THE PERILS OF BUILDING STATES BY FORCE: How Attempts to Assert the State's Monopoly of Violence Create Lasting Incentives for Violent Banditry

Soeren J. Henn Christian Mastaki Mugaruka

Raúl Sánchez de la Sierra* Miguel Ortiz David Qihang Wu

July 20, 2021

*Mailing address: 1307 E 60th St, Chicago, IL 60637. Email: raul@uchicago.edu. Phone: 9174889151.
We thank Gauthier Marchais, Jean-Paul Zibika, and all members of the Marakuja Kivu Research team, for invaluable support. Especially: Desire Basibuhe, Christian Bazuzi, Aimable Amani Lameke, Simeon Lukeno, Floribert Lwaboshi, Emmanuel Kandate, Freddy Koleramungu, Salomon Salumu Kombi, Eustache Kuliumbwa, and Anne-Laure Van der Wielen. We thank Ernesto Dal Bo, Oeindrila Dube, Anne Karing, Luis Martinez, James Robinson, Vincent Tanutama, Haoyang Xie and Noam Yuchtman for comments. Sánchez de la Sierra thanks Robert Bates, Christopher Blattman, Pierre-André Chiappori, Donald Davis, Avner Greif, Macartan Humphreys, Suresh Naidu, Bernard Salanié for invaluable guidance. The data collection for this study was funded by the National Science Foundation Dissertation Improvement Grants (award number 1261025), Private Enterprise Development in Low-income countries, the International Peace Association Research Foundation, and the International Center for Taxation and Development. For two of these sources, funding originally came from the former United Kingdom's Department For International Development. The data was gathered in 2012, 2013, and 2015 by the current members of Markuja Kivu Research, non-profit data collection organization co-founded by Basibuhe, Bazuzi, Koleramungu, Lameke, Lukeno, Marchais, Mugaruka, and Sánchez de la Sierra. Word count: 13,216 words.

Abstract

This article provides evidence from a weak state that attempting to assert the state's monopoly of violence can create incentives for more, rather than less, violence against citizens. To establish this result, we combine information on the behavior of armed actors in 239 municipalities of eastern Congo with quasi-experimental variation induced by one of the largest military efforts to assert the state's monopoly of violence. The campaign successfully weakened targeted armed actors' ability to hold a monopoly of violence. However, the yearly incidence of attacks against the citizens in the targeted villages by the targeted armed actors increased from 9% to 33% for at least three years. The effect is muted for attacks by other armed actors. Using information on the purported motive of the attacks and household panel data, we quantify the value stolen and show that the attacks are violent theft operations. We provide suggestive evidence that the rise can be explained by the disruption in the armed actors' ability to frequently expropriate. Overall, the campaign increased household material welfare, driven by a decrease of informal taxes paid to the armed actors, but led to gruesome violence. We discuss alternative state-building strategies. The results suggest that building states by crushing internal opponents, key to the creation of European modern states, and a common justification for supporting weak states today, can create lasting incentives for violent banditry in weak states. This can undermine the potential welfare gains arising from such policies, and induce a trade-off between growth and safety. **JEL Codes:** H11, P48, D74

"Armed actors who do not control a village for a long period of time prefer to pillage. This is because, in that case, there is nothing for them to save."[1]

1 Introduction

Weak states pose many of the greatest development challenges. Motivated by the observation that taxation is the "hallmark of the state" (Besley and Persson, 2013), recent work has focused on strategies that improve weak states' capacity to tax, and govern, in the territory they control (Weigel, 2020; Balan et al., 2021; Bergeron et al., 2021). However, a central feature of weak states is that, *de facto*, they do not even hold a monopoly of violence in most of their *de jure* territory. For instance, in the Democratic Republic of the Congo (DRC), 95% of the area of a major district was controlled by armed groups until recently (Okapi, 2013). This article studies the unintended effects of attempts to assert the state's monopoly of violence, by examining one of the first military campaigns to do so in recent history.

The supposedly central role of holding a monopoly of violence is a standard narrative of the formation of the modern state in Europe. When rulers began to need more subjects to pay taxes, for instance to finance wars, they used force to crush competing power holders in their territory (Tilly, 1985; Elton, 1953). Most of those did not survive, and their subjects fell under the monopoly of violence of ever more centralizing rulers. The few who survived were later recognized as independent states in the international state system. Yet, a fundamental problem is that the competing power holders also create state functions themselves (Sánchez de la Sierra, 2020). Those are purportedly sustained through repeated interaction with citizens, making them residual claimants of citizens' welfare through frequently expropriating them (Olson, 1993). Although Sánchez de la Sierra (2020) and Olson (1993) did not provide evidence for this mechanism, it implies that attempts to remove challengers to the state can disrupt the very mechanisms that discipline their use of violence. The contrast between "stationary bandits" and violent "roving bandits" captures the intuition.

[1]Interview with armed group member, Sud Kivu, Democratic Republic of the Congo, 2013.

Given the desire to build states around the Western notion of a monopoly of violence, donors and policy makers have invested huge resources to strengthen weak states (Stearns, 2011; LSE-Oxford Commission on State Fragility, Growth and Development, 2017). However, we lack rigorous evidence on the effect of attempts to assert the state's monopoly of violence, because there is typically no data from areas not yet controlled by established states. It is also not obvious that competing armed groups could seamlessly be removed by force. If they are motivated by expropriation, efforts that reduce their ability to regularly expropriate from the populations they govern may transform them into a large number of "roving bandits" with no incentives to restrain violence and regularly stealing and predating on citizens.

We examine this question in the context of a large state-led operation in the province of Sud Kivu in 2009, Kimia II, aimed at gaining the monopoly of violence over armed groups, most notably various units of the Forces de Libération du Rwanda (FDLR). A number of characteristics of the DRC make the environment well-suited to elicit the response of armed groups to attempts by the central state to regain its monopoly of violence by force. First, the central state struggles to control most rural areas, enabling more than 122 armed groups today to develop their own state functions (KST, 2021). Second, armed groups in eastern DRC have a large number of village factions, which enables village-level analysis.

We first document that, by 2009, the armed factions targeted by Kimia II controlled most villages in the Chiefdom of Basile (henceforth, targeted villages), an area as large as Rhode Island, in South Kivu province. Using data presented in Sánchez de la Sierra (2020), we show that, each day, they collected taxes, maintained security, and ran fiscal and judicial administrations in the targeted villages. In contrast, despite their tendency to use violence against civilians in this period in the region, violence was almost absent in the villages they controlled. Remoteness of the Chiefdom could have played a role in enabling this stability.

We then show that their incentives to refrain from violence against citizens were disrupted when they were no longer able to frequently expropriate these villages. Specifically, we exploit the targeting of Kimia II, led by 30,000 Congolese and UN soldiers, which permanently weakened their ability to access these villages. We exploit the timing and targeting of Kimia II in an event study framework. We estimate, for each year, the difference in the incidence of attacks by the targeted factions against the targeted villages to that in the rest and examine whether that difference increased after 2009. We also estimate Differences-in-Differences and Borusyak et al. (2020)'s robust and efficient imputation estimator. Our analyses show that Kimia II is associated with an average increase in the likelihood of attacks by the targeted factions by 267% in the targeted villages. Attacks were unaffected in the rest of villages.

We then examine potential confounds that could threaten interpretation of this reduced form relationship: spatial spillovers, differential time-trends, aggregate coincidental shocks, intra-Chiefdom correlation of violence, time-varying divergence along constant characteristics, migration, and mis-measurement. We use migration data, and publicly available data on infrastructure in a simple Differences-in-Differences framework. We also perform various specification checks and validate our results using ACLED (2020). All alternative specifications confirm the reduced form relationship, and we find no evidence of confounding.

One concern with this analysis is that Kimia II could have led to a change in the dynamics of violence in the region. Namely, Kimia II might have affected the incidence of their attacks indirectly through its effect on the industrial organization of violence. Given potential concerns about this assumption, we conduct the following falsification exercises. We use data on violence by all other armed actors to provide evidence that Kimia II does not affect the incentives violence by the targeted factions through its effect on other armed groups' violence. We find that violence by other armed actors in the targeted villages is unaffected. We also find that it is unaffected in the villages the other armed actors controlled the year prior to the campaign. This suggests that Kimia II directly led to the rise in attacks.

Our analysis then turns to potential causal channels. Our qualitative interviews suggest that the attacks are driven by expropriation of household assets. Namely, following Kimia II, the armed actors formerly holding a monopoly of violence in those villages disbanded to the neighboring forests. From there, in the following years, when they had an opportunity to expropriate, they did so through quick violent operations aimed at stealing all possible assets (henceforth, pillage), rather than requesting a small payment. We use our information on each individual attack in the data to support this mechanism. Attacks can be motivated by conquest, punishment, or pillaging. The average value of goods stolen in one pillage yields the same revenue as 3.77 months of daily tax collection in the village. We find that the attacks induced by Kimia II are exclusively pillages. Consistent with this conjecture, using household panel data, we find that the rise systematically targets the richest households.

Yet, before Kimia II, while these armed actors could also violently expropriate all assets in any day if they wanted to, they refrained from doing so. Why, and why did that stop when the state asserted its monopoly of violence? This is a difficult question, even under ideal circumstances. Our task is made especially challenging because the armed actors are secretive organizations whose actions are subject to large amounts of speculation. Thus, the answer we offer is suggestive rather than conclusive. Supported by our qualitative interviews, we conjecture that refraining from maximal expropriation was an investment in future expropriation revenues. Namely, Kimia II may have incentivized the armed actor's incentive to pillage by reducing the frequency with which the armed actors could expropriate in the future. Consistent with this, our data shows that the frequency of expropriation events (pillages or tax payments) drastically shrinks following Kimia II in the targeted villages. We rationalize this intertemporal trade-off in a simple model. A key assumption is that higher expropriation today undermines future expropriation potential. Confirming this assumption, we find that a pillage permanently reduces household assets in subsequent years, undermining the armed actor's future ability to expropriate. This provides suggestive support for this mechanism. However, Kimia II may also have decreased the armed actors' revenues, creating an income effect that could have independently incentivized pillaging. Supporting this alternative, we find that the total yearly revenues of the armed actors in the sample of targeted villages shrinks from 300,000 to 100,000 USD in one year. Yet, if the rise in attacks is explained by a group-level income effect, attacks should rise in all nearby villages. Conversely, if it is due to expected expropriation frequency, attacks should only rise in villages in which expropriation frequency shrinks that year. We find that they only rise in villages with reduced expropriation frequency, consistent with the expropriation frequency channel.

We then consider leading alternative mechanisms. We analyze retaliation by the targeted armed actors and fighting with the Congolese army. We find that Kimia II led to no rise in attacks motivated by territorial control, nor by punishment of villagers. Furthermore, we find that attacks do not target the Congolese army positions, nor households who potentially collaborated with the state or who potentially accumulated debt with the targeted armed actors. This suggests that these attacks are unlikely to be explained by war or by retaliation.

We then examine the implications of Kimia II on household welfare. We use detailed information on each village history of violence, pillaging, taxes, and violence. We find that Kimia II drastically intensified sexual violence, theft, and abductions (all perpetrated during the pillages). However, it decreased the total value expropriated per household, from 25.5% to 15.5% of yearly per capita income and the fraction of unemployed. Consistent with higher incomes, it also increased the fraction who get married and who are in school—which require the payment of a bride price and a school fee, respectively. This suggests that Kimia II improved material welfare by decreasing expropriation and incentivizing labor supply into profitable occupations. However, given the violations of human dignity it caused, utilitarian criteria based on material outcomes are insufficient for making welfare statements.

Recognizing that force is in limited supply, we then account for possible general equilibrium effects of such policies. Specifically, we ask what is the effect re-allocating military resources from other areas to support these operations. We separately analyze the implications of the creation of a state vacuum as a result of Kimia II in the district of Shabunda—which later intensified as a result of a military restructuring. At the time of the operation, the fraction of villages in the district of Shabunda protected by the state shrinks. Furthermore, in 2011, following the Kimia II campaign, a military restructuring ordered the army units in Shabunda to relocate. This created a persistent state vacuum, providing a natural experiment to measure the effects of removing the state's monopoly of violence. Exploiting changes in armed actors' activity in Shabunda and elsewhere, we find that removing state forces in Shabunda leads to the creation of an entirely new armed group, the Raia Mutomboki, who performed state functions themselves in the territory vacated by the state. This suggests that dismantling armed actors' monopolies of violence in one area can come hand in hand with the creation of new monopolies of violence in areas where state force is reallocated from. While Kimia II was supported by United Nations' troops, and thus its demand on resources from other areas is uncharacteristically low, this analysis suggests that asserting the state's monopoly of violence by force in an area requires weakening it in other areas, which can create new monopolies of violence, weakening future state capacity in those areas.

Finally, we discuss alternative policies to building states by force. Rather than dismantling armed groups, weak states can offer armed groups the benefits of integration into the state in exchange for handing over the monopoly of violence in their region to the central government. This policy is common in historical accounts of the formation of non-Western states. For instance, it is a central driver for the creation of the Ottoman Empire (Newbury, 2000). It has the advantage that it does not create violent roving bandits nor creates security vacuums. We informally discuss the effect of the Sun-City peace agreement of 2004, which led to the integration of armed groups into the state monopoly of violence. We find that the agreement led to a reduction in violence and to the removal of non-state armed groups, without creating new roving bandits nor reallocating state force from other areas. Leaving aside implementation challenges that are beyond the scope of this paper, we suggest that this more realistic approach provides a promising avenue for building state capacity.

Our study complements the literature on state capacity building. To build effective states, some scholars have focused on strategies that increase the ability of the state to tax in the areas that they control (Weigel, 2020; Balan et al., 2021). However, a persistent feature of weak states is that, de facto, they do not even control most of their *de jure* territory. How can a state even tax those areas? Complementing an emerging literature, we document the success of state efforts to weaken non-state armed actors (Ferraz et al., 2021; Blattman et al., 2021), but we show how those attempts can disrupt pre-existing equilibria, potentially being counter-productive—in line with Acemoglu and Robinson (2013) argument of "political equilibria." Specifically, Sánchez de la Sierra (2020) showed that, in that context, other armed actors can develop state functions themselves. We show that weakening them without having strong capacity can create a huge cost to society. It can break underlying relationships between them and citizens that make them internalize citizens' well being and that discipline their use of violence. Some weak states may have the military capacity to dismantle some armed groups but, like any state in recorded history, they struggle to prevent violence and theft. Analyzing the effects of one episode of integration, we suggest that weak states' efforts could potentially be more productive if they focused instead on how to manage successful integration of armed groups into the state's monopoly of violence.

Our study also introduces a new economic explanation for violence. The literature on the causes of violence is rich across disciplines (Wood, 2006; Kalyvas, 2006; Blattman and Miguel, 2010; Balcells, 2012; Reno, 2011; Arjona and Mamphilly, 2015; Arjona, 2016). Yet, inpsired by Becker (1968), economists typically explain violence, and armed conflict specifically, as reflecting a occupational transitions between production and an "appropriation" sector presumed to be always violent (Dal Bó and Dal Bó, 2011; Dube and Vargas, 2013). We show that variation in the type of appropriation can play an important role to explain violence and that lower constraints to expropriation decrease, rather than increase, violence.

In doing so, we present suggestive evidence that the value of the future, which has been proposed as an explanation for behavior in macroeconomics (Gollier, 2002), corporate governance (Shleifer and Vishny, 1990), corruption by elected bureaucrats in democracies with the rule of law (Ferraz and Finan, 2011), and agricultural investment (Yamasaki, 2020), is an important factor in state formation, where it has so far been conjectured as a mere theoretical possibility (Olson, 1993; Bates et al., 2002). Yet, the extensive margin states can have important implications for state collapse, affecting the foundations of markets.

2 A Brief History of the FDLR and Kimia II Campaign

Since the Congolese Wars (1996–2003), armed actors from Rwanda have settled in rural areas of North and South Kivu. To sustain their operations, they created basic state functions and taxation systems at the village-level, which they use to extract resources from the rural population for their survival. In general, while their relationships with Congolese populations are complex, they typically have limited interested in the well-being of such populations (Vlassenroot, 2002). One of those groups is the Front De Libération du Rwanda (FDLR).

The FDLR emerged from former Rwandan armed forces and militia members that perpetrated the 1994 Rwandan genocide. Their ideology is related to grievances in their country of origin, Rwanda.[2] They are known as one of the most brutal, and one of the largest armed groups in eastern DRC (KST, 2021). They often engage in gruesome violence against civilians.[3] While they have a few parallel structures of command and have since then split today, the village-level strategies of each faction have been relatively decentralized in the period.

Yet, starting around 2005, some of their units began to settle, and by 2009 were established in the Chiefdom of Basile (South Kivu province), an area of 3,113 km^2, which they controlled entirely (henceforth, "FDLR state"). Despite their violent reputation, the units stationed in the FDLR state provided protection, and expropriated daily, which they did through taxes. They taxed markets, collected transit toll fees, poll taxes, and mining taxes. Florquin and Debelle (2015), using internal FDLR documents, shows that the revenues from market taxes and various other sources of income were self managed by each FDLR's Company, Battalion and Sector. Arbitrary violence was low. This suggests that, each day, they expropriated less than they otherwise could in each village, i.e., they refrained from maximal expropriation and instead expropriated small amounts, with high frequency.

[2]For instance, Florquin and Debelle (2015) write: "The FDLR officially strives for peace and reconciliation in Rwanda and the Great Lakes region . . . The FDLR's key demand was the opening of 'dialogue with the Rwanda government and reform of the Rwandan security forces permitting FDLR representation at a leadership level' . . . leaders have stated more clearly their intention to overthrow the Rwandan government, to pardon the actors of the genocide, and to create a Hutu-majority government."

[3]Appendix A provides more details on the origins of the FDLR.

This apparent stability was disrupted by a military operation which undermined their ability to regularly expropriate in the FDLR state villages. In March of 2009, the Congolese army, with support of the UN peacekeeping forces (MONUSCO), launched a joint operation against the FDLR named Kimia II. Kimia II had a spatial target: the FDLR state in the Province of Sud Kivu.[4] Twenty-two thousand Congolese soldiers, and 8,000 MONUSCO soldiers participated. The FDLR as a whole was believed to have around 6,000 soldiers in 2008 (Florquin and Debelle, 2015). From March to December 2009, Congolese and UN forces advanced from north to south and weakened the ability of the FDLR to regularly expropriate the villages of the FDLR state. Congolese army operations continued through 2012, aimed at holding territory that had been taken from the FDLR. Yearly maps of armed groups show that the Chiefdom of Basile remained free of FDLR until today (Vogel, 2021).[5]

In response to Kimia II, the FDLR units settled in the neighboring Itombwe forest, from where they regularly attacked the nearby villages for the following years (Sawyer and Van Woudenberg, 2009; Levine, 2014). Illustrating the brutality of the FDLR response to the Kimia II operations, the UN Group of Experts documented 1,199 human rights violations committed by the FDLR between February and October 2009, including 384 killings, 135 cases of sexual violence, 521 abductions, 38 cases of torture, and 5 cases of mutilation (UNSC, 2009). The attacks targeted the very same villages they were protecting prior to Kimia II (source: ICC (2012)):

> "A number of the victims of abuses had clearly been able to identify their attackers as FDLR since they knew them by name and had lived side-by-side with them for many years"

Experts of the DRC have attributed the rise in violence by the FDLR in the FDLR state

[4]Prior to Kimia II, in January of 2009, the Congolese armed forces and the Rwandan army launched a joint operation against the FDLR in North Kivu, named Umoja Wetu. The operation was not militarily successful and the Rwandan forces quickly withdrew.

[5]See Appendix G for a review of all yearly maps. While no data like the one described in this paper exists for after 2013, the maps, based on qualitative reports from the field, provide sound evidence that the FDLR was permanently eradicated from the Basile Chiefdom.

to Kimia II. Levine (2014) suggests that Kimia II actually created such incentives:

> "military operations to eliminate the FDLR created incentives for increased abuse of civilians . . . the level of violence against civilians seen in Kimia II also represents a failure to take into account the ways in which military operations change the dynamics of violence against civilians, and the critics who endorsed the goal of eliminating the FDLR by force also seemed to dismiss that danger"

In what follows, we first show that Kimia II caused FDLR attacks to rise in the FDLR state villages. Then, we propose an explanation for this rise.

3 Description of Data

3.1 Historical village data

The historical village data for this paper come primarily from original data collected for this analysis, first presented in Sánchez de la Sierra (2014, 2020). This is a yearly panel of 239 villages in North and South Kivu, from 1995 to 2013. Section B.1 discusses in detail why this dataset is preferred to publicly available data for the period. For robustness, we replicate the analysis that follows using the best public violence data in Appendix D.

Our data was collected between June 2012 and September 2013 in South Kivu, and between June 2015 and June 2016 in North Kivu. We randomly sampled 133 villages in South Kivu and 106 villages in North Kivu from the list of economically significant villages.[6] In each village, a team of two researchers team worked for one week, reconstructing the history of the village. The researchers lived with the community during one week. In that week, they built ties with the community, collected vast qualitative information about the

[6] Because no census has been conducted in recent years, in a first data-gathering round, we deployed teams of researchers to spend weeks in the district capitals (so-called Chiefdoms) and the lower-level districts (so-called groupements) to draw lists of all villages in each district with the help of state and customary administration. In those lists, we identified the villages that had a valuable resource—the rest typically had less armed group activity. In North Kivu, valuable resources included minerals, coffee, beans, and cacao. In South Kivu, they included only minerals.

history of the village, and worked every day with the most knowledgeable persons they identified in the village (henceforth, history specialists) and households.

The researchers recovered the villages' history since 1995.[7] The data include the following yearly information for each armed actor: whether they controlled the village (monopoly of violence), the taxes they raised, and whether they held a fiscal or a judiciary administration. Armed groups in eastern DRC always tax a subset of the following taxes: poll taxes (capitation taxes, collected per household or per adult in a regular time interval), market taxes (taxes for using the market stands), toll fees (fees to be paid each time an individual enters or exits the village), mill taxes (taxes for using the local mill), as well as taxes on various mining activities. Given our sample comprises rural households, most of those are payments to armed actors, some to the Congolese army.

