \epsilon_{it} \tag{5}$$

As there is only one pre-treatment period, pre-treatment trends cannot be explored in detail (cf. appendix). Thus, the difference-in-differences strategy is combined with propensity score screening to restrict the analysis to units with comparable treatment propensities (Rosenbaum and Rubin, 1983; Angrist and Pischke, 2009; Crump, Hotz, Imbens and Mitnik, 2009).[15] Table 3 shows the results for all *centros poblados* inside the region of common support, the latter being defined by the propensity score based on the same confounding variables as reported above.[16] Shown are the results with standard errors clustered at the village (columns I and III) and district level (columns II and IV), as well as with and without the same covariates used to

[13]The results hold if the DiD analysis is repeated in the same study region as 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 here, emergency status is not an instrument and only measured at the very outset of the counterinsurgency campaign (*Initial Military Rule*).

obtain the propensity score. State violence has a positive and substantial effect on subsequent counterinsurgent mobilization in all specifications.

Table 3: **Difference-in-Differences (OLS): Autodefensa Mobilization**

	I	**II**	**III**	**IV**
Period	0.001***	0.001**	0.001***	0.001**
	(0.000)	(0.000)	(0.000)	(0.000)
Period x State Violence	0.047***	0.047**	0.047***	0.047**
	(0.010)	(0.015)	(0.010)	(0.015)
State Violence	0.004	0.004	-0.003	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Prior Insurgent Presence			0.002	0.002
			(0.001)	(0.002)
Initial Military Rule			0.003***	0.003***
			(0.001)	(0.001)
Prior Insurgent Violence			0.026*	0.026**
			(0.011)	(0.009)
Distance to Province Capital			0.000***	0.000
			(0.000)	(0.000)
Constant	0.000	0.000	-0.003***	-0.003*
	(0.000)	(0.000)	(0.001)	(0.001)
R^2	0.029	0.029	0.035	0.035
Clusters	11958	285	11958	285
N	23916	23916	23916	23916

* p<0.05, ** p<0.01, *** p<0.001; clustered standard errors in parentheses.

The appendix presents an inspection of differences in pre-campaign mobilization and several robustness tests, including a 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 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 due to 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 variable 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 distinct profile 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 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 theory, 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 pro-social norms in communities (Gilligan, Pasquale and Samii, 2014) as well as transformed preferences at the individual level (e.g., Voors, Nillesen, Verwimp, Bulte, Lensink and Van Soest, 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 that 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 or 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 due to the canonical conceptualization of victimized civilians as homogeneous social groups. However, these processes can — and often do — occur in parallel at the local level. This implies that state violence against noncombatants is not only 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 reshape local networks and institutions, and to leave

lasting marks on social and political life long after civil wars end.

Acknowledgements

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 paper were presented at the OCV Yale speaker series, the Conference on Militias at Yale Unversity, the MPSA and APSA meetings, the ENCoRe Istanbul meeting, the Annual International Conference on Development Economics & 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, Oxford: Princeton Univ. Press.

Arjona, Ana. 2014. "Wartime Institutions: A Research Agenda." *Journal of Conflict Resolution* 58(8):1360–1389.

Balcells, Laia. 2010. "Rivalry and Revenge.Violence against Civilians in Conventional Civil Wars." *International Studies Quarterly* 54(2):291–313.

Ball, P., J. Asher, D. Sulmont and D. 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*. Washington: HRDAG, AAAS.

Bateson, Regina. 2013. Order and Violence in Postwar Guatemala PhD thesis Yale University, New Haven, CT.

Bauer, Michal, Christopher Blattman, Julie Chytilová, Joseph Henrich, Edward Miguel and Tamar Mitts. 2016. "Can War Foster Cooperation?" NBER Working Paper 22312.

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 – 724.

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.

Centro de Estudios y Promoción del Desarrollo (DESCO). 1989. *Violencia política en el Perú 1980 - 1988. Tomo I.* DESCO.

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 & Terrorism* 37(11):920–935.

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, Peru.

Comisión de la Verdad y Reconciliación. 2003. "Informe Final." Lima: CVR.

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):167–187.

Coronel, José. 1996. Violencia política y repuestas campesinas en Huanta. In *Las rondas campesinas y la derrota de Sendero Luminoso*, ed. C. I. Degregori, J. Coronel, P. del Pino and O. Starn. IEP/Universidad Nacional de San Cristóbal de Huamanga (Estudios de la Sociedad Rural, 15) pp. 29–116.

Crump, Richard, Joseph Hotz, Guido Imbens and Oscar Mitnik. 2009. "Dealing with limited overlap in estimation of average treatment effects." *Biometrika* 96(1):187–199.

Degregori, Carlos Iván. 1996. Ayacucho, después de la violencia. In *Las rondas campesinas y la derrota de Sendero Luminoso*, ed. C. I. Degregori, J. Coronel, P. del Pino and O. Starn. Lima: IEP.

Degregori, Carlos Iván. 1998. Harvesting Storms: Peasant Rondas and the Defeat of Sendero Luminoso in Ayacucho. In *Shining and Other Paths. War and Society in Peru, 1980-1995*, ed. Steve J. Stern. Durham and London: Duke University Press.

Del Pino, Ponciano. 1996. Tiempos de guerra y de dioses: Ronderos, evangélicos y senderistas en el valle del río Apurímac. In *Las rondas campesinas y la derrota de Sendero Luminoso*, ed. C. I. Degregori, J. Coronel, P. Del Pino and O. Starn. Instituto de Estudios Peruanos.

Dell, Melissa. 2010. "The Persistent Effects of Peru's Mining Mita." *Econometrica* 78(6):1863–1903.

Diamond, Alexis and Jasjeet S. Sekhon. 2013. "Genetic Matching for Estimating Causal Effects: A General Multivariate Matching Method for Achieving Balance in Observational Studies." *The Review of Economics and Statistics* 95(3):932–945.

Dirección Nacional de Censos y Encuestas. 2004. "Marco Conceptual de Precenso. Censos Nacionales de Población y Vivenda." Lima.

Editorial Normas Legales S.A. N.d. "Normas legales. Revista de Legislación y Jurisprudencia." Trujillo: Editorial Normas Legales S.A.

Fumerton, Mario. 2001. "Rondas Campesinas in the Peruvian Civil War: Peasant Self-defence Organisations in Ayacucho." *Bulletin of Latin American Studies* 20(4):470–497.

Fumerton, Mario. 2002. "From Victims to Heroes: Peasant Counter-Rebellion and Civil War in Ayacucho." PhD Thesis. University of Utrecht.

Gambetta, Diego. 2009. Signaling. In *The Oxford Handbook of Analytical Sociology*, ed. Peter Hedstroem and Peter Bearman. 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 Thesis. University of Oslo.

García-Godos, Jemima. 2008. "Victim Reparations in the Peruvian Truth Commission and the Challenge of Historical Interpretation." *The 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):604–619.

Goodwin, Jeff. 2001. *No Other Way Out. States and Reviolutionalry Movements, 1945 - 1991.* 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):201–209.

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):1161–1188.

Huillca, Hugo. 1993. El diálogo con Sendero es imposible. In *Hablan con Ronderos. La búsqueda por la paz en los Andes*, ed. O. Starn. Institutio de Estudios Peruanos pp. 44–45.

Humphreys, Macartan and Jeremy Weinstein. 2008. "Who Fights? The Determinants of Participation in Civil War." *American Journal of Political Science* 52(2):436–455.

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 *The Shining Path of Peru*, ed. David Scott Palmer. New York: St. Martin's Press.

Jentzsch, 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, 2013.

Jentzsch, Corinna, Stathis N. Kalyvas and Livia Isabella Schubiger. 2015. "Militias in Civil War." *Journal of Conflict Resolution* 59(5):755–769.

Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War*. Cambridge: Cambridge University Press.

Kalyvas, Stathis N. and Laia Balcells. 2010. "International System and Technologies of Rebellion: How the End of the Cold War Shaped Internal Conflict." *American Political Science Review* 104(3):415–429.

Kalyvas, Stathis N. and Matthew Kocher 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." White Paper. Columbus, OH, 1-15.

Keele, Luke J. and Rocío Titiunik. 2015. "Geographic Boundaries as Regression Discontinuities." *Political Analysis* 23(1):127–155.

Keele, Luke and Rocío 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* 55:201–218.

Kuran, Timur. 1995. *Private Truth, Public Lies*. Harvard University Press.

La Serna, Miguel. 2012. *The Corner of the Living. Ayacucho on the Eve of the Shining Path Insurgency*. The University of North Carolina Press.

Lee, David S. and Thomas Lemieux. 2010. "Lee and Lemieux: Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48:281–355.

Lichbach, Mark I. 1995. *The Rebel's Dilemma*. Michigan: University of Michigan Press.

Lyall, Jason. 2009. "Does Indiscriminate Violence Incite Insurgent Attacks? Evidence from Chechnya." *Journal of Conflict Resolution* 53(3):331–362.

Lyall, Jason. 2010. "Are Co-Ethnics More Effective Counter-Insurgents? Evidence from the Second Chechen War." *American Political Science Review* 104(1):1–20.

Lyall, 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*. 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-Sactioned Terror." *International Studies Quarterly* 33:175–198.

McClintock, C. 1989. Peru's Sendero Luminoso Rebellion: Origins and Trajectory. In *Power and Popular Protest*, ed. S. Eckstein. University of California Press.

McClintock, Cynthia. 1999. The Decimation of Peru's Sendero Luminoso. In *Comparative Peace Processes in Latin America*, ed. Cynthia J. Arnson. Washington DC: Woodrow Wilson Center Press pp. 223–250.

Mebane, Walter Sekhon, Jasjeet S. 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*. Publinor, Lima.

Olson, Mancur. 1971. *The Logic of Collective Action*. Harvard: 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. 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." *The Journal of the American Statistical Association* pp. 591–593.

Rubin, Donald B. 1986. "Which Ifs Have Causal Answers?" *Journal of the American Statistical Association* 81:961–962.

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):547–580.

Steele, Abbey. 2009. "Seeking Safety: Avoiding Displacement and Choosing Destinations in Civil Wars." *Journal of Peace Research* 46:419–430.

Tapia, Carlos. 1997. *Las Fuerzas Armadas y Sendero Luminoso: Dos Estrategias y un Final*. 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):222–238.

Van der Klaauw, Wilbur. 2002. "Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach." *International Economic Review* 43(4):1249–1287.

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):941–964.

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." *Annu. Rev. Polit. Sci. 2008* 11:539–561.

Biographical Statement

Livia Isabella Schubiger is the Douglas and Ellen Lowey Assistant Professor, Department of Political Science, Duke University, Durham, NC 27708.