The data also include a detailed description of the main violent operations on the villages since 1995 (henceforth, attacks). This includes information about the perpetrators of the attack, the purpose of the attack (mostly pillaging, i.e., economic motives, punishment, or conquest operations), whether the security forces were present in the village at the time of the attack, and the actions the perpetrator took during the operation.

We use this data, and the number of households in each village, to recover the average household level transfers to armed actors. Since we obtained information about the level and frequency of each tax collected in the village, we could reconstruct the yearly total tax payments per household.[8] Since we reconstructed detailed inventories of goods and assets stolen in each village during each attack that took place in the village, as well as yearly asset prices, we could reconstruct the yearly assets lost to theft per household.[9]

[7]Sánchez de la Sierra (2020) describes the data collection.

[8]For the poll tax, we obtained information, for each year and armed actor collecting it, whether it was raised by household or by person, the frequency, and the amount. This allows us to compute the yearly poll tax payments per year. For the rest of taxes, we obtained the level of each fee, and we use qualitative information to assume frequency about the usage of certain services, hence the frequency of tax payments. For toll fees, we assume that, for each household, one individual transits in and out of the village once per week. For the market tax, we assume that each household pays the tax once per week. For the mill tax, we assume that each household pays the mill tax once per month. The estimates are insensitive to the assumptions about usage frequency, since those are constant across households and time periods.

[9]The researchers, with the help of the village history specialists, established detailed lists of stolen assets

3.2 Historical household data

We combine the historical village-level data with historical household data that we collected as part of this data collection effort. Our household panel dataset comprises 1,537 households living in the villages for which we collected village historical data. We randomly sampled eight and six households in each village in South and North Kivu, respectively.[10]

In each selected household, the researcher randomly matched to that household conducted a day-long conversation with one randomly selected male adult respondent.[11] The conversation comprised breaks for lunch, informal talk, ethnographic in-depth data gathering, and it reconstructed the household's and the respondent's history, dating back to at least 1995. Working with each respondent, the researcher reconstructed the respondent's history of migration, marriage, occupation. In addition, they reconstructed the detailed history of attacks on the household, asset ownership, and investment for the household. Section B.2 provides additional details for the collection of cattle historical data.[12]

Reconstructing a household's history based on recall is subject to classic measurement error, and to strategic mis-reporting. To address these challenges, we used established methods in recall studies from eyewitnesses and, based on three months of piloting, tailored them to the cultural context. Section B.3 describes the measures taken to tackle measurement error. Section B.4 describes the details of data collection for obtaining information about when any member of the household has been victim of an attack by an armed actor, and for the respondent's occupational history, including participation into militia. Section B.5 provides a summary of the main variables collected.

We combine these data with geographical data from RGC (2010) to construct covariates.

---

in each attack. Separately, they also reconstructed the history of prices of basic assets each year.

[10]The sampling frame was obtained from, and in some cases elaborated with the village authorities.

[11]The researchers and the respondents are all male due to security considerations.

[12]Prior to gathering information, we obtained authorizations from provincial, territory, and village authorities. Ethical guidelines were followed to ensure that respondents did not feel obliged to participate.

3.3 Descriptive statistics

Figure I shows the times series of the fraction of villages attacked in the sample since 1995. Panel A breaks it down by perpetrator: the FDLR, Congolese militia, or other armed groups (which include armed groups backed by neighboring countries). Clearly, the period after Kimia II, marked as a light gray background area, is associated to a rise in the overall fraction of villages that are attacked by the FDLR, but not by other groups. As we will show in our analysis, this rise is concentrated in villages of the FDLR state, the villages targeted by Kimia II. The period of the First (1996-1997) and Second (1998-2004) Congolese Wars, denoted as a dark gray background area, is marked by high incidence of violence by all the armed actors. For this reason, we narrow down the study window to the years 2005-2012, after the Congolese Wars. The start of the study window is denoted by a red, vertical line. Panel B shows a similar pattern for the fraction of villages in which violent actions take place as part of an armed group attack on the village. During the Congolese Wars, abductions, sexual violence, and deaths are present in a large fraction of villages. Their incidence decreases with the end of the war, marking the start of the study window.

Figure II shows the location of the the FDLR and the Congolese army in the year before Kimia II (2008). Clearly, the FDLR state villages in our sample were all under the control of the FDLR, and the Congolese army is absent in the area. Table I presents the mean characteristics, FDLR state functions, and security outcomes for all sample villages, FDLR state villages (*FDLR State*) and the rest (*Rest*) in the years leading up to Kimia II.

Panel A shows that none of the villages in the FDLR state was accessible by car, and 78% were only accessible by foot. They were 29 pp. less likely to have phone coverage. The closest river and road were 1.35 km and 1.29 km farther away than the rest of villages, respectively. They were 44 pp. and 21 pp. more likely to have coltan or gold, respectively. They had lower levels of migration yearly. The FDLR conducted at least one expropriation (pillaging or tax payment) in 348 days per year on average, nearly zero in the rest. With opportunities to frequently expropriate, stealing all possible assets of the village whenever possible may be less profitable than expropriating at low rates and providing protection.

Indeed, Panel B shows that, prior to Kimia II, the FDLR units held a monopoly of violence in all these villages, and had a justice and a fiscal administration in 94% and 83% of these villages, respectively. They collected poll taxes, toll fees for transit of persons in and out of the village, taxes on the local market, and taxes on the village mill in 94%, 83%, 28% and 0% of the FDLR state villages, respectively (col. *FDLR State*), where the average level of a poll tax, for instance, is 1 USD per payment. They collected none of those taxes outside the FDLR state in the villages in the sample (col. *Rest*). Overall, this taxation system implied that each household paid, on average, 64.97 USD yearly to the FDLR in taxes. Using World Bank (2021) estimates of adjusted net national income per capita in DRC in 2006 in current USD, this implies that each household paid 25.5% of national yearly per capita income in taxes to the FDLR. The FDLR also provided security against other armed actors. Panel C shows that the frequency of violent operations against the village (henceforth, attacks) by any armed actor (6%) was half the level of that in the rest of villages.

4 Analysis

We examine the relationship between the timing and targeting of Kimia II and FDLR attacks by estimating the following Equation using OLS with two-way (village and year):

$$Y_{i,t} = \alpha_i + \beta_t + \beta_t^{NK} + \sum_{k=-4}^{k=3} \beta_k \, \mathrm{FDLR}_i \times 1(t = 2009 + k) + \epsilon_{i,t} \tag{1}$$

where α_i, β_t, β_t^{NK} are respectively village, year, and year-North Kivu province fixed effects.[13] $FDLR_i$ indicates whether village i was controlled by the FDLR *in 2008*.[14] $1(t = 2009 + k)$ is an indicator for whether the year is $t = 2009 + k$. The dependent variable $Y_{i,t}$ is an indicator for whether the FDLR attacks village i in year t. We present the results with standard

[13] That is, we estimate year effects separately for both provinces for robustness to province specific shocks.

[14] The FDLR controlled all villages in Basile and one in the neighboring Chiefdom of Wamuzimu. In our main analysis, $FDLR_i$ also takes value 1 for that village. Coding it as zero leaves results unaffected.

errors clustered two-ways at the village level and the Chiefdom-year level. The analysis using whether the FDLR is taxing village i in year t reflects the military effectiveness of Kimia II, hence our analysis focuses on the FDLR's attacks against citizens in those villages.

To shield the non-FDLR state comparison group from contamination arising from spillovers, in our main analysis, we exclude the villages of Shabunda (the Chiefdom of Bakisi, in our sample). These are villages vacated by the Congolese army during and after Kimia II, creating interference arising from a drastic and permanent decrease in state force. We henceforth refer to those as *Spillover Villages* and to the remaining sample as the quasi-experimental sample. For robustness, in our main analysis, we also show the results with spillover villages.

To test whether Kimia II increased FDLR attacks, we seek whether $\beta_k > 0$ if $k > 0$. To suggestively document the speed with which frequent expropriation may reduce violence, we also seek whether for $k < 0$, β_k is decreasing. Our analysis focuses on the years 2005 through 2012, which contains the time window of creation of the FDLR state, the Kimia II campaign, and all the years after the campaign until the end of our sample.

4.1 Event study estimates

We first visually examine the correlation between the timing and targeting of Kimia II FDLR attacks. Figure III shows (a) Congolese army territorial control and FDLR as well as non-FDLR, non-Congolese army taxation and (b) attacks for each year between 2005 and 2012. Panel A confirms that Kimia II was successful in removing the FDLR: after 2009, the Congolese army gained control over most FDLR state villages. Simultaneously, while the FDLR held a monopoly of violence in 100% of the FDLR state villages in 2008, they only did so in 10% of those villages by 2011. In this period, monopolies of violence by other armed actors remained relatively absent in the quasi-experimental sample. This provides reassuring evidence that Kimia II does not interfere with other armed actors. Panel B suggests that Kimia II led to a rise in FDLR attacks in the FDLR state. Before Kimia II, attacks by FDLR in the FDLR state villages steadily decreased, consistent with frequent expropriation progressively creating incentives to refrain from violence. After Kimia II, the FDLR's attacks skyrocketed in FDLR state villages, and remained constant in the rest.[15] In contrast, attacks by all other armed actors remained constant both in FDLR state villages and in the rest. Neither the FDLR state villages nor the rest have pre-existing trends in FDLR attacks. Having documented that Kimia II was successful at preventing FDLR taxation, our analysis in what follows focuses on the effect on the FDLR's incentives to use violence on citizens.

We present the event study estimates and confidence intervals of Equation 1 for incidence of FDLR attacks in Figure IV. The base year is 2009. The coefficients are positive for each year in the sample after 2009. The difference is statistically significant at the 1% level for each year after 2009. This suggests that the military campaign against the FDLR had a large positive and significant association with subsequent attacks by the FDLR against the villages the FDLR previously controlled. Figure IV also shows that the coefficients β_k are decreasing in k prior to Kimia II and are indistinguishable from zero in the three years leading up to Kimia II. This is consistent with frequent expropriation progressively dis-incentivizing violence by the FDLR.

4.2 Differences-in-differences estimates

In this section, we present the results using Differences-in-Differences. We use the Differences-in-Differences estimate as benchmark against which we analyze the role of potential confounders, and henceforth refer to this estimate as our main coefficient. We estimate the following Differences-in-Differences coefficient β^{DD}, where α_i^{DD} are village fixed effects, and

[15] Even though Congolese Army presence in a village was able to prevent the FDLR from taxing, it could not prevent them from pillaging. Figure E.5 shows that one third of the pillages occur in villages controlled by the Congolese army, during which time the Congolese army was absent during the attack, another third takes place in villages not controlled by the army. Our attack data show that attacks that take place when the village security force is present are 84% at night, against 64% if not. Our data indicate that forced labor occurs in 78% of attacks in Basile by the FDLR. In 77% of those events, the use of forced labor was reported to be used for transporting goods. We find that, while forced labor was used by the FDLR in Basile in 80% of attacks in which the security force was absent, it was only used in 41% of cases in which it was present.

β_t^{DD} and $\beta_t^{DD:NK}$ are the North Kivu province year fixed effects:[16]

$$Y_{i,t} = \alpha_i^{DD} + \beta_t^{DD} + \beta_t^{DD:NK} + \beta^{DD}\ FDLR_i \times 1(t > 2009) + \epsilon_{i,t}^{DD} \tag{2}$$

We present the Differences-in-Differences estimates for the effect of Kimia II on FDLR attacks in Table II. The sample is composed of 1,544 village*year observations in 2005–2012. Column (1) indicates that Kimia II led to a 24 pp. increase significant at the 5% level, from a mean of 9%, that is, to a 267% increase in the incidence of FDLR attacks.[17] This suggests a large and significant effect of being targeted by Kimia II.

However, both for the event study and the Differences-in-Differences, this does not necessarily identify the causal effect of Kimia II exposure on FDLR attacks. It is possible that the assumptions of the event study and Differences-in-Differences estimation do not hold. For instance, the rise in the coefficients after 2009 may reflect a general rise in the district, or in similar villages. It is also possible that attacks after Kimia II in the FDLR state may not be as prevalent, on average, as the entire period before. To address these concerns, we consider each of the potential confounders for this relationship. Column (2) adds the sample of spillover villages. Column (3) adds indicators for each district (administrative units called Territoire, in DRC) interacted with indicators for years as controls (there are ten districts in the sample). While visual inspection of Figures III and IV showed that differential time trends is unlikely to be a concern, Column (4) adds, as control, a linear time trend for each village. Column (5) adds, as control, the predicted probability that a village belongs to the FDLR state, interacted with indicators for years. To estimate the predicted probability that a village belongs to the FDLR state, we estimated a probit model for an indicator for whether the village is in the FDLR state on all variables presented in Table I.[18] Column (6) adds the number of immigrants and emigrants as control. Columns (7) and (8) include all

[16]The result is preserved if we use $1(t \geq 2009)$ instead.

[17]We also computed the p-value accounting for intra-Chiefdom correlation. Since there are 21 Chiefdoms, we used wild bootstrap. The p-value in that case is .02.

[18]We used indicators for whether the village is accessible by road, motorbike, by phone, distance to Rwanda, the closest river, the closest road, the closest airport and whether the village has coltan or gold.

of the previous controls for the main sample and the sample including the spillover villages, respectively. The coefficient is unaffected by each of these controls and alternative samples.[19]

Appendix C presents a series of robustness tests. Table C.1 replicates the analysis of the predicted probability that a village belongs to the FDLR state for each covariate separately. In Table C.2, we estimate Equation 2 using, instead of village and year fixed effects, indicators for $I(t > 2009)$ and for $FDLR_i$ as controls (1), excluding the only Chiefdom of the sample that produces a negative coefficient if coded as $FDLR_i$ (2), clustering the standard errors at a higher level (3), including controls for the world price of coltan or gold interacted with an indicator for whether village i has coltan or gold, respectively (4,5), controlling for the logged distance to the FDLR state interacted with year indicators (6). Column (7) includes $I(t > 2009)$ interacted with the the logged distance to the FDLR state and shows that the effect is concentrated in the FDLR state and the surrounding areas and dissipates over space, consistent with the FDLR state units displaced to the local forest of Itombwe and attacking all neighboring villages–including those of the FDLR state. In Table C.3, we omit village fixed effects and control for the lag of the dependent variable. Figure C.1 conducts two falsification exercises for the validity of the specification choice. Panel A estimates Equation 2 using each year of 2005–2011 as cutoff. Panel B separately estimates Equation 2 using each Chiefdom in sample as an indicator for FDLR state–even as there is no FDLR presence in the other Chiefdoms, nor are those targeted for Kimia II. Both exercises show that the results cannot be replicated with alternative codings for the targeting of Kimia II.[20] Appendix D estimates Equations 1 and 2 using data from ACLED (2020) including a variety of robustness checks. The findings are robust to each of these specifications.

[19] We also implemented Borusyak et al. (2020) estimator. The estimate is also .24 and the p-value 0.000.

[20] Another concern with this estimation is that the number of targeted villages is small. To examine whether this is concerning for our analysis, we simulated 10,000 random assignments of FDLR state to villages, holding the fraction of targeted villages constant. For each simulation, we estimated Equation 2. The fraction of coefficients that is larger than our main coefficient, .24, is zero. This suggests that the p-value of the sharp null is 0. Figure C.2 presents the distribution of coefficients from the simulations.

4.3 Falsification Exercise: Effect of Kimia II on Other Groups

One concern with the results from Sections 4.1 and 4.2 is that Kimia II could have led to a change in the dynamics of violence in the region for other reasons than the FDLR's expropriation trade-offs. Namely, the concern is that Kimia II might affect FDLR violence through channels other than a direct effect on the FDLR incentives for violence. Given potential concerns about this assumption, we use data on violence by all other armed actors to provide evidence that Kimia II does not affect the incentives of FDLR violence through its effect on other armed groups' violence in the region.

This data provides a falsification test for the baseline estimates. Kimia II should only predict attacks against villages of the FDLR state that are perpetrated by the FDLR. It should have no predictive power for violence perpetrated in the FDLR state by other armed actors. We can test this hypothesis by estimating the event study coefficients using attacks by other armed actors as dependent variables instead.

Panel A of Figure V presents the event study coefficients. The coefficients, both for years before and for years after 2009, are close to zero and not significant. Thus, Kimia II does not predict a rise in attacks in the FDLR state by armed actors who were not targeted by Kimia II. This falsification test suggests that Kimia II did not directly affect attacks by other armed actors in the FDLR state and thus that Kimia II could not have affected FDLR attacks by first affecting the overall dynamics of violence in the FDLR state.

As an additional falsification exercise, we examine whether Kimia II led to a rise in attacks by other armed actor on the villages they controlled—rather than on the FDLR state villages. If Kimia II only affected the incentives of the FDLR to use violence against its formerly controlled villages, it should have no predictive power for attacks by other armed actors in the villages previously controlled by other armed actors. Panel B of Figure V presents the event study coefficients. The results from this broader falsification exercise are consistent with the interpretation that Kimia II *directly* influenced the FDLR factions.[21]

[21] The Differences-in-Differences estimates for both falsification exercises yield identical conclusions.

5 Mechanisms

We now turn to an examination of potential channels for our finding that Kimia II led to an increase in attacks by the FDLR units against the FDLR state villages.

5.1 Disruption of FDLR Units' Expropriation Strategy

Available qualitative evidence suggests that the rise in attacks targeting specifically the FDLR state villages can be explained by violent theft operations. Sawyer and Van Woudenberg (2009) report citizens' accounts indicating that these attacks were clearly violent theft operations, and included forced labor to transport the stolen goods:

> "They came at night when we were in our houses. They made us get out of our homes, and then they looted all our goods . . . When they finished the operation, they made the youth transport all their looted goods to their camp in the forest"

The interviews we conducted with civilians and former armed group members in the FDLR state villages suggest that these attacks are prevalent, and gives a picture of the nature of the violence used for stealing (henceforth, pillages):

> "In the case of quick pillages such as those by the FDLR, it is just 30 minutes, and certain goods cannot be pillaged in that time (the heavy ones: cows, beans, heavy minerals), the members of the household. It is when the pillager is sure there is no threat that he can take all and use certain villagers for the transport."

Source: interviews with anonymous civilians in the Chiefdom of Basile (2013). To understand if the observed effects on FDLR attacks can be explained by pillaging, we use our detailed data on reported motive for the attacks. Our village-level data includes, specifically, for each attack, the purported motivation for the attack as concluded by the villagers in the discussions in private with households and with the history specialists. There are three types of attacks on the rural villages of North and South Kivu: pillages, conquest operations or punishment operations, which are respectively attacks with the purported intention to steal, to gain territory, or to sanction villagers. Table III shows how the attacks differ given the purported motivation. Attacks motivated by pillage tend to be shorter and take place at night, which allows to evade monitoring by the state forces. Indeed, two thirds of pillages take place at night, against 20-26% of other attacks, and fraction of pillages that take place under three hours is twice as large as any other attack. Pillages are associated to kidnapping of village men (typically for transporting stolen goods), and with the stealing of cows, goats, and pigs.[22] Overall, the average market value of stolen goods in a pillage is 5,464.82 USD, which compares to 2,764.99 and 3,258.20 for conquest and punishment, respectively.[23] This estimate implies that it takes 3.2 pillages to collect the same revenue as one year of taxation. However, a pillage purportedly reduces the ability to expropriate in the future.[24]

With this data, we now examine whether the rise in FDLR attacks could indeed be explained by pillaging. We undertake this analysis in Table IV, which presents the estimates from Equation 2. Column (1) includes, as dependent variable, an indicator for whether the FDLR conducts a pillage operation.[25] The magnitude of the coefficient is identical to our main coefficient. In Column (2) the dependent variable is again whether the FDLR conducts an attack against the village, but we include, as control, an indicator for whether the FDLR conducts a pillage. The coefficient is indistinguishable from zero. Thus, this suggests that the rise in attacks caused by Kimia II can only be explained by pillaging.

One concern with these results is that the information that the attack is motivated by pillaging is constructed from subjective reports by the villagers. Namely, if the report that the attack is motivated by pillaging is endogenous to Kimia II for other reasons than the

[22]The table excludes 93 attacks that have purportedly various motives.

[23]The p-value for the difference between the value stolen in a pillage and either other attack is .11. When we winsorize the 1% of the data, the p-value is .09.

[24]The average value of stolen goods in a pillage amounts to 20.39 USD per household. Yet, Table I showed that taxing a village yields 64.97 USD per year per household.

[25]We constructed this indicator as follows: the indicator takes value one if the villagers reported an FDLR attack with the purported intention to pillage, or in which villagers were abducted (which reflects pillaging motive as described in Section 2), or in which the FDLR confiscated wealth during the attack. Table E.1 in the online appendix shows that this effect is driven by pillaging operations.

true motive for the attack, the estimated coefficient may capture a change in the reports about the motive rather than a true change about the motive. Given potential concerns about this assumption, we complement this data with the household-year level panel dataset, which includes 8,979 observations for the whole sample in the years 2005-2012, and 6,487 observations for the quasi-experimental sample.[26] If the rise in FDLR attacks is driven by the motive to expropriate wealth, and given that pillages are relatively fast (and thus not all households can be pillaged), then those attacks should target individuals who are wealthier. Our qualitative evidence indicates that, after a few years settled in the village, the FDLR had knowledge of the wealth distribution in the village. Thus, if the attacks are motivated by expropriation, then they should target richer households.

We use the detailed information in our data to construct indicators about the respondent's father at the time of the respondent's birth: whether he was considered to be rich relative to other households in the village, the number of wives married to him and the number of plots of land he owned. We complement this data with detailed yearly information on the respondent's acquisition, sale, and loss of cattle. We use this information to reconstruct each household's asset ownership in the years prior to Kimia II for the households in our sample. Individuals who get married are typically given a plot of land and some assets, thus marriage amounts to a positive income shock for households previously headed by single men. We thus complement this information with the respondent's history of marriages. If the rise in FDLR attacks is explained by pillaging, then Kimia II should lead to a disproportionate increase in attacks by the FDLR that target the households of richer, married individuals.

In Columns (3)-(8), we use the household level panel dataset. The dependent variable is an indicator for whether the respondent's household is victim of an attack by the FDLR (irrespective of the purported motive), as reported in the household history module. Column (3) repeats the baseline specification as benchmark. The coefficient is significant at the 1% significance level. Its magnitude confirms the village level estimates, although the data

[26]For some covariates, the data was only collected in South Kivu and thus the sample is slightly smaller, but the results are unaffected by the loss of North Kivu due to our inclusion of province year fixed effects.

comes, in this case, exclusively from the household survey.[27] Columns (4)-(8) include, as control, **FDLR**$_i$ x $\mathbf{1}(t > 2009)$, multiplied with a variable, $\boldsymbol{HH_j}$, standing for: an indicator for whether the respondent's father comes from a rich family (4), the number of wives married to the respondent's father (5), the number of lands owned by the respondent's father (6), a proxy for the assets owned by the respondent's household in each year, standardized to mean zero and standard deviation of one (7), and an indicator for whether the household survey respondent is married in each year (8), respectively. Across all columns, the variables $\boldsymbol{HH_j}$ completely explain the rise in household level targeting of attacks following Kimia II. The coefficient on **FDLR**$_i$ x $\mathbf{1}(t > 2009)$ turns to zero and is almost never statistically significant. In contrast, the coefficient on **FDLR**$_i$ x $\mathbf{1}(t > 2009) \times \boldsymbol{HH_j}$ is always large and statistically significant.[28] Thus, this indicates that Kimia II led to a rise in attacks that target exclusively rich households, precisely those that would be attacked if the rise in attacks was motivated by expropriation. This suggests that Kimia II incentivized the FDLR to expropriate through pillaging.

Why could Kimia II have incentivized violent theft? One possibility is that Kimia II incentivized pillaging by decreasing the value of future expropriation in the village:

> "If an armed actor has to stay in a village, he needs the population for his survival. Those who prefer to pillage, it is because they know they cannot stay."

[27] Indeed, the village-level estimate shows that the likelihood of an FDLR state village experiencing at least one FDLR attack following Kimia II is 33% (baseline 0.09 + effect 0.24). The household-level regression shows the likelihood of a surveyed household in FDLR villages experiencing at least one FDLR attack due to Kimia II is 2% (baseline 0.00 + effect 0.02). A simple back-of-the-envelope calculation shows that this proportion corresponds to around 20 households (or 3% of the households) being directly victimized during each FDLR attack in FDLR villages due to Kimia II. The number 20 is the value of n that satisfies $(1 - (1 - 2\%)^n = 33\%)$. Based on the fraction of household respondents who report an attack on their household by the FDLR each year, we estimate that in each year in which our village data indicates an attack by the FDLR, 9.7 households are attacked, and in each year in which our village data indicates no attack by the FDLR, only .72 households are attacked.

[28] The distribution of wealth across households is not random, hence it is possible that this interaction also captures other household characteristics that could explain the targeting. To provide additional support to the interpretation that this reflects targeting of wealth for the purpose of expropriation, we also included, as control, **FDLR**$_i$ x $\mathbf{1}(t > 2009)$ interacted with other household characteristics. We found that the coefficient on the interaction remains unaffected by inclusion of these controls. This provides reassuring evidence that the interaction coefficient captures only targeting of richer households.

> "Armed groups who do not control the village for a long period do all they can to pillage the village before leaving. They know they are not secure, thus there is nothing to save."

> "It is normal. Anyone who takes a village, they develop their own strategies to maximize the revenues in that village. When we know that we are going to be displaced from a village, we make sure to steal as much as possible. This is why, the bandit is only your friend if he gets something out of it."

Source: interviews with anonymous ex-combattants and civilians in the Chiefdom of Basile (2013). With lower ability to regularly expropriate in the FDLR state villages, the FDLR units from the FDLR state may have found it optimal to pillage instead:

> "Each layer of the group now has to secure resources for its own survival . . . The FDLR–FOCA has since become increasingly reliant on revenue-generating activities such as looting and cattle raiding . . . In 2010, these activities only brought the group an estimated USD 5,000 per month in North Kivu and USD 4,000 in South Kivu—negligible amounts compared with the fortunes previously generated through the extensive unconventional logistics."

Source: Florquin and Debelle (2015). By reducing the frequency with which the FDLR can expect to expropriate, Kimia II could have altered an inter-temporal trade-off that, prior to Kimia II, disincentivized pillaging. Section F.1 presents a simple model of a bandit choosing the level of expropriation each period in a village. The model shows that, if expropriation decreases growth, the bandit refrains from pillaging as an investment in future expropriation when they expect to be able to expropriate sufficiently frequently in the future. If the future frequency of expropriation is sufficiently low, the bandit expropriates everything

We begin our examination of this trade-off by providing evidence verifying that Kimia II did shrink the frequency of expropriation. We combine taxation and pillaging events. Using the information of the frequency of collection of toll fees (daily), poll taxes (weekly, monthly, depending on the episode), mill taxes (daily), market taxes (weekly), and the number of pillages per year, we construct the frequency of expropriation episodes per year. Figure VI, Panel A, confirms that Kimia II is associated to a reduction in the frequency of FDLR expropriation events in the FDLR state from 350 to less than 50 events per year.

A key assumption for this mechanism is that maximal expropriation today, for instance through a pillage, reduces the ability to expropriate in the future. To examine whether this assumption holds, we estimate Borusyak et al. (2020) robust and efficient estimator to estimate the effect of a pillage on household assets. Figure VI, Panel B, shows that one pillage permanently reduces the assets of pillaged households, thus permanently reducing the armed actor ability to expropriate in the future. This provides support to our purported explanation for the expropriation channel that the FDLR increased pillaging because the value of future expropriation shrank as a result of Kimia II.

The simple model has an intuitive application to our context. Consider an armed actor deciding, in each period, whether to pillage, or instead refrain from pillaging. Our estimates from Table III imply that the investment in "no pillaging" has a break-even point of 3.77 months. If the armed group can expect to be able to expropriate every day in the next 3.77 months, which they can do collecting taxes, then not pillaging is a profitable investment. Conversely, if the armed group cannot guarantee daily expropriation over the next 3.77 months, a pillage becomes a more profitable expropriation strategy. Any disruption to the ability to expropriate daily for the next 3.77 months will turn pillaging optimal.

One concern with these results is that Kimia II may have increased pillaging because it reduced the contemporary income of the FDLR group as a whole, an income effect, rather than through its expected rate of expropriation. Confirming this conjecture, Panel A of Figure VII shows that the total income from pillaging and taxation by the FDLR decreased in the FDLR state. Including only the villages in our sample, total revenue by the FDLR as a whole in Basile decreased from 300,000 USD to 100,000 USD in one year.

We thus examine whether the results can be uniquely explained by either of these two channels. If the rise in attacks is driven by an income effect through a general decrease in the FDLR's income in the FDLR state, then FDLR attacks should increase in any village. In contrast, if it is driven by a reduction in the expected frequency of expropriation, it should only decrease in villages in which the frequency of expropriation decreases that year (or the years after). To examine this implication, we estimate Equation 1 using, as dependent variable, indicators for whether the FDLR attacks village i in year t and expropriates with high frequency, and for whether the FDLR attacks village i in year t, but expropriates with low frequency.[29] Panel B of Figure VII reports the event study coefficients. FDLR attacks drastically increase in villages in which the FDLR expropriates with low frequency in that year (left panel), but do not in villages that the FDLR expropriates with high frequency (right panel). This suggests that Kimia II led the FDLR to attack because it decrease its expected frequency of expropriation. It is inconsistent with the explanation that Kimia II led FDLR attacks to rise because it created a negative income effect on the FDLR group.[30]

5.2 FDLR Units' Retaliation Against Villagers in the FDLR State

The rise in FDLR attacks may reflect that the FDLR's frequency of expropriation was reduced by Kimia II, but it could also reflect that the FDLR chose to attack the villagers they formerly taxed as retaliation or punishment. For instance, they may strategically target specific villagers in those villages for collaborating with the Congolese state in helping dismantling the FDLR, or for unpaid debts. Indeed, armed groups often attack villagers as punishment for collaborating with the enemy, or for unpaid debts (Verweijen, 2013).

To analyze if the effects on FDLR violent operations can be explained by the intention to

[29] Since the 90% percentile of frequency of expropriation per year is 1, we separately examine FDLR attacks by whether the frequency of expropriation is higher or lower than 1. The results are unaffected.

[30] If Kimia II increased FDLR attacks because it disrupted their expected frequency of expropriation in those villages, then it must be that villages who are targeted first should see a faster increase in attacks by the FDLR. The Congolese army first targeted the villages that they could access first. Sine the army travels by foot, villages that are the most remote came under threat later. We estimate Equation 1 by whether the village is accessible by only foot, or by other means of transporation. Figure E.7 shows that, consistent with this interpretation, the effect of Kimia II on FDLR attacks in FDLR state villages is lagged in the villages that are only accessible by foot (Panel A), and is immediate in the villages that are accessible by foot or other means (Panel B).

punish villages and specific villagers, we first use our data on reported motive for the attacks. We undertake this analysis in Table V. Column (1) presents the Differences-in-Differences coefficient from Equation 2 but the dependent variable is an indicator for whether the FDLR conducts a violent operation with the motivation to punish villagers. The coefficient using attack motivated by punishment is statistically insignificant, and its magnitude is six times smaller than our baseline coefficient using an indicator for whether there is any FDLR attack as dependent variable. In Column (2) we estimate Equation 2, but we include, as control, an indicator for whether the FDLR attacks to punish villagers. The coefficient is almost identical to our baseline coefficient without this control, and significance is preserved.

One concern with these results is that the motive of punishment is recovered through interactions with the villagers. Namely, if the villagers are unable to guess the true motive for the attack, the estimated small coefficient may simply capture noise. Given potential concerns about this assumption, we complement this data with the household-year level data to examine whether the targeting of the attacks within village is consistent with retaliation. If the rise in attacks is driven by retaliation, individuals who should be at particular risk of retaliation for collaborating with the Congolese state include individuals who work as state officials, who are in the extended family of the village chief. Individuals who should be at particular risk of retaliation for unpaid debts include those who who disproportionately accumulated lands and cattle during the reign of the FDLR. Since the FDLR often indebts the villagers by forcing cattle upon them, and since they controlled the village and hence had power over the allocation of land, it is likely that accumulation of cattle or land during the FDLR period captures the formation of debts. If the rise in FDLR attacks is associated to retaliation, then Kimia II should predict whether the FDLR attacks disproportionately target individuals of those groups.

We undertake this household-level analysis in Columns (3)-(8). The dependent variable is an indicator for whether the household is victim of an attack by the FDLR, as reported in the household history. Column (3) is the baseline specification, and shows that Kimia II increases the fraction of households who are victimized by an FDLR attack in 2 pp. from a baseline of zero. Columns (4)-(8) include, as control the main explanatory variable, $\mathbf{FDLR}_{\mathrm{i}}$ x $\mathbf{1}(t > 2009)$, interacted with an indicator for whether the respondent of the household survey is a state official (4), has ever participated in an armed group (5), belongs to the extended family of the village chief (6), and proxies for the amount of land and of cattle accumulated by the respondent's household in the period of FDLR's reign, 2005-2008 (7,8), respectively. Across all columns, Kimia II has no predictive power on whether any of these groups of individuals are targeted. Furthermore, the size of the coefficient on $\mathbf{FDLR}_{\mathrm{i}}$ x $\mathbf{1}(t > 2009)$, is entirely unaffected by inclusion of these controls, and statistical significance is preserved. This suggests that the rise in FDLR attacks in the FDLR state does not reflect that the FDLR intended to punish the villagers in the villages it formerly controlled.

5.3 War Between FDLR Units and the Congolese army

A last possibility is that, even if expropriation is a driving factor for the rise in attacks we document, the attacks themselves could simply be reflecting war between the FDLR and the Congolese army. We now examine whether war can explain our reduced form coefficient. If the effect of Kimia II on FDLR's attacks reflects war with the Congolese army, Kimia II should predict whether the FDLR attempts to regain territory, whether the FDLR attacks the villages already retaken by the Congolese army, and whether the Congolese army engages in attacks in the FDLR state villages. Conversely, if the rise in attacks does not reflect war with the Congolese army, Kimia II should have no predictive power on any of those outcomes.

We undertake this analysis in Table VI. Column (1) estimates Equation 2 but uses, as dependent variable, an indicator variable for whether the FDLR conducted a conquest operation. The coefficient is nearly zero, negative, and statistically insignificant. Columns (2)-(5) estimate Equation 2 with different controls. Column (2) adds, as control, an indicator for whether the FDLR conducts a conquest operation in the village. The coefficient is identical to the main coefficient. Column (3) adds, as control, an indicator for whether the Congolese army controlled the village in the preceding year. The main coefficient is unchanged.[31] Columns (4) and (5) subset the sample to whether the Congolese army already controlled the village, or did not control the village, respectively. The coefficient in column (4) is zero and not significant, while that in column (5) is significantly larger and significant at the 1% level. This suggests that there is no effect of Kimia II on FDLR attacks in villages already successfully taken over by the Congolese army. In contrast, all attacks are concentrated in villages that the Congolese army has not taken over. This rules out that their attacks targeted the Congolese army due to war. Column (6) uses, as dependent variable, an indicator variable for whether the Congolese army themselves conducted an attack. Similarly, the coefficient is nearly zero, negative, and statistically insignificant. Thus, Kimia II has no predictive power on conflict between the FDLR and the Congolese army, suggesting that war is unlikely to explain the reduced form relationship between Kimia II and FDLR violence.

6 Implications for Household Welfare

To understand the overall impact of asserting the state's monopoly of violence on households' welfare, we now analyze its effect on household income through its effect on overall expropriation. Section F.2 presents an organizing framework to analyze household welfare.

We begin our examination of household welfare implications by documenting that Kimia II led households to experience brutalizing and traumatic episodes of violence. Table VII estimates Equation 2 where the dependent variables in Columns (1)-(4) are indicators for whether the village experiences each of the following (independently of perpetrator): sexual violence by armed actors (1), villagers' killing by armed actors (2), looting of households (3), and abduction of villagers (4). Kimia II doubles the fraction of villages who experience sexual violence and of household looting by armed actors. It also quintuples the fraction of villages in which armed actors abduct villagers, typically for forced labor including transporting of

[31] We use the lag of Congolese army control since in various cases, an FDLR attack in a village controlled by the Congolese army in that year precedes the control of the Congolese army. Since control by the Congolese army is permanent in our sample, this does not affect the interpretation.

assets stolen. The costs of these experiences are incalculable as they involve basic dignity.

Columns (5)-(11) analyze the material benefits of Kimia II. In column (5), the dependent variable is our estimated yearly per capita expropriation through pillaging. In Column (6)-(9), the dependent variables are our estimated amounts taxed by any armed actor in the village market, the village mill, implements a toll booth for entering and exiting the village, and collects a poll tax, respectively, in USD. Column (10) uses as dependent variable the total estimated tax payments from all taxation instruments. Finally, Column (11) uses as dependent variable the total amount expropriated through either pillaging or taxation by any armed actor. Columns (5) and (11) show that the total value expropriated through pillage for the average household increases in 1.42 USD for each household following Kimia II (more than doubling its baseline level), the total taxation payments per year per household decreases from 63.84 USD to 37.69 USD, a decrease in 41%.[32] Overall, the combination of both changes amounts to a reduction in the total per household payments from 64.31 USD to 39.53 USD, amounting to a 39% decrease. Using World Bank (2021) estimates of adjusted net national income per capita in DRC in 2009 in current USD, our estimate implies that the total expropriation, all of which are informal payments (including to the Congolese army) or outright theft, decreased from 25.17% to 15.48% of national yearly per capita income.[33]

While Kimia II decreased total value expropriated, its effect on household material welfare depends on whether it affected gross income. It is possible that, due to the oppressive tax regime on profitable activities, the FDLR reign is associated with a distortion in the supply of labor towards profitable income-generating activities, such as mining.[34] Obtaining retrospective estimates of income is impossible in this context. Thus, we analyze household variation over time for variables that proxy for gross income, and for household consumption.

[32]The decrease is tamed by the fact that the Congolese army also levies informal taxes, albeit at a lower rate and in lower incidence. Ignoring the taxes by the Congolese army, the total household tax payment on average in the period decreases by 46.80 USD, from 63.84 USD to 17.04 USD, a 70% decrease.

[33]Table E.2 replicates the analysis of pillage and taxation using as dependent variables indicators taking value one if pillage or each of the taxation instruments is recorded in a given village and a given year. The conclusions support the conclusions for this analysis and indicate that the decrease in tax payments arises mostly from a decrease on the extensive margin: the FDLR stops taxing.

[34]For a description of how armed groups' presence distorts economic activity, see (Nest et al., 2011).

Specifically, we analyze the household histories of occupational choice. We focused first on occupations for which income generation is known to be small or close to zero: unemployment, and subsistence farming. If Kimia II reduced insecurity in a way that it enabled the supply of labor, we should expect unemployment and subsistence farming to go down in response to Kimia II, as the households increase labor supply, and labor supply in the most profitable occupation, mining, to go up. Finally, since both schooling and getting married require significant payments in this setting (school fees and bride price, respectively), these are meaningful proxies for the household's spending.

Table VIII examines the effect of Kimia II on the occupation of the household survey respondent. Columns (1)-(2) use, as dependent variables, indicators for whether the main occupation of the respondent generates no income: unemployment and subsistence agriculture, respectively. Column (3) uses an indicator for whether the respondent worked in mining, the most lucrative activity in the region. Columns (4) and (5) use, as proxies for consumption, indicators for whether the respondent is in school and whether they are married, respectively. Kimia II decreased the fraction of individuals who were unemployed and the fraction of those who worked in subsistence farming (1 and 2). In contrast, it increased the fraction working in mining (3) and the fraction of individuals in school and those who are married (4 and 5), consistent with increases in income.[35]

In sum, Kimia II increased household net income, driven by a lower informal taxes and by higher gross income. But the price of those material gains is brutalizing violence.

7 The Cost of Re-Allocating State Force Over Space

Our analysis has focused on a local effect of state force. In the campaign we study, additional resources were created specifically for this operation through the support of United Nations. However, in reality, state force is scarce and typically must be reallocated from other areas.

[35] In Table E.3, we present a validation exercise for the household accounting. We reconstructed, for each household, the history of acquisitions and sales of cows, pigs, and goats. This allows us to compute, for each year, the household net savings, through the net increase in the stock of cattle.

Thus, the general equilibrium effect of military campaigns to remove armed groups is likely to include the effect of reducing state force elsewhere.

To understand the trade-offs typically involved in using force to establish a monopoly of violence, we examine the implications of re-allocating force away from the Chiefdom of Bakisi, in the district of Shabunda. We focus our analysis in the district from where some Congolese army forces were re-deployed a the time of Kimia II, and take advantage of the fact that the district was also home to the second most relevant military policy in eastern Congo in the period: the "Regimentation" process, which created a state vacuum. In 2011, all army units from the district of Shabunda were called into provincial capitals.

This history provides a test for the implication of removing state forces in the context of a weak state. Figure VIII, Panel A, shows that, while the fraction of villages controlled by the Congolese state was steadily increasing over time in most villages, it drastically dropped in 2011 in the district of Shabunda, where we plotted all of the villages in the sample, from 60% to 18% within two years. The panel shows that, following the vacuum, the fraction of villages that had a conquest attempt rises, and the fraction of villages controlled by non-state armed groups rises to 70% of the villages. Existing qualitative evidence suggests that the effect of this vacuum were so large that it led to the formation of a monopoly of violence by non-state armed groups (Jason Stearns et. al, 2013). Indeed, as the panel shows, this rise is entirely driven by the formation of a new armed group, the Raia Mutomboki, which ended up controlling 95% of Shabunda's territory (Okapi, 2013) and made it harder for the state to recover their monopoly of violence in Shabunda in subsequent years.[36]

To establish this result formally, Panel B presents the coefficient estimates from Equation 1 in which the dependent variable is an indicator for whether armed groups hold a monopoly of violence in the village, $\mathbf{FDLR}_{\mathrm{i}}$ is re-coded to indicate the location of the Bakisi Chiefdom (our only Shabunda Chiefdom in the sample). This event study specification measures the effect of the state vacuum created by the regimentation process on whether there is an

[36]Indeed, the Raia Mutomboki ended up holding territory against, and fighting, the Congolese army. See Jason Stearns et. al (2013).

armed group's monopoly of violence in the village.[37] The coefficients are positive after 2009, confirming that the state vacuum led to the burgeoning of non-state armed actors' monopolies of violence. This suggests that the benefits for a weak state to assert a monopoly of violence in one area have to be compared to the costs arising from its weakening of the state's monopoly of violence in other areas. New armed groups can freely form, gain territory, and weaken the ability of the state to regain the lost monopoly of violence in that area.

8 How to Build State Capacity? An Example

The timeline of Shabunda also allows to discuss an alternative policy to building states by force. Instead of using threats of force to remove armed groups, weak states often recognize their limited military capacity and integrate armed groups into the state's own monopoly of violence, increasing monitoring, giving them access to resources, and exploiting their higher local military skills. In contrast to the perils we have shown, this policy could expand the state's monopoly of violence without creating violent roving bandits.

In 2004, the Congolese state signed the Sun-City peace agreement, in which it agreed to include armed groups as part of the state forces. The agreement included clauses whereby militia groups, the Mai-Mai, as well as foreign armed group factions, such as the RCD, would remain militarily and politically active but would just change their "employer." This promise also came with the expectation of access to formal and informal rents, since accessing the national military or even politics comes along with typically superior access to rents (Verweijen, 2013). As such, this agreement can be a seen as an alternative to creating a monopoly of violence by force and instead as obtained through bargaining. While implementing this "integration" into the state forces was met with a number of challenges that are beyond the scope of this paper (Autesserre, 2006), Figure VIII, Panel A, provides an encouraging alternative to attempts to build state monopoly of violence by force. In

[37] While, in our main specification, we exclude the Chiefdom of Basile to avoid Kimia II from polluting the control group, the results are preserved even if we include Basile.

response to the peace agreement, armed groups integrated the state armed forces and were redeployed to break their structures of command. In contrast with Kimia II, we see no rise in roving banditry nor violence against civilians. While the feasibility of this type of policies is subject to political constraints and can have unanticipated dynamic effects, they are a promising counter-example to commonly supported strategies to build states by force.