Appendix

State Violence and Wartime Civilian Agency:

Evidence from Peru

Contents

List of Tables

List of Figures

Overview

This appendix provides supplementary information on the data, measurement decisions, research designs, main results, and robustness checks for the article "State Violence and Wartime Civilian Agency: Evidence from Peru."

Measurement Periods

Table 1 summarizes the three measurement periods described in the empirical section of the article.

Table 1: **Three Phases of Political Violence**

Phase		**Years**	**Period in Research Design**
I	Insurgency Onset	1980, 1981, 1982	'Pre-treatment' Period
II	Counterinsurgency Campaign	1983, 1984, 1985	'Treatment' Period
III	Reorientation	1986, 1987, 1988	'Post-treatment' Period

Due to the substantive focus on community-based mobilization, the years after 1988 are not under study in this paper. In 1989, president Alan García started to openly pursue the strategy of officially forming and supporting village patrols (Degregori, 1996; Fumerton, 2002; García-Godos, 2006). In the early 1990s, president Alberto Fujimori, Garcías successor, strongly pushed the strategy of coopting the *rondas* further (e.g., McClintock, 1999, 235ff.).

IV Approach: Additional Information

As outlined in the article, the IV approach studies the effect of state violence, SV_i, by exploiting the haphazard placement of villages inside or outside the emergency zone $Zone_i$, and by taking into account that not every village in the emergency zones was targeted:

$$\frac{E[Y_i|Zone_i = 1] - E[Y_i|Zone_i = 0]}{E[SV_i|Zone_i = 1] - E[SV_i|Zone_i = 0]} \tag{1}$$

The instrumental variable approach employed in this study shares some assumptions with the geographic natural experiment and the fuzzy regression discontinuity design, in particular the absence of precise and pervasive sorting and the negligibility of compounding (Hahn, Todd and Van der Klaauw, 2001; Van der Klaauw, 2002; Keele and Titiunik, 2016).[1]

Ideally, one could resort restrict the analysis to a very narrow bandwidth around the boundary. This is not possible here due to the fact that variation in repression exposure is limited. Moreover, this study relies on the *conditional* treatment ignorability assumption, which is often warranted in geographic natural experiments due to covariate imbalances around the border (Keele and Titiunik, 2016). Thus, it is necessary to broaden the study region of interest, and to rely on a parametric approach (2SLS) with covariate adjustment. The focus is on several distinct quantities of interest: First, the intention-to-treat (ITT) effect of the instrument on the dependent variable, and second, the local average treatment effect (LATE) of SV_i on 'com-

[1] In sharp RD designs, selection into treatment is determined deterministically by a discontinuous function of the so-called 'forcing variable' S. There is a 'sharp' cutoff s_z where treatment status changes so that $T = 1$ if $S \geq s_z$ and T = 0 if $S < s_z$ (Angrist and Pischke, 2009, 251f.; Imbens and Lemieux, 2008, 617). If the conditional expectations of the potential outcomes are continuous functions of the forcing variable, and if the probability of treatment exposure jumps from 0 to 1 at a given cutoff, then the average causal effect of T on Y at the cutoff can be written as (Imbens and Lemieux, 2008, 618f.; Lee and Lemieux, 2010, 288):

$$\lim_{s \downarrow s_z} E[Y|S = s] - \lim_{s \uparrow s_z} E[Y|S = s]$$

or simply

$$E[Y^1 - Y^0|S = s_z]$$

While it is of course not possible to observe both treated and nontreated units right at the cutoff, treated and nontreated units might be similarly comparable as if randomized in the immediate region of s_z. Ideally, the causal effect can hence be identified in this region, for example via local linear regression or simple difference in means. If this is not possible due to data limitations in the narrow region of the threshold, parametric approaches have to be used (Angrist and Pischke, 2009, 251ff.; Lee and Lemieux, 2010, 286f.; see also Dunning, 2012).

In 'fuzzy' RD designs, the probability of treatment increases discontinuously with the values of the forcing variable, but the relationship is not deterministic (Angrist and Pischke, 2009, 259ff.; Imbens and Lemieux, 2008, 619; Lee and Lemieux, 2010, 300):

$$\lim_{s \downarrow s_z} Pr[T = 1|S = s] \neq \lim_{s \uparrow s_z} Pr[T = 1|S = s]$$

Consequently, here the quantity of interest is:

$$\frac{\lim_{s \downarrow s_z} E[Y|S = s] - \lim_{s \uparrow s_z} E[Y|S = s]}{\lim_{s \downarrow s_z} E[D|S = s] - \lim_{s \uparrow s_z} E[D|S = s]}$$

pliers,' calculated in essence as the intention-to-treat (or 'reduced form') effect rescaled by the first stage (Hahn, Todd and Van der Klaauw, 2001; Van der Klaauw, 2002; Angrist and Pischke, 2009, 259-267).

Omitting covariates, in the 2SLS approach the endogenous regressor SV_i in (2) is replaced with fitted values from the first stage (3). This leads to the second stage (4). The ITT effect is estimated via the reduced form (5).

$$Y_i = \alpha_1 + \beta_1 SV_i + \epsilon_{1i} \tag{2}$$

$$SV_i = \delta + \gamma Zone_i + \eta_i \tag{3}$$

$$Y_i = \alpha_2 + \beta_2 \widehat{SV}_i + \epsilon_{2i} \tag{4}$$

$$Y_i = \kappa + \lambda Zone_i + \mu_i \tag{5}$$

$Zone_i$ is the binary instrument, SV_i a binary indicator for exposure to state violence, and Y_i the binary outcome. The assumptions underlying the IV approach are that there are no unobserved variables correlated with both the instrument and the outcome of interest (conditional ignorability), that the instrument is correlated with the treatment (relevance), that there are no 'defiers' who take up the treatment only if unassigned to the instrument or vice versa (monotonicity), and that there is no direct effect of the instrument on the outcome other than through the treatment (exclusion restriction) (Sovey and Green, 2011). In addition to

standard IV assumptions, one has to rule out compound treatment effects (Keele and Titiunik, 2016) and SUTVA violations. This study employs covariate adjustment, genetic matching, and Rosenbaum sensitivity analysis to test the robustness of the results to observed and unobserved confounding (Rosenbaum, 2002; Keele, 2010). For a discussion of these assumptions, see main article.

Additional Information on Data Sources

Unit of Analysis: The Peruvian Truth and Reconciliation Commission (Comisión de la Verdad y Reconciliación, 2003*a*) geo-coded violent events with reference to the *centros poblados* as defined and recorded by the *Instituto Nacional de Estadística y Informática* (2002). Geographic information on administrative units and their boundaries is thus based on digital maps compiled by the *Instituto Nacional de Estadística e Informática* (INEI, 2002). Administrative boundary changes were traced based on the *Dirección Nacional Técnica de Demarcación Territorial* (PCM-DNTDT, 2011).

Emergency Zones: Provinces and districts under emergency and military command were coded as listed in Desco (1989, 345ff.) and as specified in the *decretos supremos* re-published in various volumes of the *'Normas legales - Revista de Legislación y Jurisprudencia,'* accessed in the *Archivo del Congreso de la República* (Editorial Normas Legales S.A., N.d.). While the emergency zones were assigned to particular provinces at the time of interest, the districts of several provinces were assigned to other (newly created) provinces later. The data presented here work with the new administrative units, taking these changes into account.

Violence: The Peruvian Truth and Reconciliation Commission (Comisión de la Verdad y Reconciliación, henceforth CVR) was created by the provisional government of Peru in 2001 with the mandate to elucidate 20 years of political violence (1980-2000). Focusing on human rights violations during the years of the internal armed conflict and the ensuing Fujimori pe-

riod, the CVR dedicated 18 months to the collection of data throughout the country. The CVR came up with more than 15,000 individual testimonies as well as more than 400 testimonies from public hearings. For an overview of the data collection see `http://www.cverdad.org.pe/`. In addition to its final report (Comisión de la Verdad y Reconciliación, 2003*b*), the CVR compiled two datasets which cover information on victims (killed and disappeared individuals) and violent events (Comisión de la Verdad y Reconciliación, 2003*a*). In an additional dataset provided by the CVR, information gathered by the CVR was complemented with data collected by various human rights organizations, namely the International Committee of the Red Cross, the *Defensoría del Pueblo*, the *Comisión de Derechos Humanos*, the *Coordinadora Nacional de Derechos Humanos*, and the *Centro de Desarrollo Agropecuario*. This dataset was used to perform multiple systems estimations (MSE) of the total number of victims (Ball, Asher, Sulmont and Manrique, 2003; Comisión de la Verdad y Reconciliación, 2003*c*). While it has not been possible to perform the MSE at the *centro poblado* level, and hence, in this study,[2] this third dataset is nevertheless very useful, as it provides additional information based on various sources other than the CVR. Therefore, this study relies on all three datasets to construct the measures for *Exposure to State Violence*, *Prior Insurgent Violence*, and *Autodefensa Mobilization*.

Outer Borders of Emergency Zones: In the instrumental variables design, the variable Emergency Zone indicates whether a certain village or town was located inside or outside the emergency zone. Figure 2 shows the outer borders of all provinces placed under emergency and military command at least once during 1983, 1984, and 1985 in the study region (elevation in the background). Information on elevation is based on GTOPO30 (EROS, U.S. Geological Survey's Center for Earth Resources Observation and Science, N.d.).[3] Only the northeast-

[2]The lowest level the MSE could be performed was the district level, and even this was only possible for a small number of districts (Ball et al., 2003).

[3]GTOPO30 is a global digital elevation model (DEM) resulting from work led by the U.S. Geological Survey's EROS Data Center and conducted in collaboration with the National Aeronautics and Space Administration (NASA), the United Nations Environment Programme/Global Resource Information Database (UNEP/GRID), the U.S. Agency for International Develop-

ern segment of this border determines the study region in the IV research design, due to the stability of this border segment over time. In this region, it is hence possible to clearly distinguish between affected and unaffected cases. Here, the emergency zone borders collide with the border segments of Tayacaja (Huancavelica), Huanta (Ayacucho), La Mar (Ayacucho), and Andahuaylas (Apurímac). Map 2 shows the relevant subregion, along with the *centros poblados* within 75,000 meters of Euclidean distance to the emergency zone boundary.