9 Conclusion

A common feature of weak states is that they do not even hold a monopoly of violence. Instead, non-state armed actors regularly tax and provide protection, which disciplines their incentives to use violence arbitrarily. A natural question for weak states trying to build state capacity is thus: how should weak states build state capacity in those areas? This paper shows that military efforts to build state capacity by removing non-state armed actors succeed in relaxing the households' informal taxation, but can come at an incalculable cost to the of citizens. Our findings provide tentative evidence supporting that this effect can be explained by a reduction in the violence-disciplining ability by the armed actors to regularly expropriate citizens in the areas in which they have a monopoly of violence. These findings offer a new avenue for explaining the causes of violence, and suggest that the ability to regularly expropriate citizens in the future can be a source of their own protection. We discuss alternative strategies for weak states to assert their monopoly of violence.

Harris School of Public Policy, University of Chicago

Marakuja Kivu Research

Haas School of Business, University of California, Berkeley

Harris School of Public Policy, University of Chicago; National Bureau of Economic Research

Department of Economics, University of California, Berkeley

References

Acemoglu, Daron and Alexander Wolitzky, "The Economics of Labor Coercion," *Econometrica*, 03 2011, *79* (2), 555–600.

— **and James A. Robinson**, "Economics versus Politics: Pitfalls of Policy Advice," *Journal of Economic Perspectives*, 2013, *27* (2), 173–92.

ACLED, "Armed Conflict Location & Event Data Project," available at `https://acleddata.com` 2020.

Arjona, Ana, *Rebelocracy: Social Order in the Colombian Civil War*, Cambridge: Cambridge University Press, 2016.

— **and Zacharias Mamphilly**, *Rebel Governance in Civil War*, Cambridge: Cambridge University Press, 2015.

Autesserre, Séverine, "Local violence, National Peace? Post-War Settlement in the Eastern D.R. Congo," *African Studies Review*, 2006, *49* (3), 1–29.

Balan, Pablo, Augustin Bergeron, Gabriel Tourek, and Jonathan Weigel, "Local Elites as State Capacity: How City Chiefs Use Local Information to Increase Tax Compliance in the D.R. Congo. Revise and Resubmit, American Economic Review," 2021.

Balcells, Laia, "The Consequences of Victimization on Political Identities: Evidence from Spain," *Politics & Society*, 2012, *40* (3), 311–347.

Bates, Robert, Avner Greif, and Smita Singh, "Organizing Violence," *Journal of Conflict Resolution*, October 2002, *46* (5), 599–628.

Becker, Gary S., "Crime and Punishment: An Economic Approach," *Journal of Political Economy*, 1968, *76*, 169.

Bergeron, Augustin, Gabriel Tourek, and Jonathan Weigel, "The State Capacity Ceiling on Tax Rates: Evidence from Randomized Tax Abatements in the DRC," *Working Paper*, 2021.

Besley, Timothy and Torsten Persson, "Taxation and Development," STICERD - Economic Organisation and Public Policy Discussion Papers Series 41, Suntory and Toyota International Centres for Economics and Related Disciplines, LSE January 2013.

Blattman, Christopher and Edward Miguel, "Civil War," *Journal of Economic Literature*, March 2010, *48* (1), 3–57.

_ **, Gustavo Duncan, Benjamin Lessing, and Santiago Tobón**, "Gang rule: Understanding and Countering Criminal Governance," Working Paper 28458, National Bureau of Economic Research February 2021.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting Event Study Designs: Robust and Efficient Estimation," *Working paper*, 2020, *146*, 102518.

Bó, Ernesto Dal and Pedro Dal Bó, "Workers, Warriors, and Criminals: Social Conflict in General Equilibrium," *Journal of the European Economic Association*, 2011, *9* (4), 646–677.

Chwe, Michael Suk-Young, "Why Were Workers Whipped? Pain in a Principal-Agent Model," *Economic Journal*, December 1990, *100* (403), 1109–1121.

Dube, Oeindrila and Juan Vargas, "Commodity Price Shocks and Civil Conflict Evidence From Colombia," *Review of Economic Studies*, 2013.

Elton, *Tudor Revolution in Government*, Cambridge University Press, 1953.

Ferraz, Claudio and Frederico Finan, "Electoral Accountability and Corruption: Evidence from the Audits of Local Governments," *American Economic Review*, June 2011, *101* (4), 1274–1311.

— , **Joana Monteiro, and Bruno Ottoni**, "Regaining the Monopoly of Violence: Evidence from the Pacification of Rio de Janeiro's Favelas," *Working paper*, 2021.

Florquin, Nicolas and Raymond Debelle, "Waning Cohesion: The Rise and Fall of the FDLR–FOCA," in "Small Arms Survey 2015: Weapons and the World," Cambridge: Cambridge University Press, 2015, p. 199–215.

Gollier, Christian, "Time Horizon and the Discount Rate," *Journal of Economic Theory*, December 2002, *107* (2), 463–473.

ICC, "(International Criminal Court) Press Release, DRC situation: ICC issues an arrest warrant for Sylvestre Mudacumura," available at `https://www.icc-cpi.int/Pages/item.aspx?name=pr827` Jul 2012.

Jason Stearns et. al, "Raia Mutomboki: The flawed peace process in the DRC and the birth of an armed franchise," 2013.

Kalyvas, Stathis N., *A Logic of Indiscriminate Violence* Cambridge Studies in Comparative Politics, Cambridge University Press,

KST, "Kivu Security Tracker," available at `https://kivusecurity.org` 2021.

Levine, Daniel H., *The Morality of Peacekeeping.* Studies in Global Justice and Human Rights, Edinburgh University Press, 2014.

LSE-Oxford Commission on State Fragility, Growth and Development, "Escaping the fragility trap," 2017.

Nest, Michael W., François Grignon, and Emizet F. Kisangani, *The Democratic Republic of Congo: Economic Dimensions of War and Peace*, London: Lynne Rienner Publishers, 2011.

Newbury, Colin, "Patrons, Clients, and Empire: The Subordination of Indigenous Hierarchies in Asia and Africa," *Journal of World History*, 2000, *11* (2), 227–263.

Okapi, Radio, "Shabunda : la milice Raïa Mutomboki occupe 95% du territoire, selon son administrateur," 2013.

Olson, Mancur, "Dictatorship, Democracy, and Development," *American Political Science Review*, September 1993, *87* (3), 567–576.

Reno, William, *Warfare in Independent Africa* New Approaches to African History, Cambridge University Press, 2011.

RGC, "Donnees Administratives and Donnees Physiques," Data Source, Référentiel Géographique Commun 2010.

Sánchez de la Sierra, Raúl, "Essays on the Economics of Statelessness and State Formation," Doctoral Dissertation 2014.

_ , "On the Origins of the State: stationary bandits and taxation in eastern DRC," *Journal of Political Economy*, December 2020, *128* (1).

Sawyer, Ida and Anneke Van Woudenberg, "You Will Be Punished: Attacks on Civilians in Eastern Congo," Technical Report, Human Rights Watch 2009.

Shleifer, Andrei and Robert W Vishny, "Equilibrium short horizons of investors and firms," *The American Economic Review*, 1990, *80* (2), 148–153.

Stearns, Jason, *Dancing in the Glory of Monsters*, New York, NY: Public Affairs, 2011.

Tilly, Charles, "War Making and State Making as Organized Crime," in Theda Skocpol Peter Evans, Dietrich Rueschemeyer, ed., *Bringing the State Back In*, Cambridge University Press Cambridge 1985.

UNSC, "(United Nations Security Council) Final Report of the Group of Experts on the Democratic Republic of the Congo, S/2009/603 of 23 November," 2009.

Verweijen, Judith, "Military Business and the Business of the Military in the Kivus," *Review of African Political Economy*, March 2013, *40* (135), 67–82.

Vlassenroot, Koen, "Citizenship, Identity Formation Conflict in South Kivu: The Case of the Banyamulenge," *Review of African Political Economy*, 2002, *29* (93/94), 499–515.

Vogel, Christoph, "Maps of non-state armed actors in North and South Kivu," available at `https://suluhu.org/congo/mapping/` 2021.

Weigel, Jonathan L, "The Participation Dividend of Taxation: How Citizens in Congo Engage More with the State When it Tries to Tax Them," *The Quarterly Journal of Economics*, 07 2020, *135* (4), 1849–1903.

Wood, Elizabeth, "Variation in Sexual Violence During War," *Politics and Society*, September 2006, *34* (3), 1–21.

World Bank, "World Bank national accounts data, and OECD National Accounts data files," 2021.

Yamasaki, Junichi, "Time Horizon of Government and Public Goods Investment: Evidence from Japan," *Journal of Development Economics*, 2020, *146*, 102518.

Tables

Table I: Descriptive Statistics of Villages in the FDLR State and the Rest, Before Kimia II

	All	Mean outcomes				P-value
		FDLR State		Rest		
Observations		36		350		
Panel A: Village Characteristics						
Village Accessible on Foot, by Motorcycle, and by Car	0.50	0.00	(0.00)	0.56	(0.50)	0.00
Village Only Accessible by Motorcycle or on Foot	0.29	0.22	(0.42)	0.30	(0.46)	0.34
Village Only Accessible on Foot	0.21	0.78	(0.42)	0.14	(0.35)	0.00
Access to Phone Network	0.43	0.17	(0.38)	0.46	(0.50)	0.00
Distance to Rwanda (km)	91.96	75.59	(7.97)	93.64	(62.18)	0.08
Distance to River (km)	4.58	5.81	(1.88)	4.46	(4.24)	0.06
Distance to Road (km)	1.32	2.49	(3.61)	1.20	(2.60)	0.01
Distance to Airport (km)	20.21	13.28	(3.94)	20.92	(12.08)	0.00
Endowed with Coltan Mine	0.10	0.50	(0.51)	0.06	(0.24)	0.00
Endowed with Gold	0.25	0.44	(0.50)	0.23	(0.42)	0.01
Number of Immigrants into Village	29.65	2.00	(6.76)	32.97	(173.76)	0.45
Number of Emigrants from Village	33.94	10.24	(28.87)	36.71	(229.35)	0.64
% of Household Survey Respondents in Farming	0.46	0.39	(0.30)	0.48	(0.32)	0.30
% of Household Survey Respondents in Mining	0.17	0.13	(0.16)	0.18	(0.26)	0.50
% of Household Survey Respondents in Govt	0.07	0.11	(0.17)	0.06	(0.11)	0.12
% of Household Survey Respondents in School	0.08	0.12	(0.10)	0.07	(0.11)	0.14
% of Household Survey Respondents Unemployed	0.21	0.25	(0.17)	0.21	(0.20)	0.46
FDLR Expropriation Frequency (# Days per Year)	32.74	347.67	(73.79)	0.34	(3.93)	0.00
Panel B: FDLR State Functions						
Monopoly of Violence	0.10	1.00	(0.00)	0.01	(0.09)	0.00
Justice Administration	0.09	0.94	(0.23)	0.00	(0.05)	0.00
Fiscal Administration	0.08	0.83	(0.38)	0.00	(0.05)	0.00
Poll Tax	0.09	0.94	(0.23)	0.00	(0.00)	0.00
Toll Fees for Transit	0.08	0.83	(0.38)	0.01	(0.08)	0.00
Market Tax	0.03	0.28	(0.45)	0.00	(0.00)	0.00
Mill Tax	0.00	0.00	(0.00)	0.00	(0.00)	.
Total Value Taxed per Household, Yearly (USD)	33.61	64.97	(53.85)	30.39	(70.63)	0.00
Panel C: Security Outcomes						
Attack by any Actor	0.12	0.06	(0.23)	0.13	(0.34)	0.19
Attack with Expropriation by any Actor	0.12	0.06	(0.23)	0.13	(0.34)	0.20

Notes: This table shows the mean of the main village characteristics and outcome variables before Kimia II, in the years 2007 and 2008. Columns FDLR State and Rest show the means for the sample of villages of the FDLR State and for villages outside the FDLR State, respectively. All variables, unless otherwise noted, are binary indicators. Standard deviation of the variables are in parentheses. P-value reports the p-value of the t-test for whether the mean in column FDLR State and Rest is different. When the respondent has multiple occupations, we report the occupations that are their main occupation, according to the respondent. The P-value for the test of equality of means for the variable "Mill Tax" is missing, because there are no observations with a mill tax being collected in that period.

Table II: Differences-in-Differences Estimates

	Dependent Variable: *Attack by FDLR*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbf{FDLR_i}$ x $\mathbf{1}(t > 2009)$	0.24	0.18	0.24	0.40	0.21	0.23	0.46	0.50
	(0.07)	(0.07)	(0.07)	(0.08)	(0.10)	(0.07)	(0.16)	(0.14)
Village FE	Y	Y	Y	Y	Y	Y	Y	Y
Year-Province FE	Y	Y	Y	Y	Y	Y	Y	Y
Spillover Villages	N	Y	N	N	N	N	N	Y
District-Year FE	N	N	Y	N	N	N	Y	Y
Village Year Trends	N	N	N	Y	N	N	Y	Y
Time-Varying Controls	N	N	N	N	Y	N	Y	Y
Control for Migration	N	N	N	N	N	Y	Y	Y
Observations	1,544	1,912	1,528	1,544	1,376	1,246	1,104	1,422
R^2	0.29	0.25	0.31	0.30	0.29	0.32	0.35	0.32
Village Clusters	193	239	193	193	193	193	193	239
Chiefdom-Year Clusters	168	176	168	168	168	168	168	176
Mean Dep. Var.	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09

Notes: This table reports the coefficient estimates from Equation 2. The dependent variable is an indicator variable taking value 1 in village i and year t if village i is attacked by the FDLR in year t, and zero otherwise. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Spillover Villages*: includes the sample of villages where the FDLR is known to have been displaced (Chiefdom of Bakisi in Shabunda district). *District-Year FE*: are fixed effects for year and district. There are ten districts. The district corresponds to "Territoire" in the DRC's administration. *Village Year Trends:* include linear time trends for each village, separately. *Time-Varying Controls:* include controls for all characteristics that predict the location of the FDLR state by 2008, interacted with indicators for years. To implement this specification, we first estimate a probit model for whether a village belongs to the FDLR state on all observable characteristics. Then, for each village, we estimate the predicted probability that the village belongs to the FDLR state based on the vector of characteristics. We then estimate Equation 2, but include, as controls, the predicted probability interacted with year indicators. *Control for Migration:* includes controls for the number of immigrants and the number of emigrants yearly in each village. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

Table III: Mechanisms—Descriptive Statistics of Attacks, by Attack Type

	All	Mean outcomes			P-value
		Pillage	Conquest	Punishment	
Observations		373	162	177	
Attack at Night (Between 6pm and 4am)	584	0.67	0.20	0.26	0.00
		(0.47)	(0.40)	(0.44)	
Attack Duration is Under Three Hours	584	0.14	0.06	0.07	0.11
		(0.35)	(0.23)	(0.25)	
# Villagers Killed	576	4.22	4.41	4.57	0.61
		(10.24)	(9.07)	(9.57)	
# Kidnapped Men	394	4.54	1.93	3.51	0.33
		(6.46)	(5.64)	(16.91)	
# Women Raped	528	4.35	2.31	3.20	0.31
		(8.09)	(6.43)	(12.32)	
# Cows Looted	570	8.23	3.53	4.45	0.23
		(46.99)	(19.01)	(23.47)	
# Goats Looted	556	31.97	15.58	17.24	0.02
		(83.20)	(31.76)	(48.08)	
# Porks Looted	397	5.05	2.72	2.11	0.08
		(15.72)	(9.59)	(5.80)	
Market Value of Stolen Goods (USD)	584	5464.82	2764.99	3258.20	0.11
		(20535.14)	(7687.34)	(10779.96)	

Notes: This table shows the mean of attack characteristics for different types of attacks. The table excludes 93 attacks that have purportedly more than only one motive. *P-value* denotes the p-value for the t test for whether the mean characteristic in a pillage attack is identical to the mean characteristic of an attack that is either a conquest or a punishment.

Table IV: Mechanisms — Expropriation Strategy

	Dependent Variable							
	Village level		Household level					
	FDLR Pillage Attack	*FDLR Any Attack*	*FDLR Any Attack on Household*$_j$					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FDLR$_i$ x $\mathbf{1}(t > 2009)$	0.23	-0.00	0.02	0.00	0.01	0.00	0.01	0.00
	(0.09)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
FDLR$_i$ x $\mathbf{1}(t > 2009)$ x **HH**$_j$				0.05	0.02	0.02	0.02	0.02
				(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Village FE	Y	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Control for FDLR pillage	N	Y	Y	Y	Y	Y	Y	Y
HH$_j$ Characteristics				Rich	# Wives	# Lands	Assets	Married
Observations	1,544	1,544	6,487	6,455	6,439	6,415	3,459	6,487
R^2	0.29	1	0.17	0.17	0.17	0.17	0.19	0.17
Village Clusters	193	193	187	187	187	187	187	187
Chiefdom-Year Clusters	168	168	160	160	160	160	160	160
Mean Dep. Var.	0.07	0.07	0.00	0.00	0.00	0.00	0.00	0.00

Notes: This table reports the coefficient estimates from Equation 2. In columns (1), (2), and (3)-(8), the dependent variable is an indicator variable taking value 1 in village i and year t if: the village is attacked by the FDLR with the purported intention to expropriate the village (FDLR Pillage Attack), the village is attacked by the FDLR, household j in village i is attacked by the FDLR, respectively, and zero otherwise. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Control for FDLR punishment*: includes as control an indicator taking value 1 if village i is attacked by the FDLR in year t with the motive to punish the village. *HH$_j$ Characteristics*: reports the indicator variable with which the main explanatory variable **FDLR**$_i$ x $\mathbf{1}(t > 2009)$ is multiplied in the respective column. The variable Rich takes value 1 if the respondent's father came from a family considered rich at the time of the respondent's birth. The variables # Wives, # Lands are the number of wives married to the respondent's father and the number of land plots owned by the respondent's father at the time of the respondent's birth. Assets is a continuous variable, standardized to mean zero and standard deviation of 1, measuring the number of animals owned by the respondent's household in year $t-1$. Married is an indicator variable taking value 1 if the respondent is married in year t and zero otherwise. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II. The R-square in column (2) is close to 1, illustrating that most FDLR attacks in the FDLR state following Kimia II are pillages.

Table V: Mechanisms — Retaliation

	Dependent Variable							
	Village level		Household level					
	FDLR Punishment Attack	*FDLR Any Attack*	*FDLR Any Attack on Household*$_j$					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbf{FDLR}_i$ x $\mathbf{1}(t > 2009)$	0.04	0.20	0.02	0.02	0.02	0.02	0.02	0.02
	(0.03)	(0.08)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\mathbf{FDLR}_i$ x $\mathbf{1}(t > 2009)$ x $\mathbf{HH}_j$				0.01	0.05	0.05	0.20	-0.00
				(0.03)	(0.04)	(0.05)	(0.22)	(0.08)
Village FE	Y	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Control for FDLR punishment	N	Y	N	N	N	N	N	N
$\mathrm{HH_j}$ Characteristics				Official	Fighter	Chief	Land	Cattle
Observations	1,544	1,544	6,487	3,630	6,487	6,455	6,066	3,841
R^2	0.15	0.37	0.17	0.21	0.17	0.17	0.16	0.16
Village Clusters	193	193	187	187	187	187	187	187
Chiefdom-Year Clusters	168	168	160	160	160	160	160	160
Mean Dep. Var.	0.02	0.07	0.00	0.00	0.00	0.00	0.00	0.00

Notes: This table reports the coefficient estimates from Equation 2. The dependent variable is an indicator variable taking value 1 for village i and year t if: (1) the village is attacked by the FDLR *with the purported intention to punish the villagers* (FDLR Punishment Attack), (2) the village is attacked by the FDLR, and (3)-(8) household j in village i is directly attacked by the FDLR in year t, and zero otherwise. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Control for FDLR punishment*: includes as control an indicator taking value 1 if village i is attacked by the FDLR in year t with the motive to punish the village. *HH_j Characteristics*: reports the indicator variable with which the main explanatory variable $\mathbf{FDLR}_i$ x $\mathbf{1}(t > 2009)$ is multiplied in the respective column. Official, Fighter, Chief are indicator variables taking value 1 if: the respondent is a state official any of the years preceding Kimia II, the respondent has ever participated in any armed group up until year $t-1$, the respondent's father belonged to the family of the village chief, respectively, and zero otherwise. Land, Cattle are continuous variables, standardized to mean zero and standard deviation of 1, measuring the number of animals, and the number of plots of land, that the respondent's household obtained during the years of the FDLR's reign, respectively. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

Table VI: Mechanisms — War

	Dependent Variable					
	FDLR Conquest Attack	*FDLR Any Attack*				*Congolese Army Attack*
	(1)	(2)	(3)	(4)	(5)	(6)
FDLR$_{\mathrm{i}}$ x $\mathbf{1}(t > 2009)$	-0.00	0.24	0.27	-0.00	0.33	-0.02
	(0.02)	(0.07)	(0.07)	(0.00)	(0.12)	(0.02)
Village FE	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y
Control for FDLR Conquest Operation	N	Y	N	N	N	N
Control for Lagged Congolese Army Presence	N	N	Y	N	N	N
Lagged Congolese Army Presence=1 Only	N	N	N	Y	N	N
Lagged Congolese Army Presence=0 Only	N	N	N	N	Y	N
Observations	1,544	1,544	1,351	296	934	1,544
R^2	0.13	0.33	0.34	0.16	0.42	0.13
Village Clusters	193	193	193	193	193	193
Chiefdom-Year Clusters	168	168	168	168	168	168
Mean Dep. Var.	0.02	0.07	0.07	0.07	0.07	0

Notes: This table reports the coefficient estimates from Equation 2. The dependent variable is an indicator variable taking value 1 for village i and year t if: (1) the village is attacked by the FDLR *with the purported intention to gain territorial control of the village* (FDLR Conquest Attack), (2)-(5) the village is attacked by the FDLR, and (6) the village is attacked by the Congolese army, and zero otherwise. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Control for FDLR Conquest Operation*: includes as control an indicator taking value 1 if village i receives a conquest operation by the FDLR in year t. *Control for Lagged Congolese Army Presence*: includes as control an indicator taking value 1 if village i was controlled by the Congolese army in year $t-1$. *Lagged Congolese army presence = 1 only:* includes only the sample of i, t observations for which the Congolese army controls village i in year $t-1$. *Lagged Congolese army presence = 1 only:* includes only the sample of i, t observations for which the Congolese army does not control village i in year $t-1$. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

Table VII: Implications for Household Welfare: Disutility of Violence and Household Informal Payments

	Dep. Var.: *Disutility of Violence*				Dep. Var.: *Household Transfers to Armed Actors (USD)*						
	Village experiences:				*Pillage*	*Taxation*					*Total*
VARIABLES	*Rape*	*Death*	*Looting*	*Kidnap*	*Theft*	*Market*	*Mill*	*Toll*	*Poll*	*Total*	*Total*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
FDLR$_i$ x $\mathbf{1}(t > 2009)$	0.10	0.05	0.18	0.24	1.42	-1.94	0.39	-8.21	-16.40	-26.15	-24.78
	(0.04)	(0.05)	(0.06)	(0.06)	(0.95)	(1.51)	(0.67)	(2.70)	(7.61)	(9.68)	(9.34)
Village FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,416	1,416	1,416	1,416	1,465	1,480	1,480	1,480	1,480	1,480	1,480
R^2	0.17	0.16	0.17	0.19	0.20	0.63	0.48	0.76	0.38	0.40	0.40
Village Clusters	178	178	178	178	185.0	185	185	185	185	185	185
Chiefdom-Year Clusters	160	160	160	160	168	168	168	168	168	168	168
Mean Dep. Var.	0.04	0.06	0.08	0.06	0.5	3.93	0	17.36	42.55	63.84	64.31

Notes: This table reports the coefficient estimates from Equation 2. The dependent variables in columns (1)-(4) are indicator variable listed in the headers. The dependent variables in columns (5)-(11) are continuous variables in USD listed in the headers. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

Table VIII: Implications for Household Welfare: Gross Income

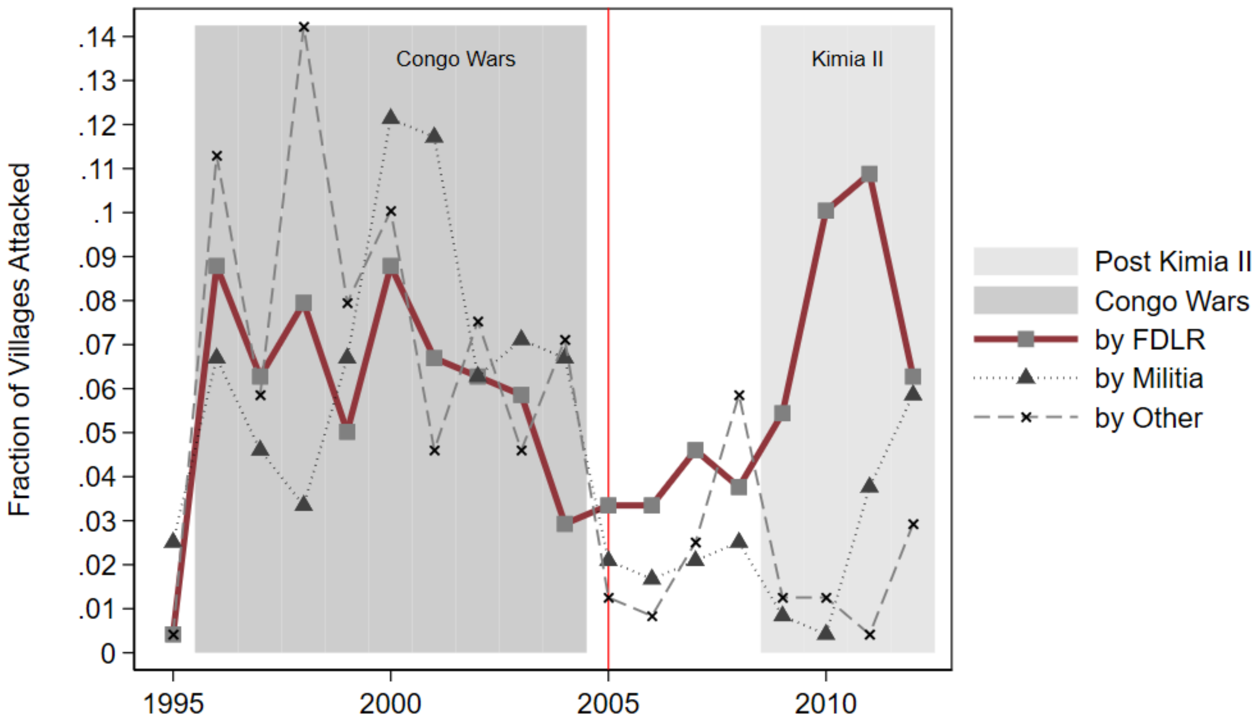
	Dep. Var.				
	No Gross Income		Gross Income	Consumption	
	Unempl.	*Farming*	*Mining*	*In school*	*Married*
	(1)	(2)	(3)	(4)	(5)
$\mathbf{FDLR}_i$ x $\mathbf{1}(t > 2009)$	-0.07	-0.05	0.06	0.04	0.04
	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)
Village FE	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y
Observations	3,662	3,662	3,662	3,628	3,628
R^2	0.13	0.31	0.35	0.49	0.49
Mean Dep. Var.	0.29	0.40	0.15	0.76	0.77

Notes: This table reports the coefficient estimates from Equation 2. The dependent variables in columns (1)-(5) are indicator variable listed in the headers: respondent's main occupation as dependent variables (columns 1-3) and whether the respondent is in school or is married in each year (columns 4-5). Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

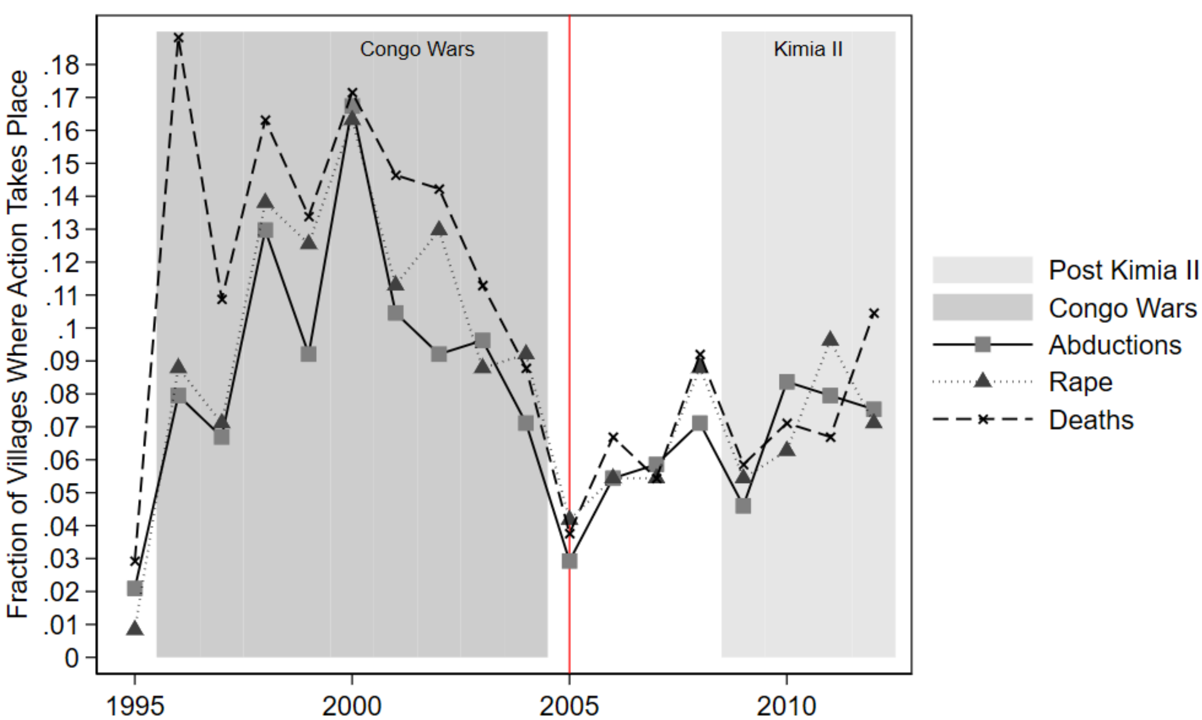
Figures

Figure I: Violence in the Region, in Historical Context

A. Attacks, by Perpetrator Type

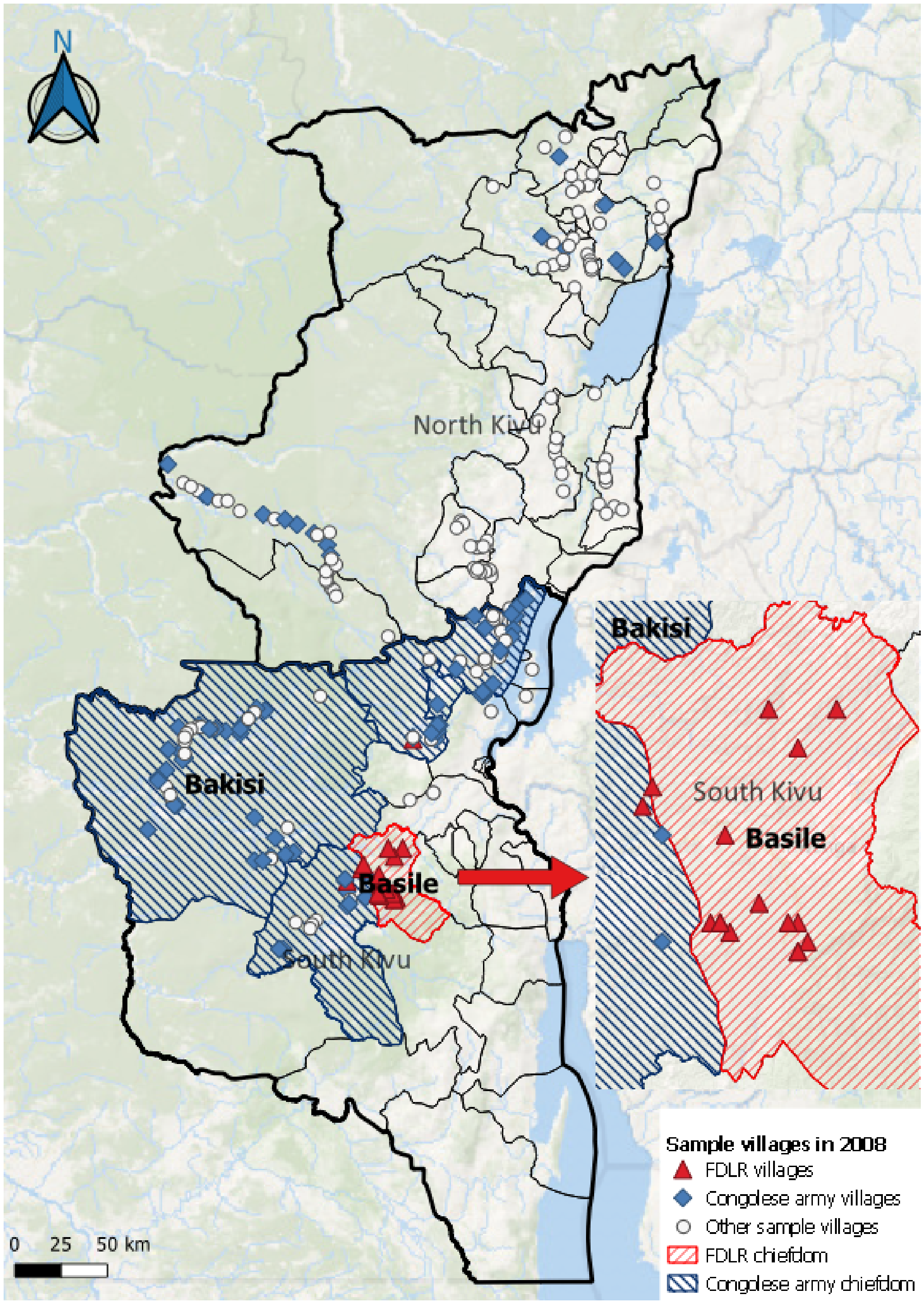


B. Types of Violence



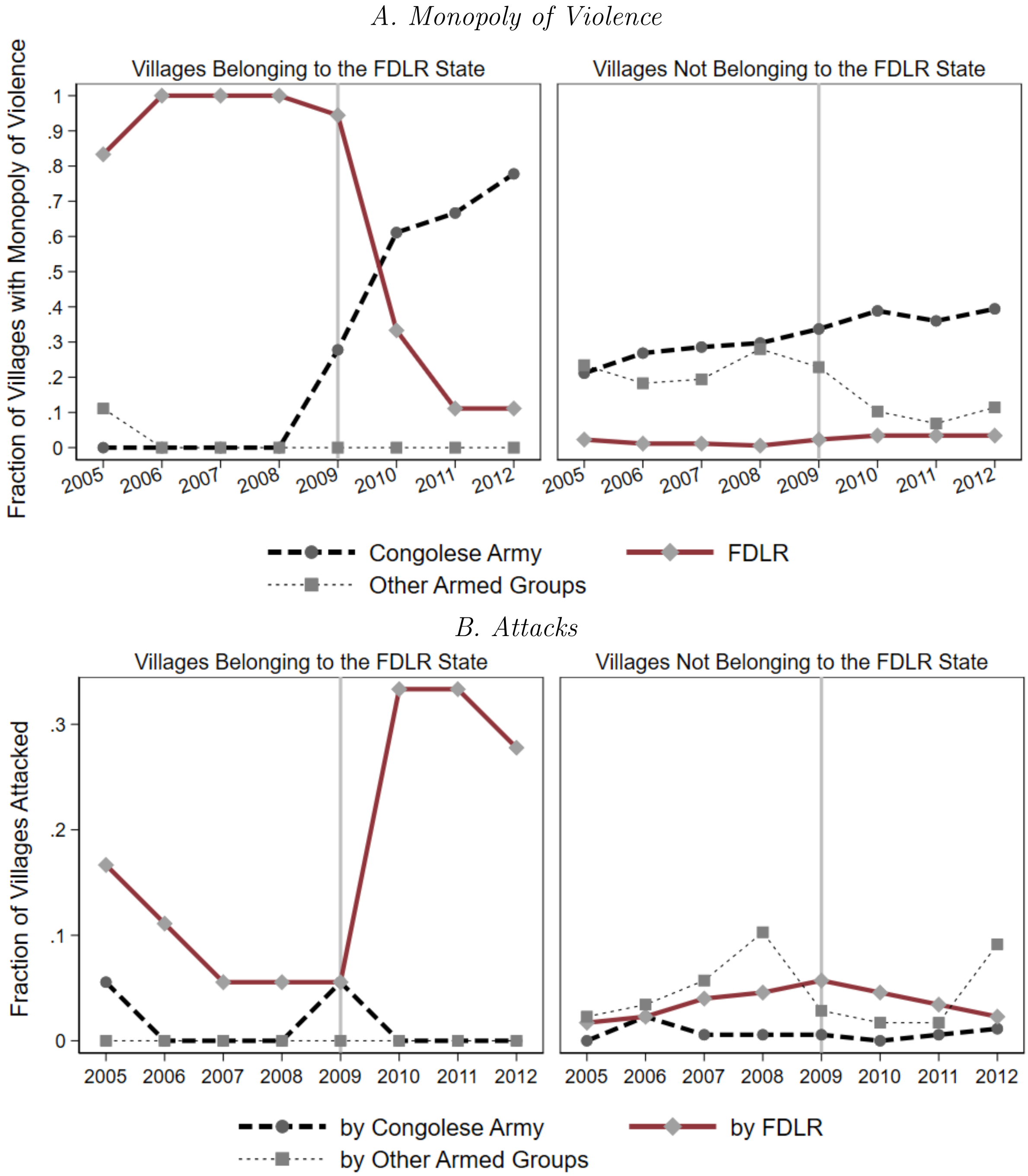
Notes: Panel A presents the fraction of villages in the sample attacked by the FDLR, Rwanda-backed armed groups, or Congolese militia, respectively. Panel B presents the fraction of villages in the sample where the following actions take place, in a given year, during an attack on the village by an armed actor: abduction of villagers, deaths of villagers, sexual violence against villagers. Red, vertical lines indicate the start of the study period for analyzing the effect of Kimia II.

Figure II: Sample Villages in 2008: the FDLR State and the Rest



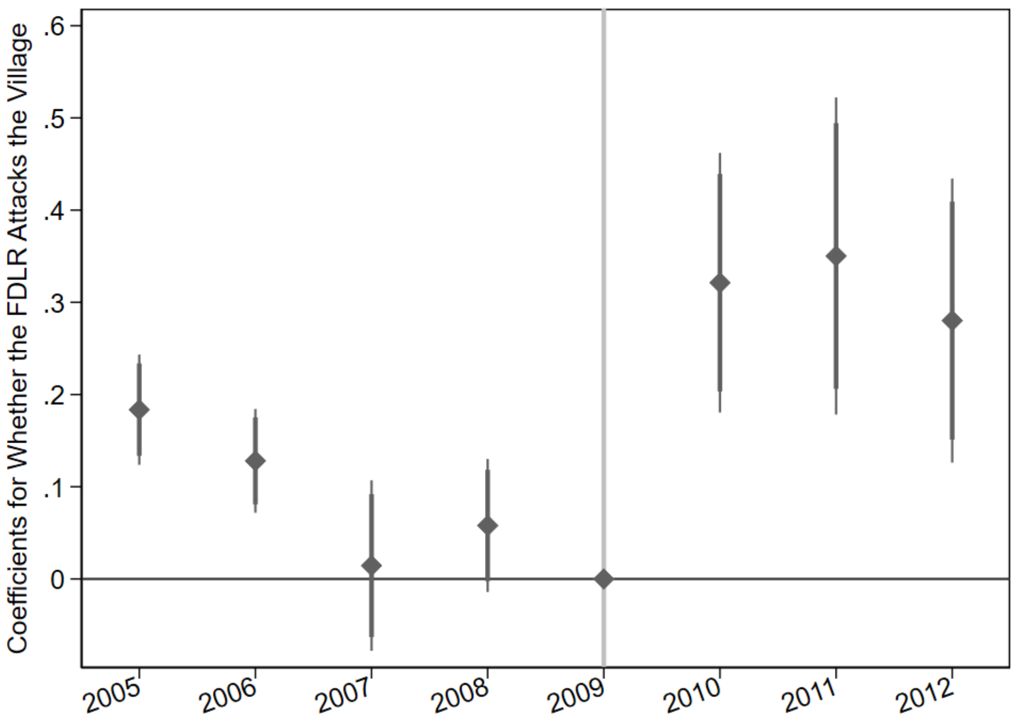
Notes: This figure shows the map of the survey villages in our sample, covering the provinces of North Kivu and South Kivu. Red triangles are the villages where FDLR had control in 2008, blue squares are those where the Congolese army had control in 2008. Red striped area indicate the Chiefdoms of the FDLR state. Blue striped areas indicate Chiefdoms in which more than 50% of the villages were controlled by the Congolese army in 2008.

Figure III: Trends of FDLR Monopoly of Violence and FDLR Attacks



Notes: Panel A presents the trends of who holds the monopoly of violence (Congolese army, FDLR, other armed actors). Panel B presents the trends of who attacks (FDLR, other armed actors).

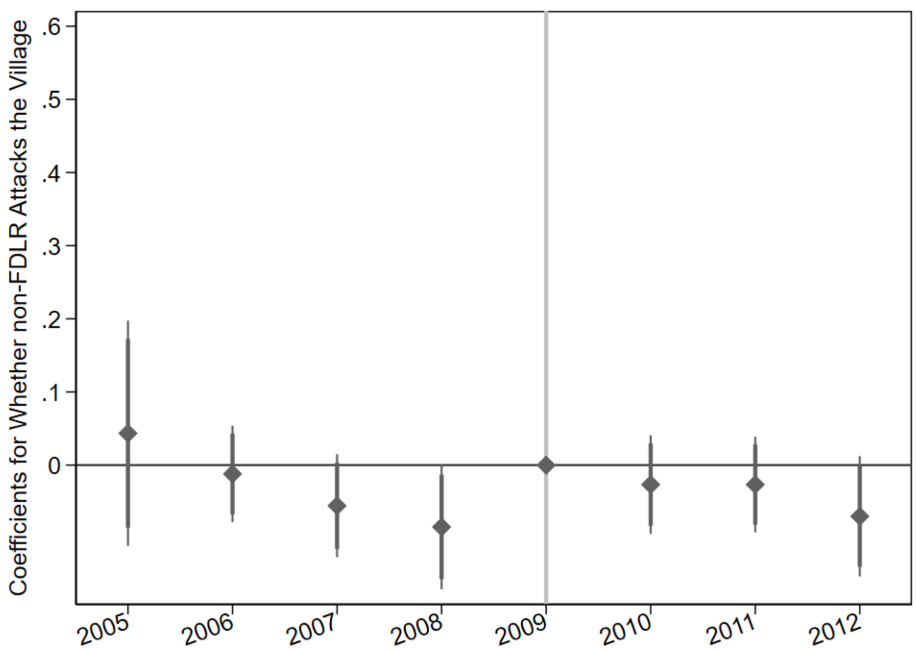
Figure IV: Event Study Estimates



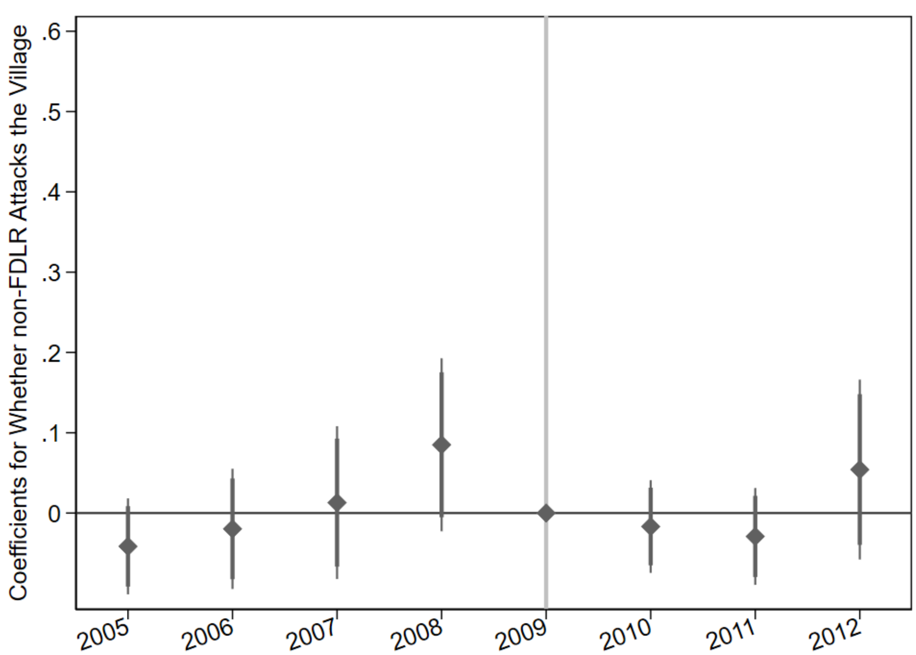
Notes: This figure shows the coefficients $\beta_k,\ k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1. The dependent variable is an indicator for whether the FDLR attacks village i in year t. The year 2009 is the omitted category. 1,544 village-year observations were used in the estimation. The R-squared is 29.7%. There are 193 and 168 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.