Territorial Units and Distance Measures: Since some of Peru's administrative boundaries changed over time (i.e., between the 1980s and 2002, when the digital map underlying this study was produced), territorial consistency regarding the borders of districts, provinces, and departments was checked and adjusted where necessary, based on official records of changes in administrative boundaries (PCM-DNTDT, 2011). Geographic proximity to the closest province capital and closest point on the emergency zone boundary from each *centro poblado* is measured in Euclidean distance (meters), based on a digitized map of political-administrative units (INEI, 2002). All distance measures were calculated in ArcGIS10.

ment (USAID), the Instituto Nacional de Estadística Geografíca e Informática (INEGI) of Mexico, the Geographical Survey Institute (GSI) of Japan, the Manaaki Whenua Landcare Research of New Zealand, and the Scientific Committee on Antarctic Research (SCAR).

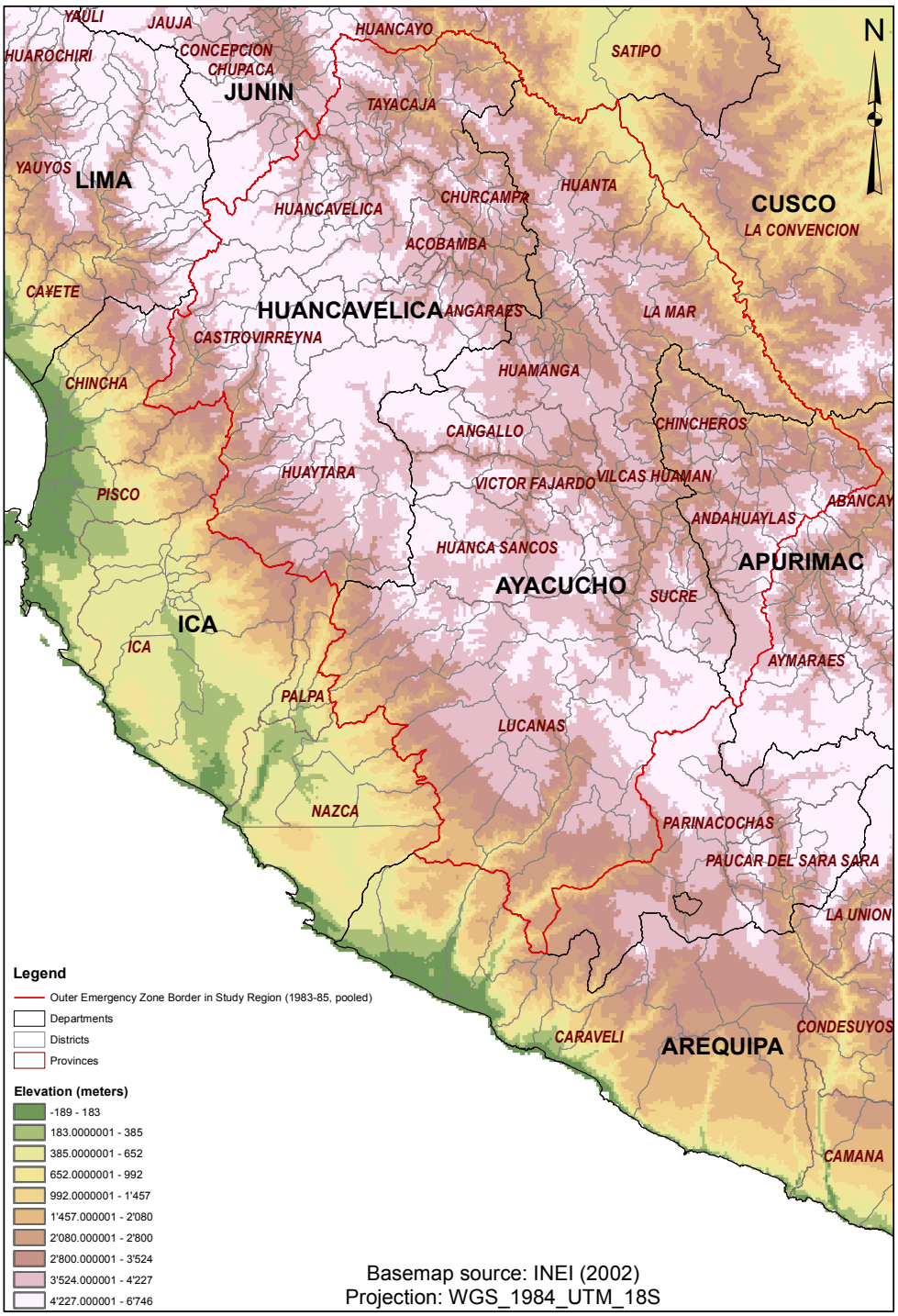

Figure 1: Outer Border of Areas under Emergency 1983-1985 (Study Region Only).

Figure 2: Centros Poblados under Study in IV Design

Additional Information on Results and Robustness Checks

Table 2 provides descriptive statistics for a bandwidth of 75 kilometers on each side of the emergency zone boundary. What is evident is that *autodefensa* mobilization — despite its surge and huge impact on the war in the 1990s — was still very rare in the period of interest.

Table 2: **Descriptive Statistics (Bandwidth = 75 km; N = 7,295)**

Variable	Mean	SD	Min	Max
Autodefensa Mobilization (Post)	0.006	0.077	0	1
State Violence	0.049	0.216	0	1
Emergency Zone	0.496	0.500	0	1
Prior Insurgent Presence	0.109	0.312	0	1
Prior Insurgent Violence	0.016	0.125	0	1
Distance to Emergency Zone Border†	-453	43573	-74923	74960
Distance to Province Capital†	22697	14814	0	115028
Mita	0.815	0.388	0	1
Autodefensa Mobilization (Pre)	0.000	0.02	0	1

† Meters

Table 3: **Outcome (75 km bandwidth)**

	Freq.	Percent	Cum.
0	7,252	99.41	99.41
1	43	0.59	100.00
Total	7,295	100.00	
Mean: 0.006			

The outcome variable is described in table 3 for a bandwidth of 75km on each side of the emergency zone border. Next, the distribution of the data on each side of the border is illustrated graphically. Plotted are the observed means of the respective variables for 2.5km bins of Euclidean distance to the border. Plotted are also the smoothed lines from linear and polynomial regressions where the emergency zone dummy is interacted with distance and quadratic or cubic polynomials thereof, respectively, along with the 95% confidence intervals.

Figure 3 illustrates exposure state violence as a function of geographic location, and Figure 4 presents the graph for the ITT equivalent. Shown are the results for the bandwidth of 75 kilometers on each side of the border. Of primary interest is the discontinuity where distance equals 0, i.e., the border of the emergency zones. The graphs consistently suggest that the

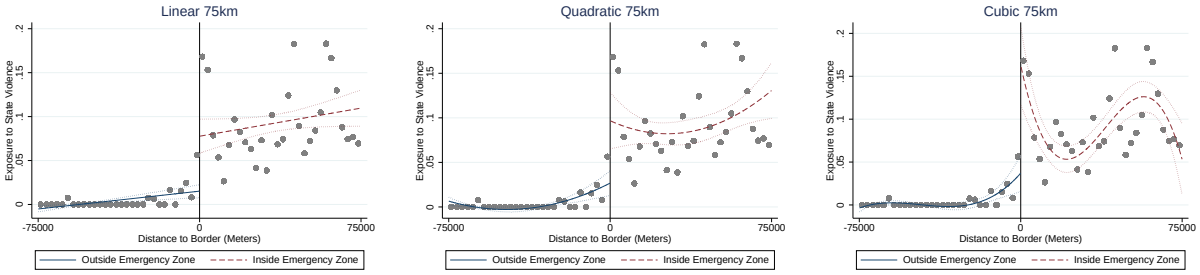

Figure 3: State Violence (First Stage)

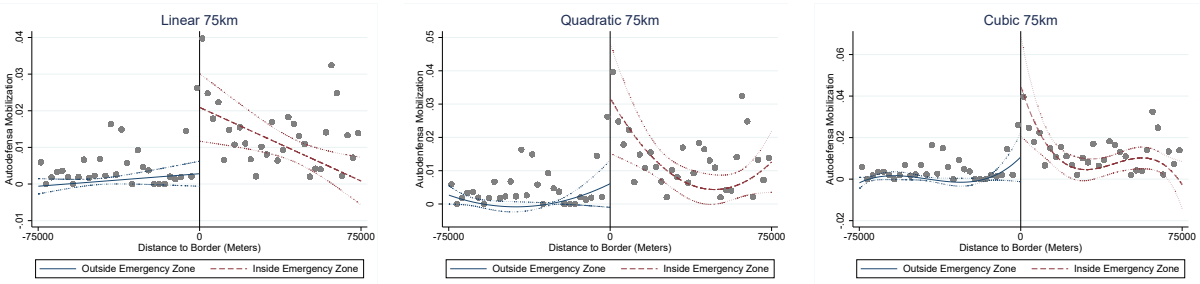

Figure 4: Autodefensa Mobilization (Reduced Form/ITT)

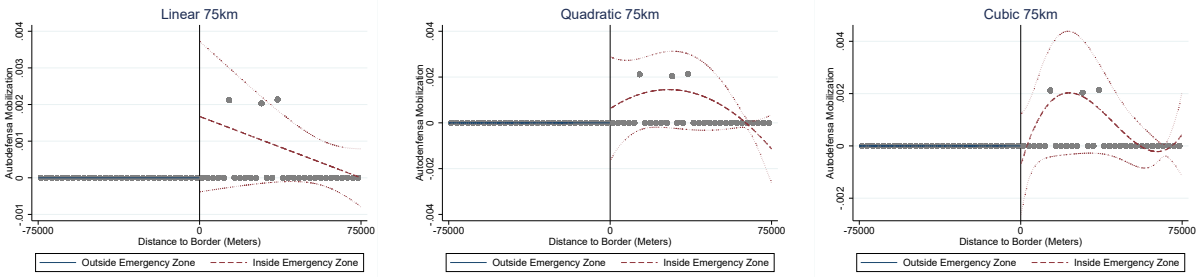

Figure 5: Pre-Campaign Autodefensa Mobilization

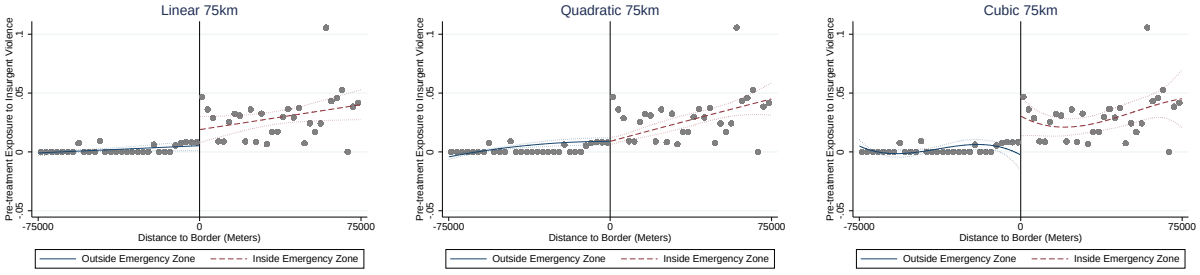

Figure 6: Prior Insurgent Violence

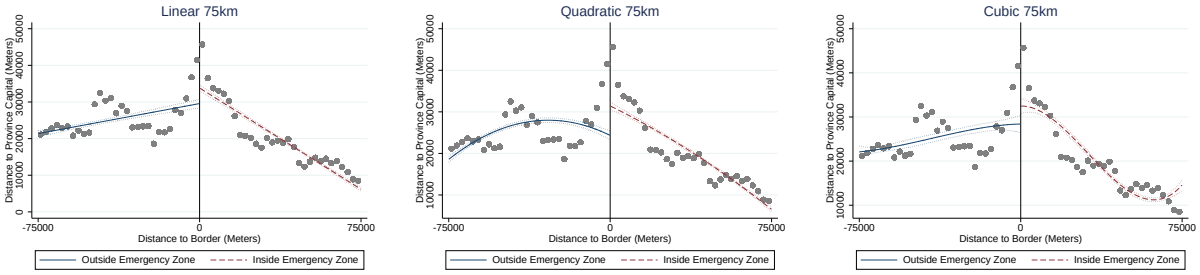

Figure 7: Distance to Province Capital

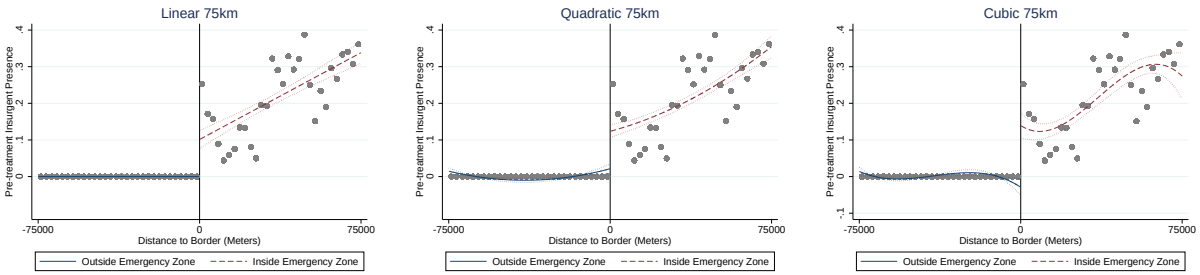

Figure 8: Prior Insurgent Presence

discontinuity at the emergency zone border is not an artifact of the functional form (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).

An assumption that follows from the haphazard assignment of the instrument is that pre-treatment covariates vary across the boundary in a relatively smooth way. Figure 5 plots the pre-campaign outcome on each side of the border; there is no mobilization during the pre-campaign period in the relevant study region outside the emergency zone, and only very few cases of *autodefensa* mobilization within, leading to very wide confidence intervals that include 0. Figure 6 shows that the regression line for exposure to Prior Insurgent Violence suggests smooth variation only once we specify a quadratic functional form, while Figures 7 and 8 suggest discontinuities for the variables Prior Insurgent Presence and Distance to Province Capital. The discontinuity is particularly pronounced for Prior Insurgent Presence, a variable that could only be coded at the district level. Such covariate imbalances at the boundary, while common in geographic natural experiments (Keele and Titiunik, 2016), have to be taken into account. The IV approach is thus combined with covariate adjustment, genetic matching, subset analysis (i.e.

Table 4: **Manipulation Test**

Running variable: Distance	Left of border	Right of border
Number of obs	3679	3616
Eff. Number of obs	1220	852
Order est. (p)	2	2
Order bias (q)	3	3
BW est. (h)	23220.762	19111.889
Result Conventional	-1.0740	0.2828
Result Robust	0.4318	0.6659

Number of obs = 7295; model: unrestricted; BW method = comb
Kernel = triangular; VCE method = jackknife

analysis where Prior Insurgent Presence equals zero), as well as Rosenbaum sensitivity analysis; the results are reported in the main article.

The no-sorting assumption is tested with the density test proposed by Cattaneo, Jansson and Ma (2018). Note that this is an imperfect test in this case, as the sorting assumption is tested at the *centro poblado* level during a particular point in time, whereas migration across the boundary could occur at the individual or small group level and at different points in time. It is also important to note that migration is not, as such, necessarily inconsistent with the assumptions made here (unless occurring prior to treatment assignment and in a precise, pervasive, and systematic way). Migration can, for example, be an important channel through which effects occur (Dell, 2010). Nevertheless, table 4 presents the results of the Cattaneo, Jansson and Ma (2018) manipulation test using local polynomial density estimation. It suggests that sorting around the emergency zone boundary is not a significant issue in this case.

Tables 5, 6, 7, and 8 report the second stage results of several robustness checks based on alternative violence measures. The results presented in Table 5 are based on the same estimations as reported in the article, but with instances of non-lethal violence excluded from the indicator measuring state violence; disappearances are counted as lethal violence. Table 6 uses another alternative measure of state violence that includes lethal and non-lethal forms of violence, yet only counts instances in villages where there were no joint operations. Joint operations are indicative of top-down mobilization efforts by state troops during the counterinsurgency campaign, and may also indicate a higher (actual or perceived) capacity for selective violence. The

results in Table 7 are based on estimations with the same variable definitions as reported in the article, yet with state and insurgent violence, as well as autodefensa mobilization, measured exclusively relying on the Peruvian truth commission's own data, discarding the information gathered by several additional human rights organizations during the war. Finally, Table 8 reports the results based on the same data and variable definitions as reported in the article, with the exception of insurgent violence, which now excludes instances of non-lethal violence.

Table 5: **IV Results (2SLS): Effect of State Violence on Autodefensa Mobilization**

	I	II	III	IV	V	VI
State Violence†	0.352**	0.419*	0.340*	0.360*	0.285*	0.294*
	(0.130)	(0.179)	(0.132)	(0.155)	(0.140)	(0.140)
Distance to Em. Zone Border	-0.000*	-0.000*	-0.000**	-0.000*	-0.000+	-0.000+
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization	-0.162	-0.183	-0.172	-0.181	-0.257**	-0.306*
	(0.113)	(0.136)	(0.108)	(0.115)	(0.094)	(0.119)
Prior Insurgent Presence	-0.002	-0.005	0.009	0.013	0.029+	0.046+
	(0.008)	(0.012)	(0.009)	(0.011)	(0.015)	(0.025)
Distance to Province Capital	0.000**	0.000*	0.000*	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence	-0.060	-0.076	-0.046	-0.048	-0.044	-0.047
	(0.061)	(0.084)	(0.068)	(0.075)	(0.078)	(0.075)
Bandwidth	75km	65km	55km	45km	35km	25km
N	7295	6383	5276	4243	3335	2419
F-stat	16.68	10.30	16.20	12.83	15.21	12.23
BS Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.05, ** p<0.01, *** p<0.001
Robust standard errors in parentheses.
† Exposure to State Violence (alternative measure I), instrumented.

As discussed in the article, the emergency zone boundary partially overlaps with the historical mita boundary (Dell, 2010). Figure 9 provides a map of the mita border and how it relates to the emergency zone boundary in the study region, based on Dell (2010). Table 9 repeats the analysis and includes a dummy variable identifying units inside and outside the former mita zone. Table 9 shows that the results remain stable, although the estimates for the 35 km and 25 km bandwidths narrowly fall short of reaching the $p < 0.05$ threshold. Moreover, the F statistic falls below the conventional threshold of 10 for two bandwidths. These results remain largely the same if LIML estimation is performed instead of 2SLS (Table 10). Tables 11 and 12

Table 6: **IV Results (2SLS): Effect of State Violence on Autodefensa Mobilization**

	I	II	III	IV	V	VI
State Violence†	0.303**	0.350*	0.291**	0.305*	0.249*	0.262*
	(0.109)	(0.144)	(0.111)	(0.128)	(0.121)	(0.123)
Distance to Em. Zone Border	-0.000*	-0.000*	-0.000**	-0.000*	-0.000+	-0.000+
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization	-0.147	-0.163	-0.157+	-0.163+	-0.231**	-0.276**
	(0.095)	(0.111)	(0.091)	(0.096)	(0.082)	(0.105)
Prior Insurgent Presence	-0.000	-0.001	0.011	0.013	0.030*	0.046+
	(0.008)	(0.010)	(0.009)	(0.010)	(0.015)	(0.025)
Distance to Province Capital	0.000**	0.000**	0.000*	0.000**	0.000*	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence	-0.040	-0.049	-0.028	-0.028	-0.030	-0.037
	(0.054)	(0.070)	(0.061)	(0.067)	(0.074)	(0.075)
Bandwidth	75km	65km	55km	45km	35km	25km
N	7295	6383	5276	4243	3335	2419
F-stat	21.60	14.09	21.22	17.29	19.47	15.03
BS Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.05, ** p<0.01, *** p<0.001
Robust standard errors in parentheses.
† Exposure to State Violence (alternative measure II), instrumented.

Table 7: **IV Results (2SLS): Effect of State Violence on Autodefensa Mobilization**

	I	II	III	IV	V	VI
State Violence†	0.337*	0.386*	0.341*	0.374*	0.319+	0.391+
	(0.131)	(0.173)	(0.145)	(0.176)	(0.171)	(0.207)
Distance to Em. Zone Border	-0.000*	-0.000*	-0.000*	-0.000+	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization	-0.175	-0.189	-0.176	-0.181	-0.270*	-0.376*
	(0.106)	(0.123)	(0.110)	(0.122)	(0.111)	(0.171)
Prior Insurgent Presence	0.000	-0.000	0.006	0.009	0.025	0.036
	(0.007)	(0.010)	(0.009)	(0.011)	(0.015)	(0.026)
Distance to Province Capital	0.000*	0.000*	0.000*	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence††	-0.034	-0.049	-0.044	-0.058	-0.072	-0.103
	(0.067)	(0.091)	(0.085)	(0.097)	(0.108)	(0.122)
Bandwidth	75km	65km	55km	45km	35km	25km
N	7295	6383	5276	4243	3335	2419
F-stat	16.35	10.72	13.83	10.34	10.87	6.71
BS Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

+ p<0.1, * p<0.05, ** p<0.01, *** p<0.001
Robust standard errors in parentheses.
† Exposure to State Violence (alternative measure III), instrumented.
†† Insurgent Violence (alternative measure I).