Figure V: Falsification Test: Attacks by Other Armed Actors

A. In FDLR State



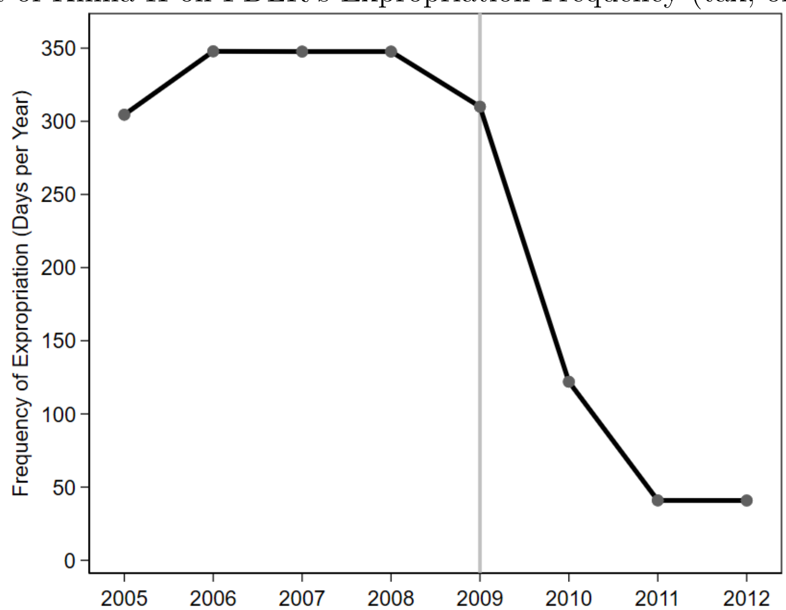
B. In Villages Controlled by Other Actors



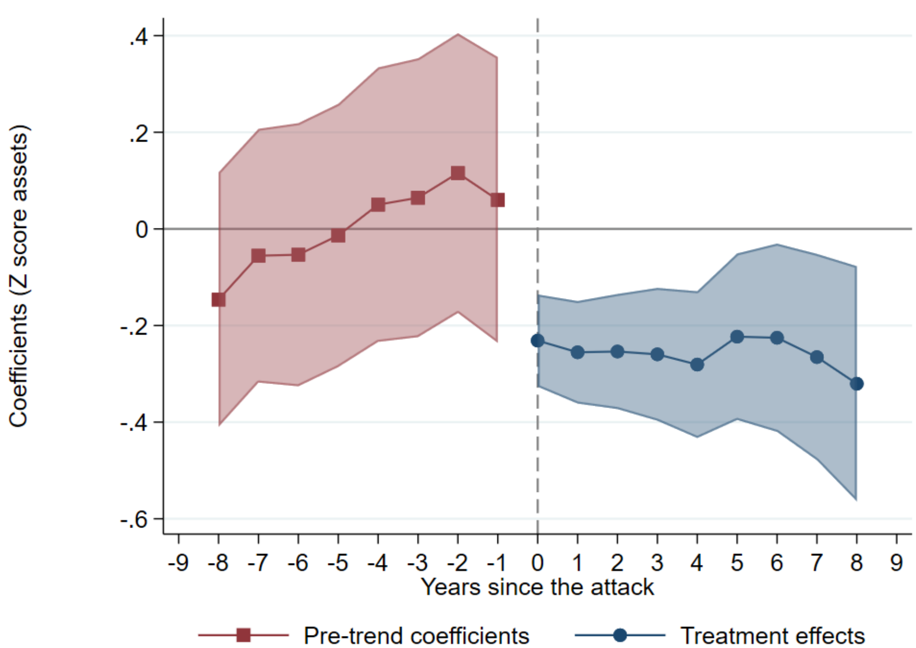
Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1. The year 2009 is the omitted category. 1,544 village-year observations were used in the estimation. In Panel A, the dependent variable is an indicator for whether any armed actor other than the FDLR attacks village i in year t. The R-squared is 16.2%. In Panel B, the dependent variable is an indicator for whether any armed actor other than the FDLR attacks village i in year t, and the variable $\boldsymbol{FDLR_i}$ here takes value 1 if any non-FDLR actor controlled village i in the year 2008, and zero otherwise. The R-squared is 16.9%. In both panels, there are 193 and 168 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.

Figure VI: Mechanism: Expropriation, Value of Future Expropriations—Validation

A. Effect of Kimia II on FDLR's Expropriation Frequency (tax, or pillage)



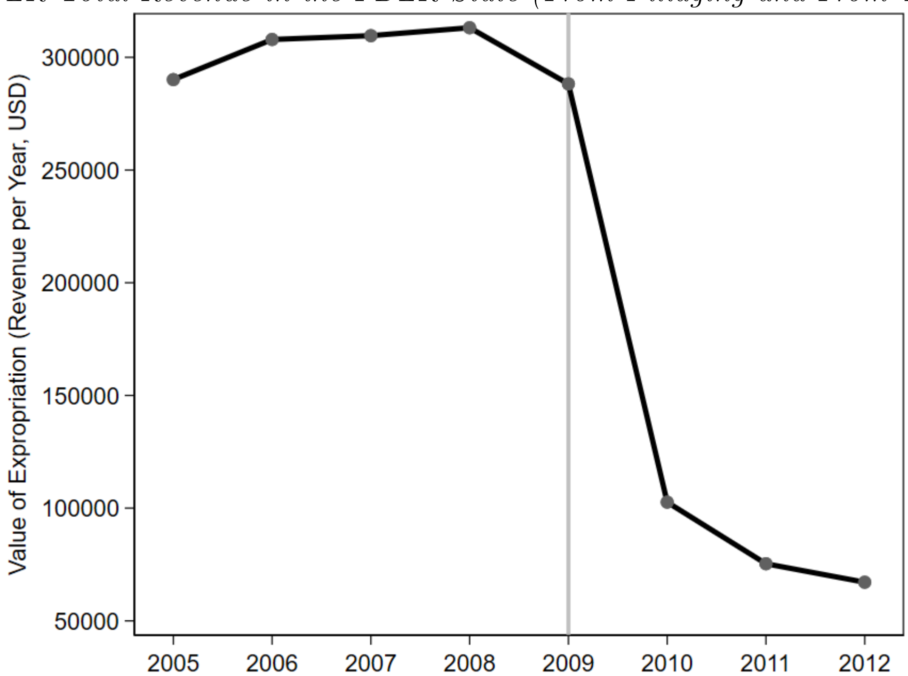
B. Effect of One Pillage on Household Assets



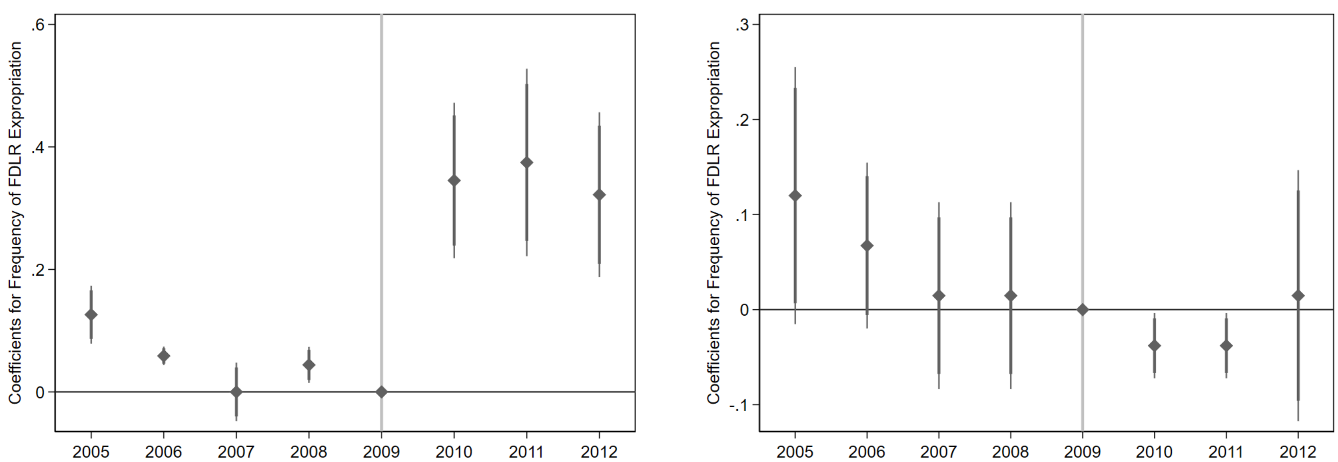
Notes: Panel A shows the trend of frequency of FDLR expropriation in the FDLR state. The frequency of expropriation is the sum of pillaging and tax payment events per year. Panel B shows the coefficients from Borusyak et al. (2020)'s efficient and robust estimator for the effect of a pillage on the household's stock of cattle, standardized to mean zero and standard deviation of one. Figure E.6 shows these coefficients separately for when the dependent variable is the number of goats, porks, cows, respectively.

Figure VII: Mechanism: Expropriation: Group-Level Income Effect

A. FDLR Total Revenue in the FDLR State (From Pillaging and From Taxes)



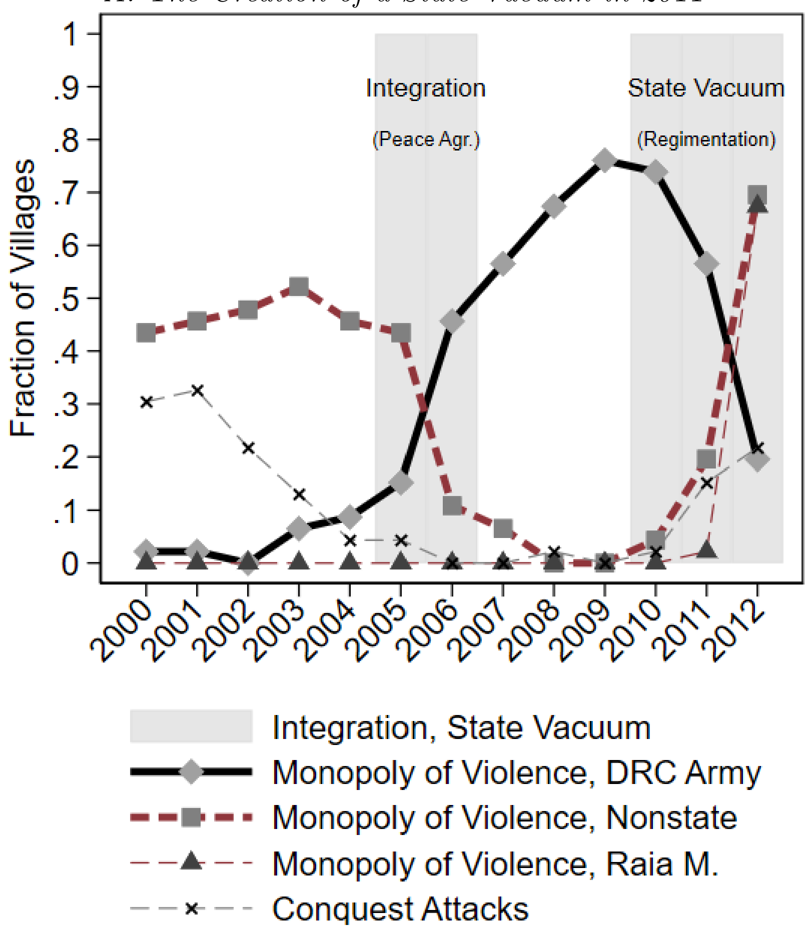
B. Event Study: FDLR Attacks in Low vs. High Frequency of Expropriation



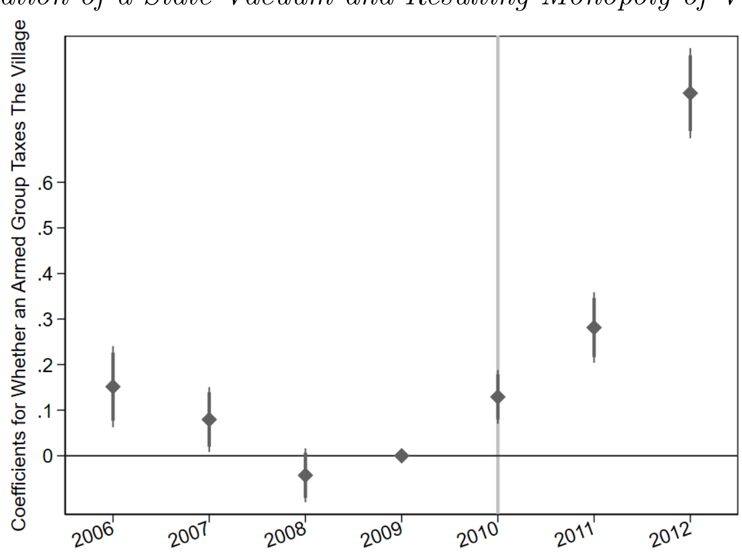
Notes: Panel A shows the total income in USD generated by the FDLR in the FDLR state, through taxation and through pillage confounded. Panel B shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1. In the left panel, the dependent variable is an indicator for whether the FDLR attacks village i in year t and the FDLR expropriates the village with low frequency. The R-squared is 32.8%. In the right panel, the dependent variable is an indicator or whether the FDLR attacks village i in year t and the FDLR expropriates the village with high frequency. The R-squared is 18.7%. In both panels, there are 193 and 168 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.The year 2009 is the omitted category. 1,544 village-year observations were used in the estimation.

Figure VIII: Building States by Force, in Context

A. The Creation of a State Vacuum in 2011



B. Event Study: The Creation of a State Vacuum and Resulting Monopoly of Violence by Armed Groups



Notes: Panel A presents the fraction of villages that are under a monopoly of violence by the DRC State, nonstate armed groups, or the Raia Mutomboke militia, respectively, as well as the fraction of villages where a conquest attempt takes place, by nonstate armed actors. The sample in the left panel includes only the villages in the area vacated by the Congolese army. The sample in the right panel includes all the remaining villages — including the villages in the FDLR state. Panel B presents the event study estimates for the dependent variables: (left) whether an armed group taxes the village and (right) whether an armed group attacks the village.

Online Appendix

A Additional Details on the Origins of the FDLR

The armed group known as the Front de Liberation du Rwanda (FDLR) is an ethnic Hutu group; in 2009, it was composed of approximately 6,000 combatants.

In July 1994, a rebel movement took power in Rwanda, ending the genocide that had been perpetrated by government supported militias, the Interahamwe, and the government forces. In response, two million Rwandans, mostly Hutus, fled into eastern DRC, specifically North Kivu. Among them were the Interahamwe, but also former Rwandan state bureaucrats and armed forces. They formed the Armée de Libération du Rwanda (AliR), the predecessor of the FDLR. They opposed the government in Kigali and used North and South Kivu as a base for rebel activity against Rwanda.

In 1996, the Rwandan government launched a military campaign that started the First Congo War (1996–1997). One of the goals was to eliminate the insurgent threat coming from the Kivus. While the Rwandan coalition succeeded in defeating Congolese government forces, installing a new president, and occupying large parts of the country, they failed to completely defeat Rwandan rebel activity in eastern DRC.

Conflicts between the new Congolese government and its Rwandan and Ugandan backers in 1998 plunged the DRC into the Second Congo War (1998–2004). During this war, Rwanda backed a rebel group, the Rassemblement Congolais pour la Democratie (RCD), that quickly controlled the eastern half of the country, where it overtook the apparatus of the state and all urban areas. In the countryside, resistance militia had formed, which the RCD fiercely fought through counterinsurgency campaigns. The Congolese state had no formal control over the east (Verweijen and Vlassenroot, 2015, Clark, 2002, Ngonzola-Ntalaja, 2002).

Instead, the Congolese government supported various armed groups and provided them with funds and ammunition to fight the RCD. Among them were the former Rwandan government forces and militia members, AliR, who in 2000 formed the FDLR. The FDLR is, in most areas of DRC, a foreign-armed group. By 2004, all major armed groups, except the FDLR, vacated the east in exchange for benefits precluded in a peace agreement.

B Variable Description

B.1 Existing Violence Datasets

Publicly available conflict data, the most comprehensive of which is ACLED (2020), are poorly suited for our purposes. There are a number of aspects which discourage the use of (ACLED, 2020) data as the main analysis in this paper – although we replicate all results using (ACLED, 2020) data in the appendix.

First, the data from Sánchez de la Sierra (2020) is significantly richer than the data in ACLED (2020). For a start, it contains detailed information on armed actors taxation, the presence of a stationary bandit, and whether armed actors run fiscal and judicial administrations. Furthermore, regarding the violence data, the data in Sánchez de la Sierra (2020) also contains richer information, including systematic records of whether villagers were forcefully abducted (typically to transport foods), whether sexual violence occurred, number of deaths, whether assets were stolen and their value.

Second, ACLED data has limited spatial coverage. ACLED primarily relies on publicly available news coverage to populate its data set. When a local or international news outlet reports a violent event, it is then added to the ACLED data. However, in our context, the FDLR operates in extremely remote areas of the Eastern DRC which receive little to no media attention. Many pillage attacks in remote villages are thus not included in the ACLED data. Our data on the other hand relies on villagers' own account of attacks on their village and are thus much more likely to include all past attacks. As a result, from 2001 to 2013 ACLED data includes only three violent events involving the FDLR in Basile while our data includes 33 such events just in the villages surveyed by us in Basile.

Third, ACLED data has low geographical precision. Since ACLED data is based on news sources, the geographic information about violent events is only as precise as the news coverage. Many reports about violent events in the DRC do not mention the precise location of an attack and even if they include a village name, the lack of publicly available geographic data prevents ACLED from accurately geo-locating the event. Instead administrative levels are used. In our case, between 2001 and 2013, 31 out of 38 ACLED violent events for the Chiefdom of Basile (the FDLR state) are geo-tagged as having happened in one location, the capital of the Chiefdom. This is obviously impossible and suggests the data is severely geographicaly mis-coded. Our enumerators, instead physically visited all villages in the sample and we thus have the precise location of each attack.

Finally, ACLED relies on the news coverage to fill in the necessary information. If a report did not mention certain details, then they cannot be included in the data. Our survey was designed with a detailed module on each attack and on armed group governance. We thus have consistent evidence on violent events in each village.

B.2 Estimation of stock of cattle

Each respondent in South Kivu is asked to list yearly purchase and sales for farm animals (cows, goats, and pigs). For asset stock at birth, we ask how many cows, goats, pigs, and fields the respondent's father had when the respondent was born. We also ask each respondent to report their asset stock at the survey year in farm animals but not fields.

We adopt the following approach to construct asset stock in cows, goats, and pigs. If the respondent is not married, for farm animals and lands, we start from respondent's current asset stock and calculate respondent's asset stock in previous year by subtracting respondent's net purchase of asset this year from current asset stock. We calculate respondent's asset stock in each year backward up to year 1995. If the respondent is married, we calculate respondent's asset stock backward up to the year when respondent was first married (89.9% of respondents who have hold marriages are only married once). Before the year respondent was first married, we start from respondent's asset stock at birth and calculate asset stock in following years by adding net purchase of asset up to the year before respondent was first married. The reason for this approach is that respondent is separated from his original household and starts a new household when he is married. For fields, since we did not ask each respondent to report their fields at the survey year, we calculate respondent's stock of fields starting from birth and adding net purchase of fields in the years that follow.

We then include information on attacks motivated by expropriation (pillages) to estimate the average assets lost to theft. To compute the total value pillaged, we use information from the village survey in which respondents, during one week of work, do the inventory of what was stolen in the village in each attack. We then use information on yearly prices for each of those objects to compute the total value of goods stolen. To compute the total value taxed, we used information in the survey reporting the level of each tax, for each year in which it was collected. For market tax, mill tax, and toll fee, we simply assume that each household pays each of those taxes weekly, based on our qualitative work. For poll tax, we use the exact amount of the poll tax which we gathered using information on the level of the poll tax, the frequency of its payment, and the unit on which it was imposed, for each year in which the poll tax was collected. We omit the effect on mining taxes since its computation requires information on output and is thus not reliable. Since mining taxes occurrence decreases, the effects on total taxation we estimate are therefore a lower bound of the effect of Kimia II on total value taxed.

B.3 Strategies to tackle measurement error

First, in each province and for each year, we identified time cues that respondents would remember from their experience of the regional history. When asking a question about a historical event, such as an attack or the acquisition of cattle, to identify the year in which the event took place, the field researchers first examined whether the event was before or after the time cues in that area. Respondents sometimes did not know the exact year, but they recalled with certainty whether it was after or before a given time cue. Since we know the year of the time cues, this allowed the field researchers to pin down the exact years.