Table 8: **IV Results (2SLS): Effect of State Violence on Autodefensa Mobilization**

	I	II	III	IV	V	VI
State Violence †	0.316**	0.374*	0.306**	0.322*	0.258*	0.276*
	(0.114)	(0.154)	(0.117)	(0.136)	(0.126)	(0.130)
Distance to Em. Zone Border	-0.000**	-0.000*	-0.000**	-0.000*	-0.000+	-0.000+
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization	-0.149	-0.165	-0.160+	-0.167+	-0.236**	-0.284**
	(0.100)	(0.119)	(0.096)	(0.101)	(0.082)	(0.109)
Prior Insurgent Presence	-0.001	-0.003	0.010	0.013	0.030*	0.046+
	(0.008)	(0.011)	(0.009)	(0.010)	(0.015)	(0.025)
Distance to Province Capital	0.000**	0.000**	0.000*	0.000**	0.000*	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence††	-0.046	-0.062	-0.033	-0.031	-0.031	-0.038
	(0.057)	(0.077)	(0.066)	(0.071)	(0.078)	(0.076)
Bandwidth	75km	65km	55km	45km	35km	25km
N	7295	6383	5276	4243	3335	2419
F-stat	19.46	12.37	18.73	15.29	17.47	13.33
BS Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

+ p<0.1, * p<0.05, ** p<0.01, *** p<0.001
Robust standard errors in parentheses.
† Exposure to State Violence, instrumented.
†† Insurgent Violence (alternative measure II).

report the results of balance checks for the genetic matching results shown in the article, and table 13 shows the results of the main specification (table 1, main article), but based on OLS.

Finally, relaxing the assumption that assignment around the emergency zone boundary occurs at the village level, table 14 presents an ITT analysis by cluster means, and table 15 the results of an IV analysis with standard errors clustered by district. Both analyses broadly support the reported results. In the latter, however, the instrument gets weak (Pflueger and Wang, 2015). Hence, weak-instrument robust inference is used to calculate confidence intervals. The Anderson - Rubin (AR) test indicates a positive confidence set that is robust to bias induced by weak instruments at a 95% confidence level for the 75 km bandwidth (Finlay, 2013).

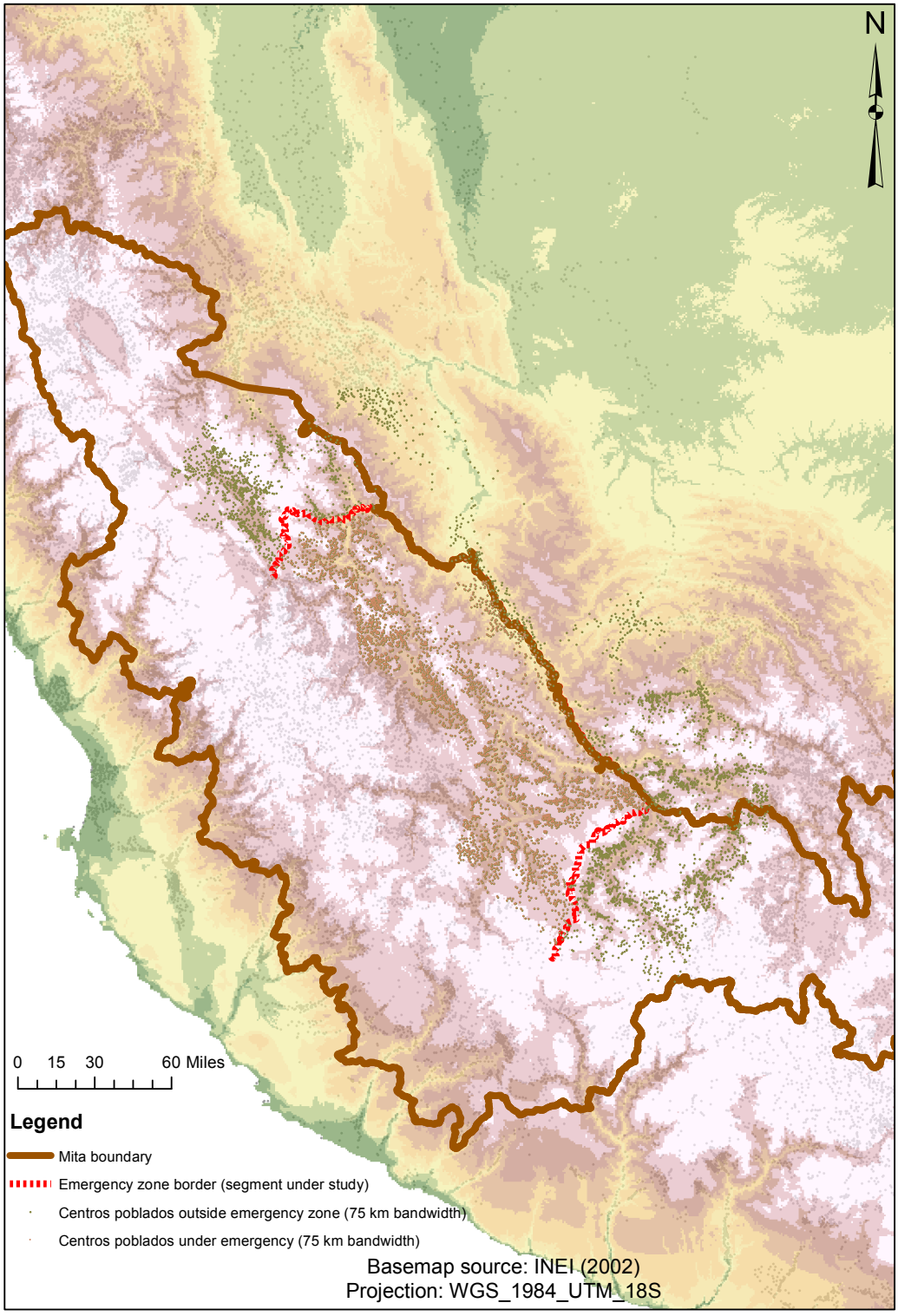

Figure 9: Mita and Emergency Zone Boundaries in Study Region.

Table 9: **IV Results (2SLS): Effect of State Violence on Autodefensa Mobilization (incl. Mita)**

	I	**II**	**III**	**IV**	**V**	**VI**
State Violence †	0.339**	0.436*	0.345*	0.384*	0.304+	0.352+
	(0.130)	(0.202)	(0.144)	(0.178)	(0.160)	(0.185)
Distance to Em. Zone Border	-0.000*	-0.000*	-0.000*	-0.000+	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization	-0.150	-0.174	-0.163	-0.173	-0.257*	-0.341*
	(0.108)	(0.143)	(0.110)	(0.123)	(0.102)	(0.149)
Prior Insurgent Presence	-0.003	-0.007	0.008	0.011	0.028+	0.048+
	(0.009)	(0.014)	(0.009)	(0.011)	(0.015)	(0.026)
Distance to Province Capital	0.000*	0.000	0.000	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence	-0.060	-0.092	-0.055	-0.065	-0.062	-0.083
	(0.063)	(0.096)	(0.075)	(0.087)	(0.091)	(0.097)
Mita	-0.003	-0.007	-0.006	-0.011	-0.009	-0.015
	(0.004)	(0.006)	(0.006)	(0.008)	(0.009)	(0.013)
Bandwidth	75km	65km	55km	45km	35km	25km
N	7295	6383	5276	4243	3335	2419
F-stat	16.06881	8.473679	13.76249	10.29319	12.30658	8.100525
BS Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

† p<0.1 * p<0.05
Robust standard errors in parentheses.
† Exposure to State Violence instrumented by Location inside/outside Emergency Zone.

Table 10: **Second Stage (LIML): State Violence and Autodefensa Mobilization (incl. Mita)**

	I	**II**	**III**	**IV**	**V**	**VI**
State Violence†	0.339**	0.436*	0.345*	0.384*	0.304+	0.352+
	(0.130)	(0.202)	(0.144)	(0.178)	(0.160)	(0.185)
Distance to Em. Zone Border	-0.000*	-0.000*	-0.000*	-0.000+	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization	-0.150	-0.174	-0.163	-0.173	-0.257*	-0.341*
	(0.108)	(0.143)	(0.110)	(0.123)	(0.102)	(0.149)
Prior Insurgent Violence	-0.060	-0.092	-0.055	-0.065	-0.062	-0.083
	(0.063)	(0.096)	(0.074)	(0.087)	(0.090)	(0.097)
Prior Insurgent Presence	-0.003	-0.007	0.008	0.011	0.028+	0.048+
	(0.009)	(0.014)	(0.009)	(0.011)	(0.015)	(0.026)
Distance to Province Capital	0.000*	0.000	0.000	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mita	-0.003	-0.007	-0.006	-0.011	-0.009	-0.015
	(0.004)	(0.006)	(0.006)	(0.008)	(0.009)	(0.013)
Constant	-0.008*	-0.005	-0.003	-0.000	0.000	0.001
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.008)
N	7295	6383	5276	4243	3335	2419
Bandwidth	75km	65km	55km	45km	35km	25km
BS Fixed Effects	7295	6383	5276	4243	3335	2419
N	7295	6383	5276	4243	3335	2419
F-stat	16.06881	8.473679	13.76249	10.29319	12.30658	8.100525

+ p<0.1, p<0.05, ** p<0.01
Robust standard errors in parentheses.
† Exposure to State Violence instrumented by Location inside/outside Emergency Zone.