Second, built-in in the surveys was a strategy for auto-generation of person-specific relevant time-cues, anchored to common knowledge historical time cues. For instance, at the start of each survey, the field researcher asked about the easiest information to recall: when they were born, when they got married, when they migrated (if applicable), using the historical common knowledge time cues. These life events provided respondent-specific time cues that field researchers then were trained to use for the remainder of the survey. This made it straightforward to determine the years at which the following events discussed in the survey took place, even when respondents were not sure a priory about the year.[38]

Third, we also administered working memory measurements. This allows us to weight observations by the ability of the respondent to memorize numbers, for robustness.

B.4 Detailed Description of Specific Survey Measures

B.4.1 History of Attacks on Household Members

Based on our own qualitative interviews and extensive discussions with specialists of the survey regions, we classify all the armed groups into: (1) Congolese militia, (2) Foreign armed groups, (3) Congolese army.[39]

The sources are a household attack history module and a household roster.

In the household attack history module, each respondent is asked to report up to nine attacks by armed actors that happened in the village where they live in. Each respondent on average reports 2.08 attack events; the $99\%^{th}$ percentile is seven events. Thus, reporting limit

[38]For instance, it was always easy for the respondent to answer whether they had acquired a cow before or after their marriage, or before or after the Second Congo War started.

[39]We built this classification on the basis of existing literature on the eastern Congolese conflict, qualitative fieldwork, and our quantitative data. For each armed group reported in the survey, we collected information on the name of the group, whether the group had been formed in the survey entity, the nationality of the leaders of the group and whether the group was perceived as local to the entity. Given the franchise character of armed movements such as the Mai-Mai or Raia Mutomboki, composed of numerous subgroups, the questions were geared toward the group that was present in the entity and not the movement as a whole. Furthermore, we triangulate the classifications of the household survey with those of the village survey.

did not lead to loss of data. For each event, we obtain perpetrators' identity, the perceived intention behind the attack, whether respondent was physically victimized, whether the household was pillaged, the number of fatalities in the village, the number of persons who suffered sexual violence in the village, and whether the village chief was victimized.[40]

The household roster contains information on all the members of the household at the time of the survey. For each household member, each respondent reported (1) up to three events in which armed actors victimized the household member directly, and (2) of these, up to three events in which the armed actor perpetrated sexual violence on the household member. For each of those events, we identify the year in which they took place.[41] Based on this information, we construct an indicator for whether respondent reported an attack on the household members for each year. We link the attack information in both modules.[42]

B.4.2 Household and Respondent's Economic History

For each year since 1995, we observe how many cows, goats, and pigs the household bought or sold; how many fields the household bought or sold; whether respondent held a marriage; and whether the respondent worked in any of the following categories: agriculture, school student, mining, government-related jobs, or unemployment. Information of farm animals is only asked in the South Kivu survey.

We obtain information on whether the respondent participated in militias or armed groups.[43] The information comes from the security module in the household survey. To obtain this, we first ask each respondent to list the armed groups that have been in the village where they lived at the time of the interview. For each armed group, each respondent is asked whether he had participated in it and if yes, the start date and end date. In addition, for respondents not born in the village of interview, in a separate module, we asked the

[40] We only ask whether village chief is victimized in the South Kivu survey.

[41] Since the household information module also elicits whether respondents themselves are attacked, this information overlaps with respondent attack module where respondents are asked whether they suffer from physical assault during attacks on village. We clean the latter information using the former.

[42] We use the attack information from both modules for each household member attack.

[43] We define participation as the active involvement in the security-related activities of an armed group.

respondent to describe each episode of participation in an armed group in years preceding the arrival to this village. Based on this information, we construct the respondent's history of participation in a Congolese militia (henceforth, "participation").

B.5 Summary of Variables

Table B.1: Variable Definition

Variable	Explanation
Access to road	Whether village i can be accessed through paved road in year t
Access to moto	Whether village i can be accessed by motorcycle in year t
Access to phone network	Whether village i is connected to phone network in year t
Endowed with coltan mine	Whether village i is engaged in coltan mining in year t
Endowed with gold	Whether village i is engaged in gold mining in year t
Number of immigrants	Number of villagers who migrated into village i in year t
Number of emigrants	Number of villagers who migrated out of village i in year t
% of subjects working in ag primarily	% of sampled respondents in village i who primarily work in agriculture in year t
% of subjects working in mining primarily	% of sampled respondents in village i who primarily work in mining sector in year t
% of subjects working in govt primarily	% of sampled respondents in village i who primarily work as civil servant in year t
% of subjects in school primarily	% of sampled respondents in village i who still go to school in year t
% of subjects unemployed	% of sampled respondents in village i who are unemployed or do not go to school in year t
Intention: Pillage	Whether village i has reported an attack by FDLR in year t whose intention is to pillage villagers
Intention: Punishment	Whether village i has reported an attack by FDLR in year t whose intention is to punish villagers
Intention: Conquest	Whether village i has reported an attack by FDLR in year t whose intention is to conquest other armed forces
Value Expropriated by FDLR (USD)	The estimated value of farm animals lost during the FDLR attack (including cows, goats, and pigs)
Attack with Deaths	Whether village i has reported an attack by FDLR in year t with any fatality
Attack with Forced Labor	Whether village i has reported an attack by FDLR in year t where FDLR forced or kidnapped any villagers for labor
Attack with Theft	Whether village i has reported an attack by FDLR in year t with any reported looting of farm animals
Attack with Sexual Violence	Whether village i has reported an attack by FDLR in year t with any reported sexual victimization on women
Attack: non-FDLR	Whether village i has reported an attack by non-FDLR armed group in year t
Monopoly of Violence	Whether FDLR has occupied village i in year t and has established monopoly of violence as a stationary bandit
Taxes	Whether FDLR has imposed any taxes on village i in year t (including poll tax, toll tax, sales tax, mill tax)
Value of Poll Tax per village yearly (USD)	The estimated value of yearly poll tax per household on village i in year t
Fiscal administration	Whether FDLR has administered any fiscal administration on village i in year t
Justice administration	Whether FDLR has administered any justice administration on village i in year t

Notes: This table presents the variable definitions used in this paper.

C Robustness

Table C.1: Selection Into the FDLR State

	Dependent Variable: *FDLR Attack*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDLR$_{\text{i}}$ x $I(t > 2009)$	0.23	0.22	0.22	0.23	0.23	0.23	0.23	0.25	0.21
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Control	Access Road	Access Moto	Access Network	Dist RWA	Dist River	Dist Road	Dist Airport	Coltan	Gold
Village FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,352	1,352	1,351	1,544	1,544	1,352	1,544	1,544	1,544
R^2	0.29	0.28	0.29	0.29	0.29	0.29	0.29	0.29	0.29
Village Clusters	193	193	193	193	193	193	193	193	193
Chiefdom-Year Clusters	168	168	168	168	168	168	168	168	168
Mean Dep. Var.	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741

Notes: This table presents the coefficient estimates from Equation 2. *Control*: includes, as control, the time-invariant variable indicated in that row, multiplied with indicator variables for each year in the sample. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

Table C.2: Alternative Econometric Specifications

	Dependent Variable: *FDLR Attack*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FDLR$_{\text{i}}$ x $\mathbf{1}(t > 2009)$	0.23	0.23	0.26	0.25	0.24	0.27	
	(0.06)	(0.07)	(0.03)	(0.07)	(0.07)	(0.12)	
$\mathbf{1}(t > 2009)$							0.16
							(0.07)
Log(Distance to FDLR State+1)$_{\text{i}}$ x $\mathbf{1}(t > 2009)$							-0.03
							(0.01)
Village FE	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y	N
Observations	1,544	1,536	880	1,544	1,544	1,536	1,536
R^2	0.06	0.29	0.31	0.29	0.29	0.30	0.28
Village Clusters	193	193	193	193	193	193	193
Chiefdom-Year Clusters	168	168	168	168	168	168	168
Mean Dep. Var.	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741

Notes: This table reports the coefficient estimates from Equation 2. The dependent variable is an indicator variable taking value 1 in village i and year t if village i is attacked by the FDLR in year t, and zero otherwise. Column (1) replaces village and year fixed effects with FDLR state and post fixed effects, (2) excludes the Chiefdom of Buloho, (3) clusters the standard errors at the groupment level (the unique administrative level below the Chiefdom and above the village), (4) includes the yearly world coltan price and (5) the gold price interacted with whether the village has coltan/gold, (6) controls for **Log(Distance to FDLR State+1)**$_{\text{i}}$ where Distance to FDLR State is the distance between village i and the FDLR state (in km) multiplied with year indicators. In this regression, all villages in the FDLR state have zero distance. In Column (7), **FDLR**$_{\text{i}}$ x $\mathbf{1}(t > 2009)$, is replaced with **Log(Distance to FDLR State+1)**$_{\text{i}}$ x $\mathbf{1}(t > 2009)$. For transparency, in that column, we omit the Province-Year fixed effects. This allows interpreting the coefficient on $\mathbf{1}(t > 2009)$ as the effect in the FDLR state. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

Table C.3: Specification with Lagged Dependent Variable

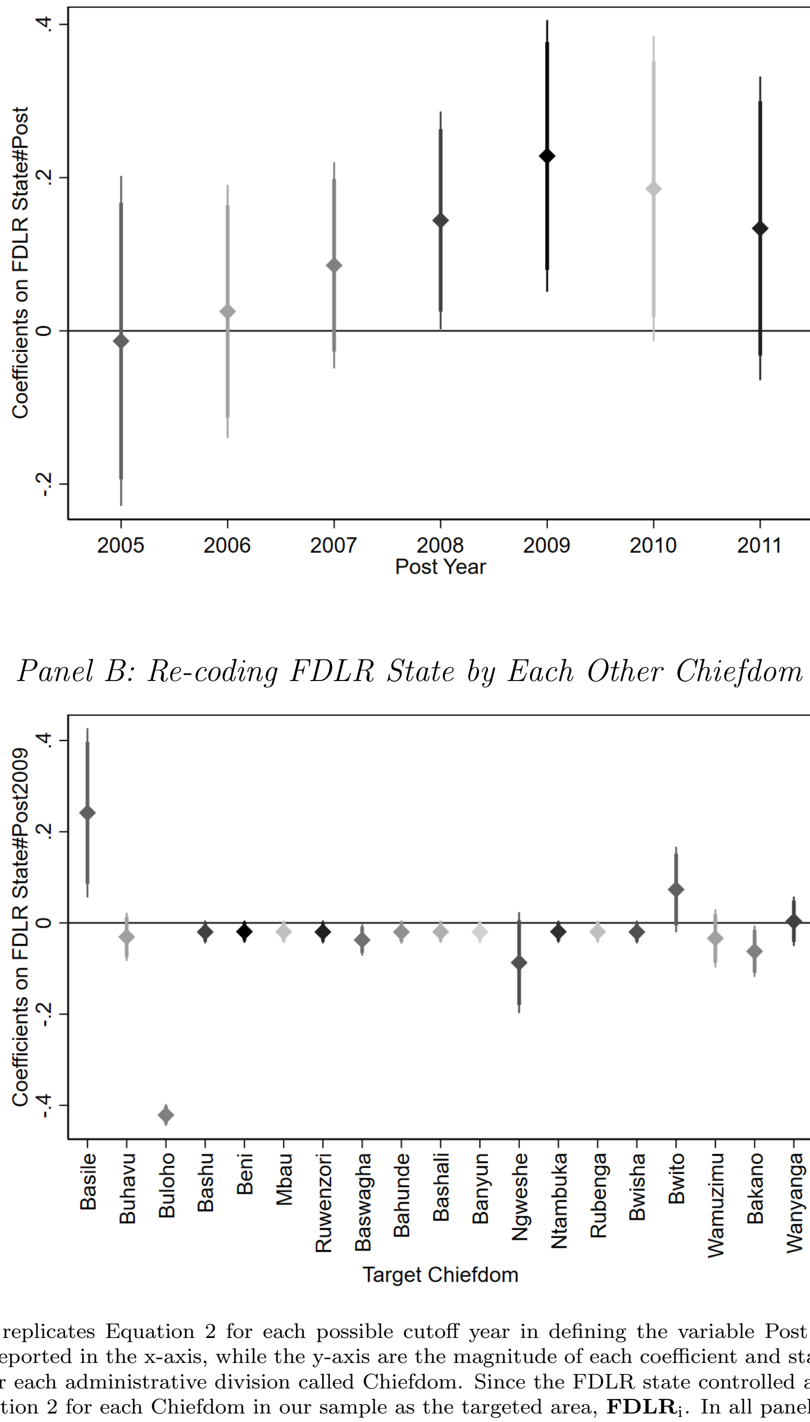
	Dependent Variable: *FDLR Attack*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FDLR$_i$ x $\mathbf{1}(t > 2009)$	0.22	0.22	0.22	0.24	0.15	0.23	0.22	0.24
	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.06)	(0.06)
Lagged Dependent Variable	Y	Y	Y	Y	Y	Y	Y	Y
Year-Province FE	Y	Y	Y	Y	Y	Y	Y	Y
Spillover Villages	N	Y	N	N	N	N	N	Y
District-Year FE	N	N	Y	N	N	N	Y	Y
Village Year Trends trends	N	N	N	Y	N	N	Y	Y
Time-Varying Controls	N	N	N	N	Y	N	Y	Y
Control for Migration	N	N	N	N	N	Y	Y	Y
Observations	1,351	1,351	1,337	1,351	1,204	1,101	977	1,256
R^2	0.12	0.12	0.14	0.12	0.14	0.18	0.22	0.19
Village Clusters	193	193	193	193	193	193	193	239
Chiefdom-Year Clusters	168	168	168	168	168	168	168	176
Mean Dep. Var.	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741	0.0741

Notes: This table reports the coefficient estimates from Equation 2, but instead of including village fixed effects, we include the lag of the dependent variable. The dependent variable is an indicator variable taking value 1 in village i and year t if village i is attacked by the FDLR in year t, and zero otherwise. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Spillover Villages*: includes the sample of villages where the FDLR is known to have been displaced (Chiefdom of Bakisi in Shabunda district). *District-Year FE*: are fixed effects for year and district. There are ten districts. The district corresponds to "Territoire" in the DRC's administration. *Village Year Trends:* include linear time trends for each village, separately. *Time-Varying Controls:* include controls for all characteristics that predict the location of the FDLR state by 2008, interacted with indicators for years. To implement this specification, we first estimate a probit model for whether a village belongs to the FDLR state on all observable characteristics. Then, for each village, we estimate the predicted probability that the village belongs to the FDLR state based on the vector of characteristics. We then estimate Equation 2, but include, as controls, the predicted probability interacted with year indicators. *Control for Migration:* includes controls for the number of immigrants and the number of emigrants yearly in each village. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

Figure C.1: Alternative Treatment Definitions

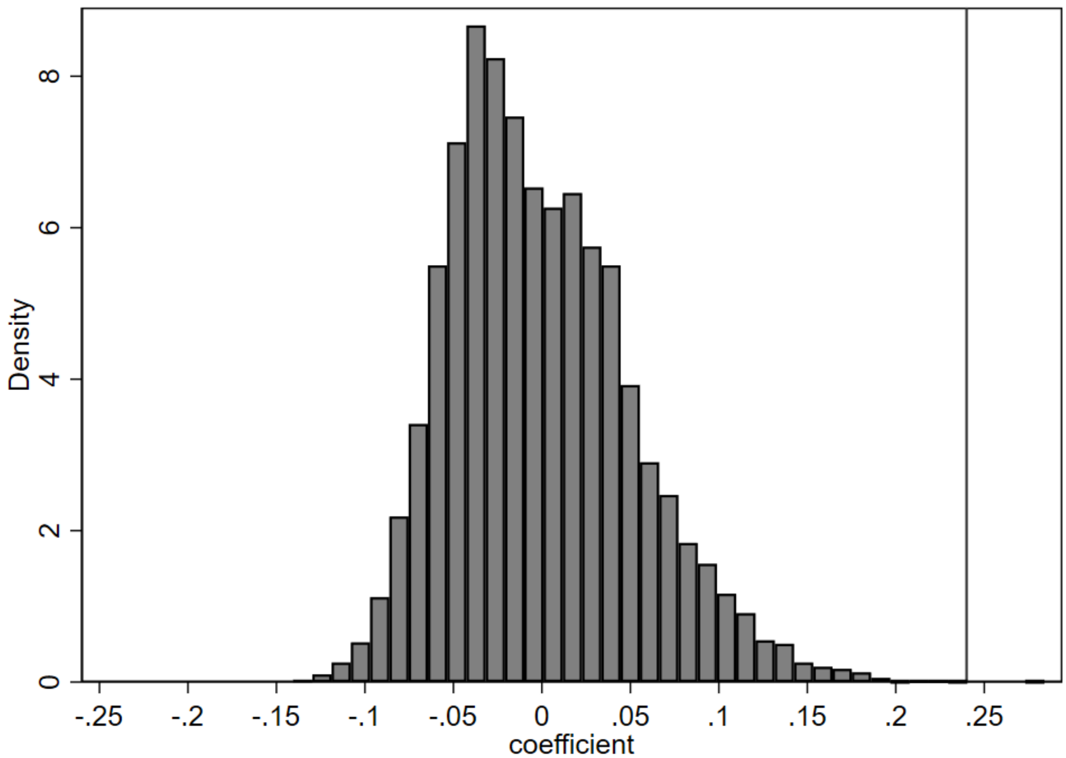
Panel A: Re-Coding the Cutoff Year for Kimia II Operation

Panel B: Re-coding FDLR State by Each Other Chiefdom



Notes: Panel A replicates Equation 2 for each possible cutoff year in defining the variable Post. The cutoff years for $I(t > 2009)$ are reported in the x-axis, while the y-axis are the magnitude of each coefficient and standard errors. Panel B does the same for each administrative division called Chiefdom. Since the FDLR state controlled an entire Chiefdom, we re-estimate Equation 2 for each Chiefdom in our sample as the targeted area, $\mathbf{FDLR}_i$. In all panels, thick lines represent 90% confidence intervals and thin lines represent 95% confidence intervals. Standard errors are clustered at the village level.

Figure C.2: Randomization Inference



Notes: This figure presents the distribution of estimated coefficients using randomization inference. We simulate 10,000 random assignments of FDLR state to villages, holding the fraction of targeted villages constant. For each simulation, we estimated Equation 2. The figure plots the distribution of those coefficients against the true coefficient as well as the associated p-value.The vertical line indicates the magnitude of the true coefficient.

D Replication Using Public Data (ACLED, 2020)

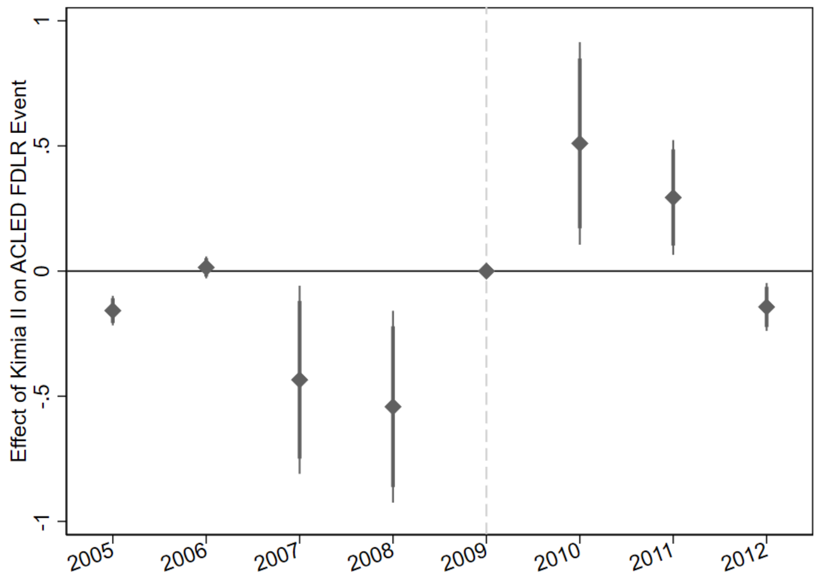
In this section, we present the analysis replication using ACLED (2020) data.

First, we replicate the main event study result. Figure D.1 replicates the event study estimation. The coefficients are positive after 2009, but are indistinguishable from zero before 2009, thus confirming the conclusion using the survey data.

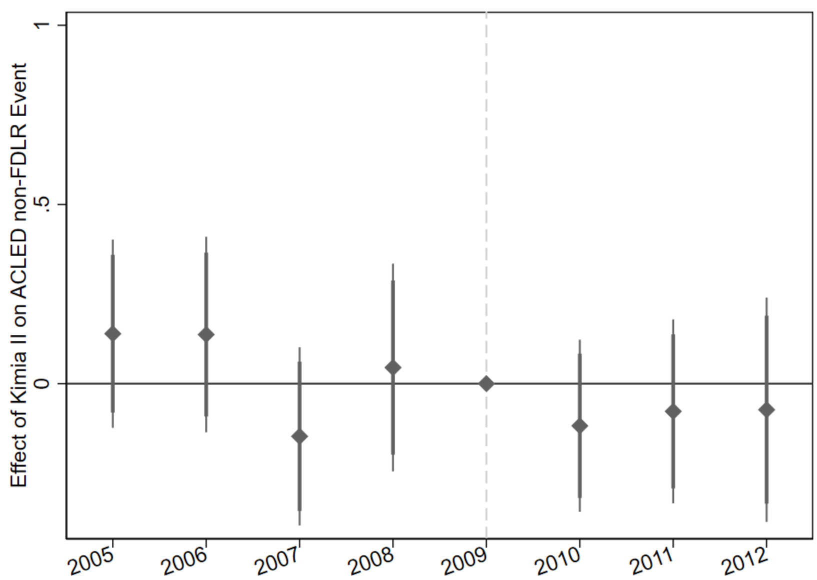
Second, we replicate the Differences-in-Differences estimation and implement robustness checks using the data in ACLED. Figures D.2 and D.3 estimate the Difference-in-Differences coefficients, using different definitions for matching of a violent event. Similarly, Figure D.4 estimates the Difference-in-Differences coefficients, using a different definition for the targeted area (using Mwenga district as targeted area). The results are preserved by each of these robustness checks.