Table 11: **Covariate Balance for Genetic Matching (Full Set)**

	Before Matching	After Matching
	Prior Insurgent Presence	
mean treatment	0.22041	0.22041
mean control	0	0
std mean diff	53.164	53.164
mean raw eQQ diff	0.22041	0.22571
med raw eQQ diff	0	0
max raw eQQ diff	1	1
mean eCDF diff	0.1102	0.11285
med eCDF diff	0.1102	0.11285
max eCDF diff	0.22041	0.22571
var ratio (Tr/Co)	Inf	Inf
T-test p-value	< 0.000000000000000222	< 0.000000000000000222
	Distance to Province Capital	
mean treatment	19927	19927
mean control	25421	19917
std mean diff	-45.225	0.076127
mean raw eQQ diff	5477.6	19.384
med raw eQQ diff	4333.1	14.315
max raw eQQ diff	50336	798.45
mean eCDF diff	0.091305	0.00054799
med eCDF diff	0.085911	0.00035932
max eCDF diff	0.1799	0.0035932
var ratio (Tr/Co)	0.53631	0.99759
T-test p-value	< 0.000000000000000222	0.34395
KS Bootstrap p-value	< 0.000000000000000222	1
KS Naive p-value	< 0.000000000000000222	0.99961
KS Statistic	0.1799	0.0035932
	Prior Insurgent Violence	
mean treatment	0.029591	0.029591
mean control	0.0021745	0.029591
std mean diff	16.177	0
mean raw eQQ diff	0.027655	0
med raw eQQ diff	0	0
max raw eQQ diff	1	0
mean eCDF diff	0.013708	0
med eCDF diff	0.013708	0
max eCDF diff	0.027416	0
var ratio (Tr/Co)	13.234	1
T-test p-value	< 0.000000000000000222	1
	Mita	
mean treatment	0.9917	0.9917
mean control	0.64148	0.9917
std mean diff	386.06	0
mean raw eQQ diff	0.35039	0
med raw eQQ diff	0	0
max raw eQQ diff	1	0
mean eCDF diff	0.17511	0
med eCDF diff	0.17511	0
max eCDF diff	0.35022	0
var ratio (Tr/Co)	0.035775	1
T-test p-value	< 0.000000000000000222	1
	Prior Mobilization	
mean treatment	0.00082965	0.00082965
mean control	0 0	
std mean diff	2.8812	2.8812
mean raw eQQ diff	0.00082965	0.00041066
med raw eQQ diff	0	0
max raw eQQ diff	1	1
mean eCDF diff	0.00041482	0.00020533
med eCDF diff	0.00041482	0.00020533
max eCDF diff	0.00082965	0.00041066
var ratio (Tr/Co)	Inf	Inf
T-test p-value	0.083265	0.083222

Table 12: **Covariate Balance for Genetic Matching (Prior Insurgent Presence = 0)**

	Before Matching	After Matching
Distance to Province Capital		
mean treatment	21048	21048
mean control	25421	21048
std mean diff	-36.732	-0.001612
mean raw eQQ diff	4669.9	17.873
med raw eQQ diff	3003.1	14.021
max raw eQQ diff	50336	798.45
mean eCDF diff	0.07255	0.00052196
med eCDF diff	0.060016	0.00037026
max eCDF diff	0.17086	0.0033324
var ratio (Tr/Co)	0.51505	0.99732
T-test p-value	< 0.000000000000000222	0.98167
KS Bootstrap p-value	< 0.000000000000000222	1
KS Naive p-value	< 0.000000000000000222	0.99927
KS Statistic	0.17086	0.0033324
Prior Insurgent Violence		
mean treatment	0.023413	0.023413
mean control	0.0021745	0.023413
std mean diff	14.043	0
mean raw eQQ diff	0.021284	0
med raw eQQ diff	0	0
max raw eQQ diff	1	0
mean eCDF diff	0.010619	0
med eCDF diff	0.010619	0
max eCDF diff	0.021238	0
var ratio (Tr/Co)	10.539	1
T-test p-value	0.00000000000075184	1
Mita		
mean treatment	0.99291	0.99291
mean control	0.64148	0.99291
std mean diff	418.64	0
mean raw eQQ diff	0.35154	0
med raw eQQ diff	0	0
max raw eQQ diff	1	0
mean eCDF diff	0.17571	0
med eCDF diff	0.17571	0
max eCDF diff	0.35143	0
var ratio (Tr/Co)	0.030632	1
T-test p-value	< 0.000000000000000222	1
Prior Mobilization		
mean treatment	0.00035474	0.00035474
mean control	0	0
std mean diff	1.8834	1.8834
mean raw eQQ diff	0.00035474	0.00004114
med raw eQQ diff	0	0
max raw eQQ diff	1	1
mean eCDF diff	0.00017737	0.00002057
med eCDF diff	0.00017737	0.00002057
max eCDF diff	0.00035474	0.00004114
var ratio (Tr/Co)	Inf	Inf
T-test p-value	0.3174	0.31731

Table 13: **OLS: State Violence and Autodefensa Mobilization**

	I	II	III	IV	V	VI
State Violence	0.055*	0.060*	0.080*	0.093**	0.110**	0.121**
	(0.012)	(0.011)	(0.019)	(0.016)	(0.015)	(0.016)
Distance to Em. Zone Border	-0.000*	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Mobilization	-0.092*	-0.101**	-0.112*	-0.120**	-0.151**	-0.168***
	(0.016)	(0.013)	(0.022)	(0.014)	(0.017)	(0.010)
Prior Insurgent Presence	0.013+	0.015+	0.018	0.020	0.035	0.048*
	(0.005)	(0.005)	(0.010)	(0.013)	(0.023)	(0.012)
Distance to Province Capital	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence	0.060+	0.069*	0.063	0.060+	0.035*	0.022
	(0.019)	(0.019)	(0.034)	(0.023)	(0.011)	(0.031)
Constant	-0.004	-0.005	-0.005	-0.006	-0.005	-0.008
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.010)
Bandwidth	75km	65km	55km	45km	35km	25km
BS Fixed Effects	7295	6383	5276	4243	3335	2419
N	7295	6383	5276	4243	3335	2419
r2	.0443535	.0500371	.0621044	.0684676	.0706247	.0741849

+ p<0.1, p<0.05, ** p<0.01
Robust standard errors in parentheses.

Table 14: **Two-sample t test with unequal variances**

Group	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
0	150	.0002952	.0001891	.0023159	-.0000784	.0006689
1	113	.0078517	.0023458	.024936	.0032038	.0124996
combined	263	.0035419	.0010372	.0168198	.0014997	.0055841
diff	-.0075564	.0023534	-.0122187	-.0028942		

diff = mean(0) - mean(1), t = -3.2109, Ho: diff = 0
Satterthwaite's degrees of freedom = 113.457
Ha: diff < 0: Pr(T < t) = 0.0009
Ha: diff != 0: Pr(|T| > |t|) = 0.0017
Ha: diff > 0: Pr(T > t) = 0.9991

Alternative Identification

In the second research design, a difference-in-differences (DiD) strategy is applied. While the IV approach is closely tied to the geographic area described above, this is not the case for the difference-in-differences approach. Hence, for this strategy I focus on the departments most heavily affected by political violence during the first decade of the war (Ayacucho, Huancavelica, Apurímac).

Table 15: **State Violence and Autodefena Mobilization (2SLS)**

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
First Stage (DV: Exposure to State Violence)												
Emergency Zone	0.038*	0.034	0.045*	0.050*	0.061*	0.065*	0.034+	0.028	0.038*	0.041+	0.051*	0.052*
	(0.019)	(0.021)	(0.021)	(0.025)	(0.026)	(0.031)	(0.017)	(0.019)	(0.019)	(0.022)	(0.023)	(0.026)
Prior Mobilization	0.199	0.191	0.193	0.183	0.550***	0.759***	0.197	0.189	0.192	0.182	0.548***	0.755***
	(0.349)	(0.350)	(0.351)	(0.346)	(0.164)	(0.020)	(0.349)	(0.349)	(0.350)	(0.345)	(0.163)	(0.022)
Prior Insurgent Presence	0.051*	0.056*	0.034	0.028	0.030	-0.003	0.052*	0.057*	0.035	0.028	0.028	-0.009
	(0.025)	(0.028)	(0.029)	(0.037)	(0.037)	(0.064)	(0.024)	(0.028)	(0.029)	(0.036)	(0.037)	(0.062)
Distance to Province Capital	0.000	0.000	0.000	-0.000	-0.000	-0.000	0.000	0.000	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence	0.425***	0.428***	0.444***	0.432***	0.499***	0.467***	0.424***	0.427***	0.441***	0.428***	0.494***	0.459***
	(0.050)	(0.052)	(0.065)	(0.066)	(0.064)	(0.076)	(0.049)	(0.052)	(0.064)	(0.065)	(0.064)	(0.076)
Distance to Em. Zone Border	-0.000	-0.000	-0.000	-0.000	-0.000*	-0.000+	-0.000	-0.000	-0.000	-0.000	-0.000*	-0.000+
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mita							0.020*	0.023*	0.030*	0.034*	0.033*	0.043+
							(0.010)	(0.012)	(0.013)	(0.015)	(0.017)	(0.024)
Constant	-0.005	-0.001	-0.010	-0.006	-0.015	-0.010	-0.022	-0.022	-0.035+	-0.035	-0.041+	-0.043+
	(0.013)	(0.014)	(0.013)	(0.015)	(0.013)	(0.012)	(0.018)	(0.020)	(0.020)	(0.022)	(0.022)	(0.026)
Reduced Form (DV: Autodefensa Mobilization)												
Emergency Zone	0.012*	0.013+	0.014+	0.016+	0.016+	0.018	0.011*	0.012+	0.013+	0.016+	0.016	0.018
	(0.006)	(0.007)	(0.007)	(0.009)	(0.010)	(0.011)	(0.006)	(0.006)	(0.007)	(0.009)	(0.010)	(0.011)
Prior Mobilization	-0.083**	-0.092**	-0.097*	-0.103*	-0.090*	-0.076**	-0.083**	-0.092**	-0.097*	-0.103*	-0.090*	-0.076**
	(0.030)	(0.034)	(0.040)	(0.042)	(0.037)	(0.029)	(0.030)	(0.034)	(0.040)	(0.042)	(0.037)	(0.029)
Prior Insurgent Presence	0.015+	0.018+	0.020	0.022	0.037+	0.045	0.015+	0.018+	0.020	0.022	0.037+	0.045
	(0.009)	(0.010)	(0.012)	(0.015)	(0.022)	(0.034)	(0.009)	(0.010)	(0.012)	(0.015)	(0.022)	(0.034)
Distance to Province Capital	0.000*	0.000*	0.000+	0.000	0.000	0.000	0.000*	0.000*	0.000+	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence	0.083***	0.095***	0.097**	0.099**	0.088*	0.078	0.083***	0.094***	0.097**	0.099**	0.088*	0.078
	(0.024)	(0.027)	(0.035)	(0.036)	(0.044)	(0.048)	(0.024)	(0.027)	(0.035)	(0.036)	(0.044)	(0.049)
Distance to Em. Zone Border	-0.000+	-0.000+	-0.000+	-0.000+	-0.000	-0.000	-0.000*	-0.000+	-0.000*	-0.000+	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mita							0.003	0.003	0.004	0.003	0.001	0.000
							(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)
Constant	-0.012*	-0.012*	-0.012*	-0.012*	-0.011+	-0.014+	-0.015*	-0.015*	-0.015*	-0.014*	-0.013*	-0.014+
	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)
Second Stage (DV: Autodefensa Mobilization)												
State Violence (Instrumented by Em. Zone)	0.319*	0.380+	0.310*	0.327*	0.263*	0.281*	0.339*	0.436	0.345*	0.384*	0.304*	0.352*
	(0.149)	(0.214)	(0.133)	(0.144)	(0.116)	(0.114)	(0.171)	(0.282)	(0.167)	(0.192)	(0.151)	(0.159)
Prior Mobilization	-0.146	-0.164	-0.157+	-0.163+	-0.235**	-0.289**	-0.150	-0.174	-0.163	-0.173	-0.257*	-0.341**
	(0.091)	(0.107)	(0.090)	(0.097)	(0.087)	(0.095)	(0.098)	(0.126)	(0.102)	(0.116)	(0.106)	(0.129)
Prior Insurgent Presence	-0.001	-0.003	0.009	0.013	0.029	0.046+	-0.003	-0.007	0.008	0.011	0.028	0.048+
	(0.011)	(0.015)	(0.010)	(0.013)	(0.019)	(0.027)	(0.012)	(0.019)	(0.011)	(0.014)	(0.019)	(0.027)
Distance to Province Capital	0.000*	0.000+	0.000+	0.000*	0.000*	0.000+	0.000+	0.000	0.000	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior Insurgent Violence	-0.052	-0.068	-0.040	-0.042	-0.043	-0.053	-0.060	-0.092	-0.055	-0.065	-0.062	-0.083
	(0.070)	(0.099)	(0.068)	(0.057)	(0.056)	(0.057)	(0.078)	(0.127)	(0.080)	(0.075)	(0.069)	(0.072)
Distance to Em. Zone Border	-0.000+	-0.000	-0.000*	-0.000+	-0.000	-0.000	-0.000+	-0.000	-0.000*	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mita							-0.003	-0.007	-0.006	-0.011	-0.009	-0.015
							(0.003)	(0.007)	(0.006)	(0.008)	(0.008)	(0.011)
Constant	-0.011*	-0.011*	-0.009*	-0.010*	-0.008*	-0.011+	-0.008*	-0.005	-0.003	-0.000	0.000	0.001
	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)	(0.006)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.007)
BS Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	75km	65km	55km	45km	35km	25km	75km	65km	55km	45km	35km	25km
N	7295	6383	5276	4243	3335	2419	7295	6383	5276	4243	3335	2419
Clusters	263	240	206	163	138	104	263	240	206	163	138	104
Montiel-Pflueger eff. F-stat	3.999	2.586	4.551	4.017	5.290	4.369	3.708	2.089	4.007	3.406	5.021	4.112
Anderson - Rubin Test p-value	0.0476	0.0657	0.0586	0.0822	0.1144	0.1134	0.0471	0.0654	0.0622	0.0863	0.1202	0.1109

* p<0.05, ** p<0.01, *** p<0.001; standard errors clustered by district in parentheses.

Difference-in-Differences

In the difference-in-differences setup, the data are divided into two time periods: Pre-campaign (i.e., pre 1983; 1980-82) and post-campaign (i.e., post 1985; 1986-88). The effect of interest is (e.g., Angrist and Pischke, 2009, 221ff.; Khandker, Koolwal, and Samad, 2010, 71ff., 190):

$$E(Y_{ipost} - Y_{ipre}|SV = 1) - E(Y_{ipost} - Y_{ipre}|SV = 0) \tag{6}$$

where $SV = 1$ denotes exposure to state violence and $SV = 0$ non-exposure, and Y_i the outcomes during the pre- and post-campaign period (subscripts *pre* and *post*) respectively. I estimate the quantity of interest via the following regression, where the coefficient of the interaction term between the period indicator $Period_t$ and the state violence indicator SV_i indicates the average effect on the treated (ATT):

$$Y_{it} = \alpha + \beta(SV_i \times Period_t) + \delta SV_i + \phi Period_t + \epsilon_{it} \tag{7}$$

The crucial identifying assumption underlying the DiD strategy is that the outcome over time would have been the same or followed parallel trends for 'treated' and 'untreated' cases had no 'treatment' occurred (Angrist and Pischke, 2009). If there are initial conditions that affect both exposure to state violence and subsequent mobilization dynamics, this assumption is violated. While the parallel trends assumption cannot be explored in detail, the similarity in outcomes prior to the counterinsurgency campaign increases the confidence that this assumption is valid (see below). The DiD approach is combined with propensity score screening to focus on units with similar probability of exposure to state violence, and to increase precision (Angrist and Pischke, 2009; Crump, Hotz, Imbens and Mitnik, 2009).[4] Here, a few villages are dropped that were not targeted and had a probability of being targeted that was lower than among any of the treated units. The propensity score screening does not change the substantive results.

The propensity score is the conditional probability of being exposed to a treatment T given

[4]For an example of a difference-in-differences analysis with propensity score matching and weighting (instead of screening), see Van de Walle and Mu (2007).

a set of observable pre-treatment covariates (Rosenbaum and Rubin, 1983): $p(X) \equiv Pr(T = 1|X)$. As outlined in the article, several factors are expected to be associated with differences in exposure to state violence as well as diverging mobilization dynamics over time, and should therefore be included in the estimation of the propensity score. Historical case studies suggest that the quality of the relationship between insurgents and civilian communities could often be traced to roots that preceded the beginning of the war, such as the perceived legitimacy of communities' governing bodies and local institutions (La Serna, 2012). Moreover, qualitative evidence suggests that exposure to insurgent violence was a crucial trigger of civilian resistance from early on (e.g., Coronel, 1996). It is therefore important to identify those areas where the insurgents had the strongest presence and support by the end of 1982, as well as those villages and towns that had already fallen victim to insurgent violence prior to the onset of the counterinsurgency campaign. Both factors are likely to influence exposure to state violence and subsequent patterns of mobilization over time. Another important covariate to consider is the local capacity of state actors to project power, which is typically less pronounced in rural areas (Kocher, 2004; Kalyvas, 2006). Finally, it is crucial to identify those areas placed under military control at the very onset of the counterinsurgency campaign, i.e., those areas where the armed forces moved in first.

In addition to the combination of a DiD approach with propensity score screening, this appendix also presents the results of a non-DiD estimation approach that controls for pre-campaign mobilization.

Study Region and Data

Figure 10 provides a map of the study region of this second identification approach, the departments of Apurímac, Ayacucho, and Huancavelica.

Initial Military Rule: The data sources for this research strategy are the same as in the first research design. Information on emergency zone status is not an instrument in this research design, however, and is only measured at the very onset of the counterinsurgency campaign as an indicator for *Initial Military Rule*. The variable Initial Military Rule captures whether a given village was located in a region placed under emergency and military command once the armed forces entered, at the end of 1982. Areas under emergency and political-military command were coded as listed in Desco (1989, 345ff.) and as specified in the *decretos supremos* re-published in various volumes of the *'Normas legales - Revista de Legislación y Jurisprudencia,'* accessed in the *Archivo del Congreso de la República* (Editorial Normas Legales S.A., N.d.). Identified are those administrative units placed under political-military command during the first 60 days of the counterinsurgency campaign.

Descriptive Statistics and Propensity Score

Basic information on the distribution of pre- and post-campaign outcomes is given in tables 16 and 17. The analysis starts with a probit model that estimates the propensity score for all 12,336 *centros poblados* in the departments of Ayacucho, Apurímac, and Huancavelica, based on the variables discussed above (Prior Insurgent Violence, Prior Insurgent Presence, Initial Military Rule, Distance to Province Capital). Restricting the sample to the region of common support ensures that targeted villages ('treated units') are comparable to spared ones ('untreated units') in terms of their predicted probabilities of being exposed to state violence. 378 non-targeted units outside the region of common support are therefore dropped from the subsequent analysis. Tables 18, 19, 20, and 21 provide information on the estimated propensity score (propensity score I) that defines the region of common support as well as descriptive statistics for the main DiD analysis.

Figure 10: Centros Poblados in Study Region of DiD Design

Table 16: **Pre-Campaign Outcome**

	Freq.	Percent	Cum.
0	12,332	99.97	99.97
1	4	0.03	100.00
Total	12,336	100.00	

Table 17: **Post-Campaign Outcome**

	Freq.	Percent	Cum.
0	12,292	99.64	99.64
1	44	0.36	100.00
Total	12,336	100.00	

DiD Results

Table 22 gives an overview of the outcome distribution. Table 23 presents the results of the same DiD analysis as presented in the article, but with the sample reduced to *centros poblados* in Ayacucho, the epicenter of the civil war. Finally, table 24 shows the results with district fixed effects.

Pre-Campaign Outcomes and Additional Robustness Test

The key identification assumption underlying the difference-in-differences approach is that there is no unobserved heterogeneity that causes trends to differ between 'treatment' and 'control' units (Angrist and Pischke, 2009); in other words, it is assumed that the treatment and control outcomes would have developed similarly over time without treatment exposure. While the parallel trends assumption cannot be proven valid for good, typical tests explore pre-treatment trends. As the main interest here is on the initial phase of the war, it is not possible to explore long pre-campaign time trends in the outcome. Instead, table 25 reports a simple analysis of differences in outcomes between 'treated' and 'control' villages prior to the onset of the counterinsurgency campaign (1980-1982). If we would see an 'effect' of state violence here, this would be indicative of differences in the propensity for counterinsurgent mobilization between targeted and spared communities prior to the onset of the counterinsurgency campaign. The results do not indicate that there are such differences.

Table 18: **Probit Regression of State Violence (PS I)**

Prior Insurgent Presence	0.066
	(0.054)
Initial Military Rule	1.175***
	(0.057)
Prior Insurgent Violence	1.582***
	(0.101)
Distance to Province Capital	-0.000
	(0.000)
Constant	-2.435***
	(0.062)
Log-Likelihood	-1672.408
χ^2	1065.494
N	12336

* p<0.05, ** p<0.01, *** p<0.001

Table 19: **Propensity Score I**

Region of common support	.00630515 < p < .65111304
Obs. in region of common support	11,958
Mean	.044589
Std. dev.	.0804179
Number of blocks	7
Balancing property satisfied†	Yes (p < 0.01)

† Balancing property refers to first moment. Within each propensity score interval, the means of the covariates used to estimate the propensity score are balanced between treated and control units.

As an additional robustness check, a new propensity score is estimated based on the same variables as above, but including one additional variable measuring prior mobilization (i.e., pre-campaign outcomes). Tables 26, 27, and 28 present information on this alternative propensity score. The analysis is then repeated in a cross-sectional setup, with the post-campaign outcome as the dependent variable and the pre-campaign outcome as an additional covariate. Table 29 presents the results of this test. The estimation is again restricted to the region of common support. The effect of state violence remains positive and significant.

Table 20: **Blocks of Propensity Score I, common support**

Inferior bound	Control	Treated	N
.0063052	8,112	58	8,170
.05	971	106	1,077
.1	2,266	272	2,538
.15	13	6	19
.2	1	0	1
.6	59	94	153
Total	11,422	536	11,958

Table 21: **Descriptive Statistics (Common Support PS I; N = 11,958)**

Variable	Mean	SD	Min	Max
Autodefensa Mobilization (Pre)	0.000	0.018	0	1
Autodefensa Mobilization (Post)	0.004	0.061	0	1
State Violence	0.045	0.207	0	1
Initial Military Rule	0.315	0.465	0	1
Prior Insurgent Presence	0.141	0.348	0	1
Prior Insurgent Violence	0.014	0.119	0	1
Distance to Province Capital (Meters)	22676	12634	0	64693

Table 22: **Difference in Outcomes**

Dependent Variable (0/1)	**Mean**	**Std. Dev.**
Exposure to State Violence = 1		
Pre-Campaign Outcome	0.004	0.061
Post-Campaign Outcome	0.052	0.223
Difference	0.048	
N (Centros Poblados per Period)	536	
Exposure to State Violence = 0		
Pre-Campaign Outcome	0.000	0.013
Post-Campaign Outcome	0.001	0.037
Difference	0.001	
N (Centros Poblados per Period)	11422	
Difference-in-Differences	0.047	

Table 23: **DiD Estimates: Autodefensa Mobilization (Ayacucho only)**

	I	II	III	IV
Period	0.003**	0.003*	0.003**	0.003*
	(0.001)	(0.001)	(0.001)	(0.001)
State Violence	0.002	0.002	-0.005	-0.005
	(0.002)	(0.002)	(0.003)	(0.003)
Period x State Violence	0.053***	0.053**	0.053***	0.053**
	(0.011)	(0.016)	(0.011)	(0.016)
Prior Insurgent Presence			0.002	0.002
			(0.002)	(0.004)
Initial Military Rule			0.003*	0.003*
			(0.001)	(0.001)
Prior Insurgent Violence			0.034**	0.034**
			(0.013)	(0.011)
Distance to Prov. Capital			0.000**	0.000
			(0.000)	(0.000)
Constant	0.000	0.000	-0.006***	-0.006*
	(0.000)	(0.000)	(0.002)	(0.003)
Rsquared	0.032	0.032	0.040	0.040
Clusters	4520	111	4520	111
N	9040	9040	9040	9040

* p<0.05, ** p<0.01, *** p<0.001; clustered standard errors in parentheses.

Table 24: **DiD Estimates (OLS): District Fixed Effects**

	I	II
Period	0.001**	0.001**
	(0.000)	(0.000)
State Violence	-0.007	-0.007*
	(0.004)	(0.004)
Period x State Violence	0.047**	0.047***
	(0.015)	(0.010)
Prior Insurgent Violence	0.024*	0.024*
	(0.010)	(0.011)
Distance to Prov. Capital	0.000	0.000
	(0.000)	(0.000)
Constant	-0.002	-0.002
	(0.002)	(0.001)
Rsquared	0.025	0.058
Clusters	285	11958
N	23916	23916
District FE	Yes	Yes

* p<0.05, ** p<0.01, *** p<0.001.
Clustered standard errors in parentheses.

Table 25: **Pre-Campaign Outcomes**

	Pre-Campaign Outcome	Pre-Campaign Outcome
State Violence	0.004	0.001
	(0.003)	(0.003)
Prior Insurgent Presence		0.001
		(0.001)
Emergency Zone		0.000
		(0.000)
Prior Insurgent Violence		0.010
		(0.008)
Distance to Prov. Capital		0.000
		(0.000)
Constant	0.000	-0.000
	(0.000)	(0.000)
Rsquared	0.002	0.006
N	11958	11958

* p<0.05, ** p<0.01, *** p<0.001.
Robust standard errors in parentheses.

Table 26: **Probit Regression of State Violence (PS II)**

Prior Insurgent Presence	0.065
	(0.054)
Initial Military Rule	1.175***
	(0.057)
Prior Insurgent Violence	1.577***
	(0.102)
Distance to Province Capital	-0.000
	(0.000)
Prior Mobilization	0.440
	(0.643)
Constant	-2.435***
	(0.062)
Log-Likelihood	-1672.175
χ^2	1065.962
N	12336

* p<0.05, ** p<0.01, *** p<0.001

Table 27: **Propensity Score II**

Region of common support	.00630633 < p < .76752224
Obs. in region of common support	11,957
Mean	.0445189
Std. dev.	.0801537
Number of blocks	7
Balancing property satisfied†	Yes (p < 0.01)

† Balancing property refers to first moment. Within each propensity score interval, the means of the covariates used to estimate the propensity score are balanced between treated and control units.

Table 28: **Blocks of Propensity Score II, common support**

Inferior bound	Control	Treated	N
.0063063	8,112	58	8,170
.05	971	106	1,077
.1	2,265	271	2,536
.15	13	6	19
.2	2	1	3
.4	0	1	1
.6	58	93	151
Total	11,421	536	11,957

Table 29: **Pre-Campaign Outcome as Covariate**

	I	**II**	**III**	**IV**
State Violence	0.051***	0.051***	0.040***	0.040**
	(0.010)	(0.014)	(0.010)	(0.013)
Prior Insurgent Presence			0.002	0.002
			(0.003)	(0.005)
Initial Military Rule			0.005***	0.005***
			(0.001)	(0.002)
Prior Insurgent Violence			0.043*	0.043*
			(0.020)	(0.017)
Distance to Province Capital			0.000***	0.000
			(0.000)	(0.000)
Prior Mobilization			-0.045*	-0.045
			(0.023)	(0.024)
Constant	0.001***	0.001**	-0.005***	-0.005
	(0.000)	(0.000)	(0.001)	(0.003)
Rsquared	0.030	0.030	0.040	0.040
Clusters		285		285
N	11957	11957	11957	11957

* p<0.05, ** p<0.01, *** p<0.001

References

Angrist, Joshua D. and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics. An Empiricist's Companion*. Princeton, Oxford: Princeton Univ. Press.

Ball, P., J. Asher, D. Sulmont and D. 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.* Washington: HRDAG, AAAS.

Cattaneo, Matias, Michael Jansson and Xinwei Ma. 2018. "Manipulation Testing based on Density Discontinuity." *Stata Journal 18(1): 234-261, 1st Quarter 2018.* .

Comisión de la Verdad y Reconciliación. 2003*a*. "Documentación de las Tablas mue-des-est, dec-pen-est y actos-est." Informe Final. Lima: CVR.

Comisión de la Verdad y Reconciliación. 2003*b*. "Informe Final." Lima: CVR.

Comisión de la Verdad y Reconciliación. 2003*c*. "Intermuestra V1.2, Documentación 1.2 2003-11-10, Comisión de la Verdad y Reconciliación - Área de Sistemas de Información." Informe Final. Lima: CVR.

Coronel, José. 1996. Violencia política y repuestas campesinas en Huanta. In *Las rondas campesinas y la derrota de Sendero Luminoso*, ed. C. I. Degregori, J. Coronel, P. del Pino and O. Starn. IEP/Universidad Nacional de San Cristóbal de Huamanga (Estudios de la Sociedad Rural, 15) pp. 29–116.

Crump, Richard, Joseph V. Hotz, Guido Imbens and Oscar A. Mitnik. 2009. "Dealing with limited overlap in estimation of average treatment effects." *Biometrika* 96(1):187–199.

Degregori, Carlos Iván. 1996. Ayacucho, después de la violencia. In *Las rondas campesinas y la derrota de Sendero Luminoso*, ed. C. I. Degregori, J. Coronel, P. del Pino and O. Starn. Lima: IEP.

Dell, Melissa. 2010. "The Persistent Effects of Peru's Mining Mita." *Econometrica* 78(6):1863–1903.

Editorial Normas Legales S.A. N.d. "Normas legales. Revista de Legislación y Jurisprudencia." Trujillo: Editorial Normas Legales S.A.

EROS, U.S. Geological Survey's Center for Earth Resources Observation and Science. N.d. "30 arc-second DEM of South America.". Work licensed under a Creative Commons Attribution-Noncommercial 3.0 License `http://creativecommons.org/licenses/by-nc/3.0/`.

Finlay, K., Magnusson L.M. Schaffer M.E. 2013. "Weak-instrument-robust tests and confidence intervals for instrumental-variable (IV) estimation of linear, probit and tobit models." `http://ideas.repec.org/c/boc/bocode/s457684.html`.

Fumerton, Mario. 2002. "From Victims to Heroes: Peasant Counter-Rebellion and Civil War in Ayacucho." PhD Thesis. University of Utrecht.

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 Thesis. University of Oslo.

Hahn, Jinyong, Petra Todd and Wilbert Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69(1):201–209.

Imbens, Guido W. and Thomas Lemieux. 2008. "Regression Discontinuity Designs A Guide to Practice." *Journal of Econometrics* 142(2):615–35.

Instituto Nacional de Estadística y Informática. 2002. "Mapa de centros poblados del Peru. Cartografía Digital." Lima, Peru.

Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War*. Cambridge: Cambridge University Press.

Keele, Luke. 2010. "An Overview of Rbounds: An R Package for Rosenbaum Bounds Sensitivity Analysis with Matched Data." White Paper. Columbus, OH, 1-15.

Keele, Luke and Rocío Titiunik. 2016. "Natural Experiments Based on Geography." *Political Science Research Methods* 4(1):65–95.

Kocher, Matthew Adam. 2004. "Human Ecology and Civil War." PhD Thesis. University of Chicago.

La Serna, Miguel. 2012. *The Corner of the Living. Ayacucho on the Eve of the Shining Path Insurgency*. The University of North Carolina Press.

Lee, David S. and Thomas Lemieux. 2010. "Lee and Lemieux: Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48:281–355.

McClintock, Cynthia. 1999. The Decimation of Peru's Sendero Luminoso. In *Comparative Peace Processes in Latin America*, ed. Cynthia J. Arnson. Washington DC: Woodrow Wilson Center Press pp. 223–250.

PCM-DNTDT. 2011. "Identificación Relacional y Situación de Límites Territoriales de las Circunscripciones Politicas en el Perú." Presidencia del Consejo de Ministros - Dirección Nacional Técnica de Demarcación Territorial.

Pflueger, Carolin E. P and Su Wang. 2015. "A robust test for weak instruments in Stata." *The Stata Journal* 15(1):216–225.

Rosenbaum, Paul R. 2002. *Observational Studies*. 2nd ed. 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.

Sovey, Allison J. and Donald P. Green. 2011. "Instrumental Variables Estimation in Political Science: A Readers' Guide." *American Journal of Political Science* 55(1):188–200.

Van de Walle, Dominique and Ren Mu. 2007. "Fungibility and the Flypaper Effect of Project Aid: Micro-Evidence for Vietnam." *Journal of Development Economics* 84:667–685.

Van der Klaauw, Wilbur. 2002. "Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach." *International Economic Review* 43(4):1249–1287.