Figure D.1: ACLED Data Replication of Main Event Study estimation

A. Main result: Attacks by FDLR

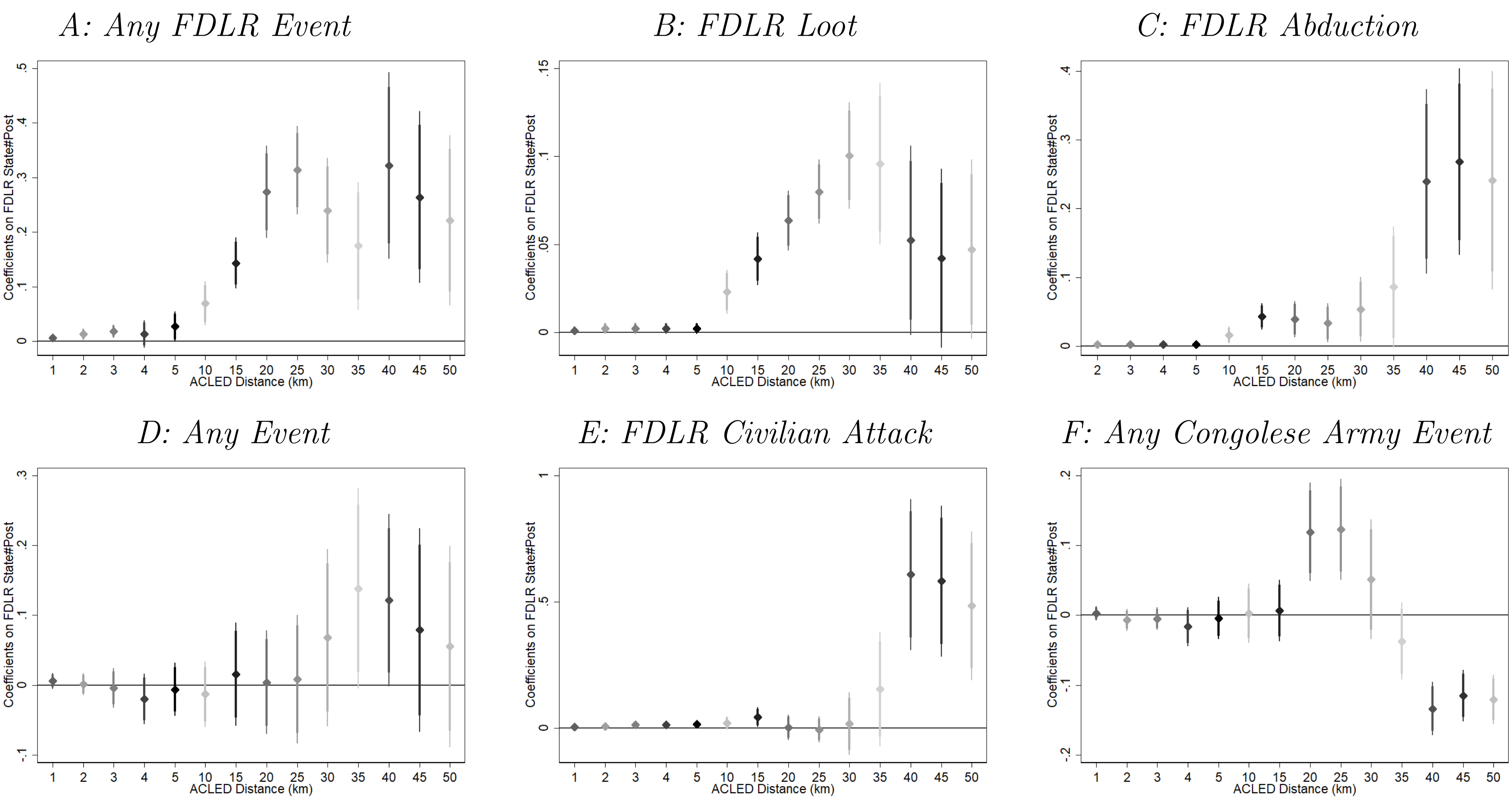


B. Falsification Exercise: Attacks by non-FDLR Armed Actors



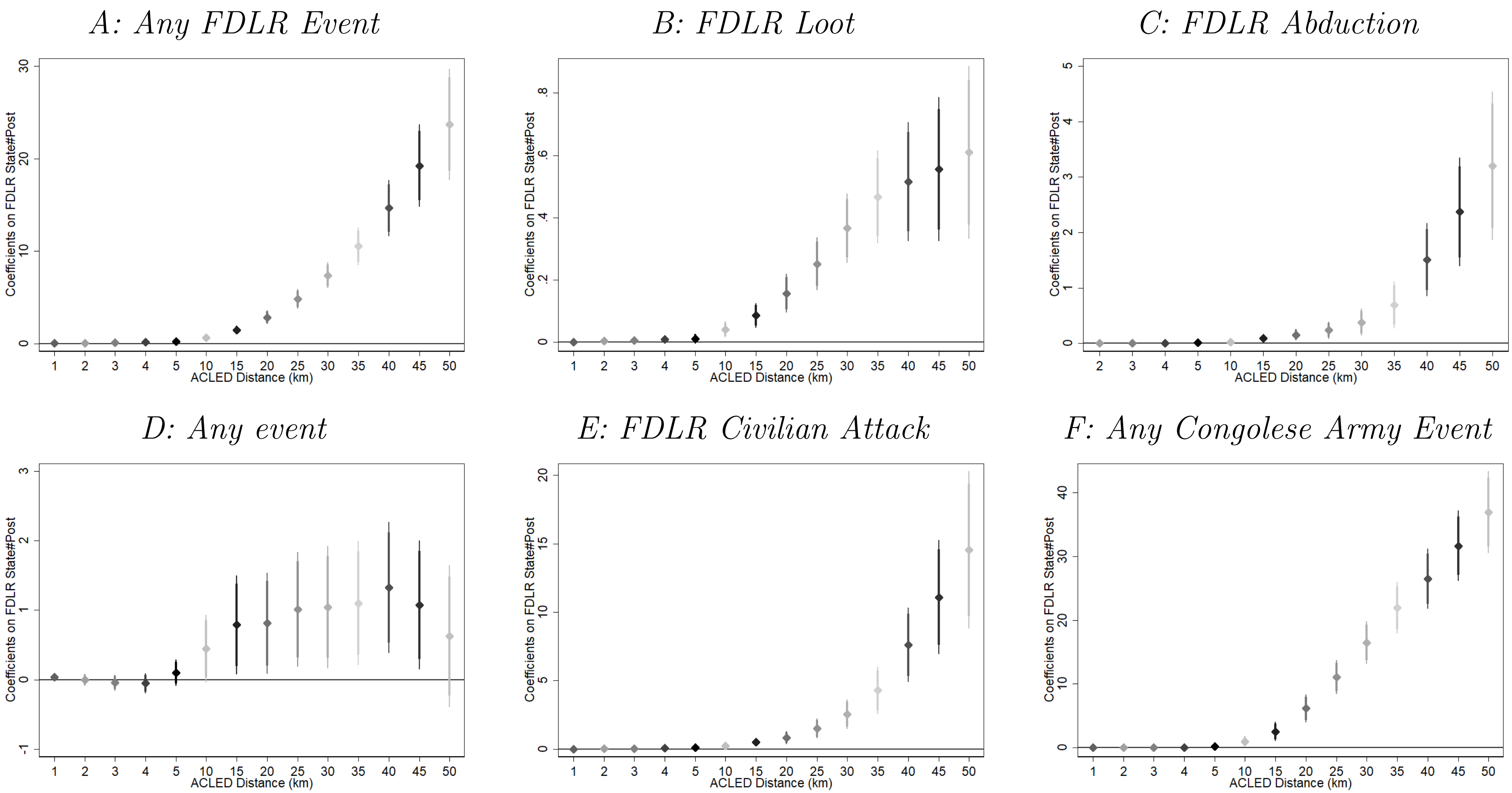
Notes: This figure shows the coefficients on year indicators estimated from Equation 1, using data from ACLED (2020). The year 2009 is the omitted category. The sample excludes Spillover Villages. Panel A shows the coefficients for the estimation using an indicator for FDLR pillages as dependent variable. Panel B (the placebo) uses an indicator for pillages by any other armed actor.

Figure D.2: Difference-in-Differences, by ACLED Event Type, for Different Radiuses Around Survey Village (Indicators)



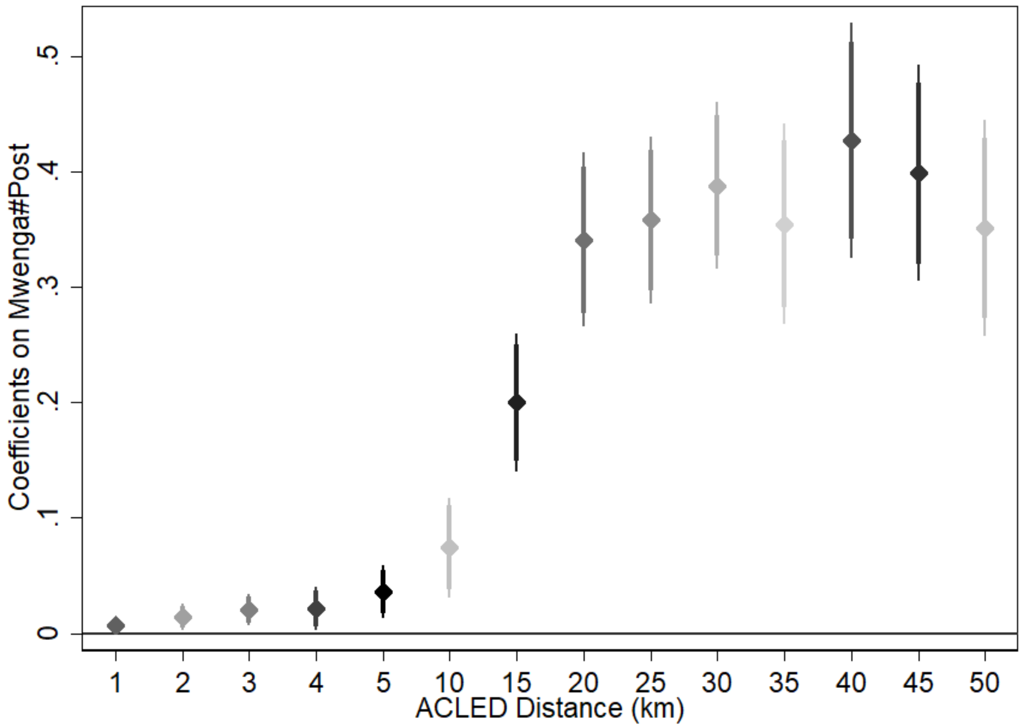
Notes: This figure shows the Differences-in-Differences coefficient using using indicator variables for different types of events different types of events in the ACLED data by the radius around the villages as dependent variables. In all panels, thick lines represent 90% confidence intervals and thin lines represent 95% confidence intervals. Standard errors are clustered at the village level.

Figure D.3: Difference-in-Differences, by ACLED Event Type, for Different Radiuses Around Survey Village (Number of Events)



Notes: This figure shows the Differences-in-Differences coefficient on different types of events in the ACLED data by the radius around the villages. In contrast to Figure D.2 this figure uses a continuous variable of the number of events within a specific radius around the village. In all panels, thick lines represent 90% confidence intervals and thin lines represent 95% confidence intervals. Standard errors are clustered at the village level.

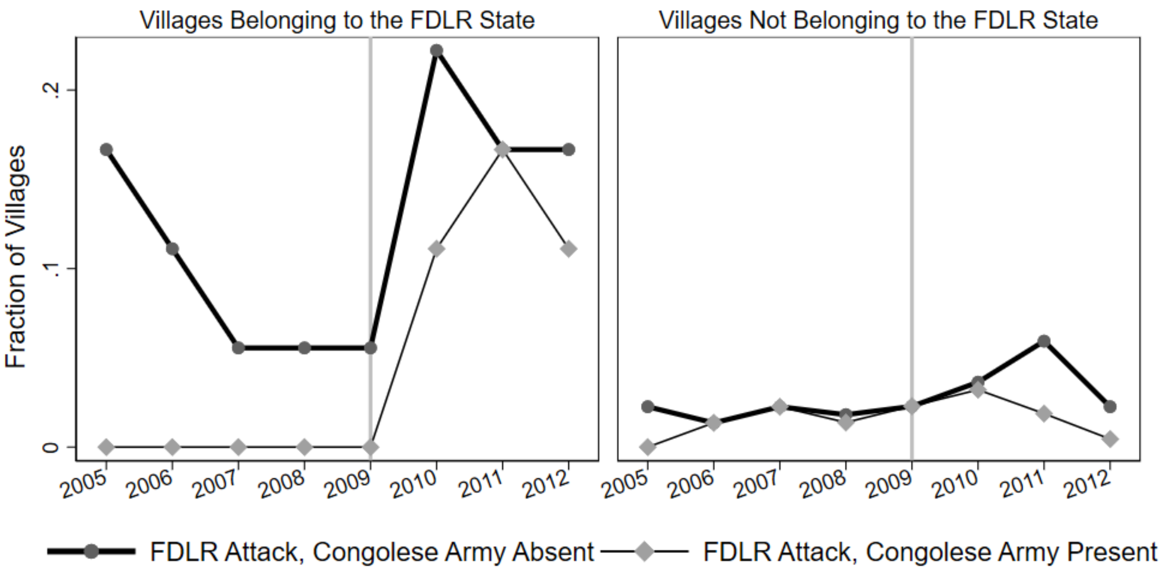
Figure D.4: ACLED, using Mwenga District as Target



Notes: In this figure, FDLR state is coded as the entire district of Mwenga. The dependent variable is an indicator variable taking value one if there is an FDLR attack as recorded by ACLED in a circle of corresponding radius from the survey village, and zero otherwise. The figure shows the Differences-in-Differences coefficients using different bandwidths around the FDLR state villages. Standard errors are clustered everywhere at the village level.

E Additional Analysis

Figure E.5: Trends of FDLR Taxation and Attacks, by Congolese Army Control



Notes: This figure shows the incidence of FDLR attacks separately for whether the Congolese army controlled the village and was present in the village at the time of the attack (Congolese army present) and whether the attack took place in a village either not controlled by the Congolese army or controlled by the Congolese army but where the Congolese army was absent during the attack. Spillover Villages are removed.

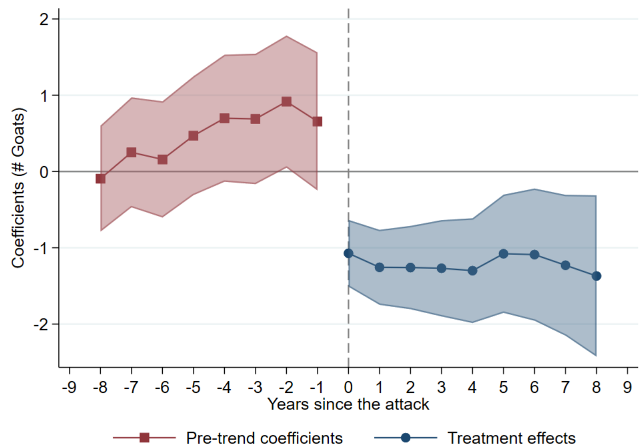
Table E.1: Differences-in-Differences Estimate on Pillages, Broken down by its Components

	Any Attack (1)	Any Pillage (2)	*Intention Pillage* (3)	*Action Theft* (4)	*Action Kidnap* (5)
FDLR State$_i$ x $I(t > 2009)$	0.24	0.24	0.22	0.19	0.24
	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)
Village FE	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y
Observations	1,544	1,544	1,544	1,544	1,544
R^2	0.29	0.29	0.27	0.25	0.25
Village Clusters	193	193	193	193	193
Chiefdom-Year Clusters	168	168	168	168	168
Mean Dep. Var.	0.0741	0.0741	0.0370	0.0741	0.0556

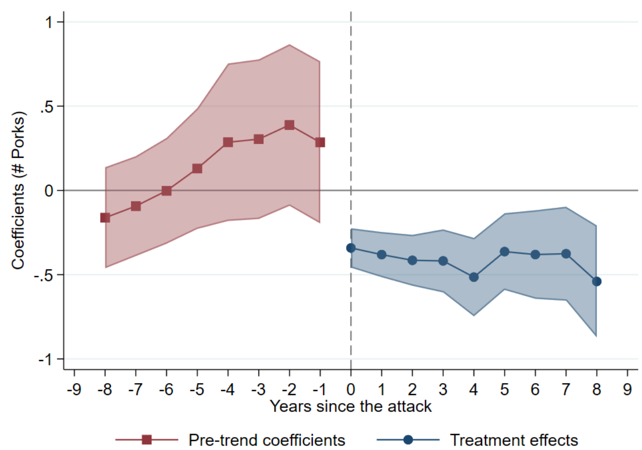
Notes: This table reports the coefficient estimates from Equation 2. The dependent variable is an indicator variable taking value 1 if village i in year t is: (1) attacked by the FDLR (2) pillaged by the FDLR, as captured by the variable "Any Pillage," and zero otherwise. The following columns decompose the components of "Any Pillage" into separate regressions. The dependent variable is an indicator variable taking value 1 if village i in year t is: (3) attacked by the FDLR with the purported intention to pillage, (4) attacked by the FDLR in an attack in which theft by FDLR of household property in the village is recorded, (5) attacked by the FDLR in an attack in which abduction of villagers by the FDLR is recorded. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

Figure E.6: Inter-temporal Trade-offs: Effect of a Pillage on a Household Cattle Stock

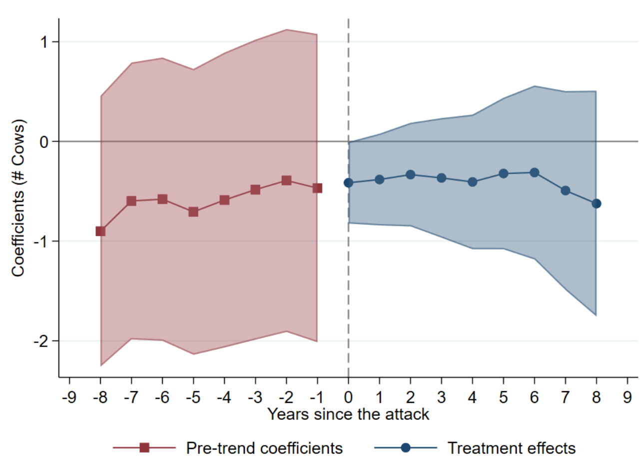
A. On Number of Goats Owned



B. On Number of Porks Owned



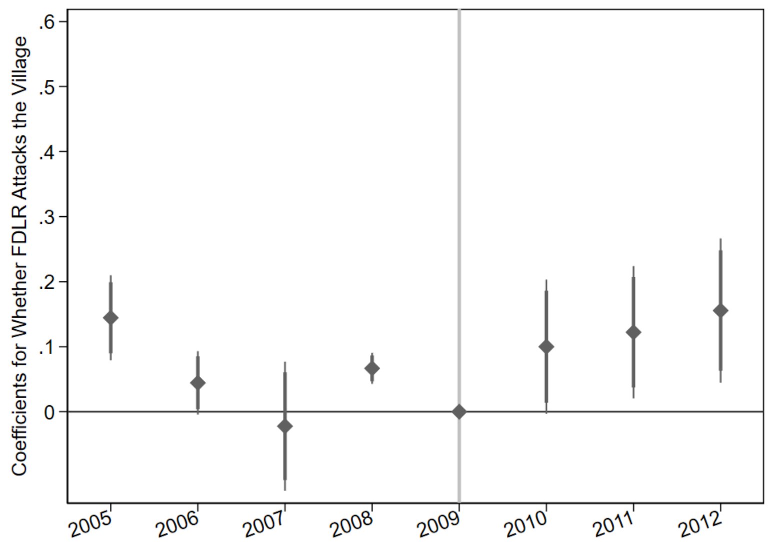
C. On Number of Cows Owned



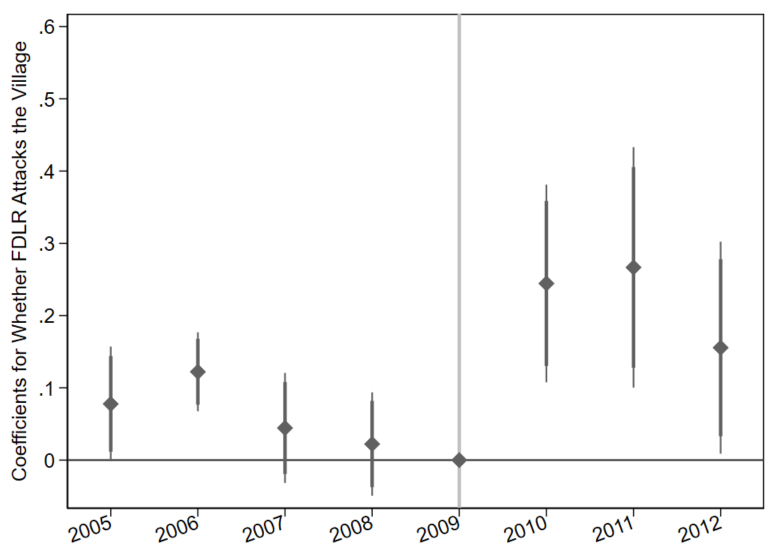
Notes: This figure shows the coefficients from Borusyak et al. (2020)'s efficient and robust estimator for the effect of a pillage on the household's stock of goats, porks, and cows.

Figure E.7: The Role of Frequency of Expropriation: Effect by Whether the Village is Accessible by Foot

A. Attacks by FDLR in Villages Accessible by Car or Motorbike



B. Attacks by FDLR in Villages Only Accessible by Foot



Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1. The year 2009 is the omitted category. 1,544 village-year observations were used in the estimation. In Panel A, the dependent variable is an indicator for whether the FDLR attacks village i in year t and the village is accessible by car or motorbike. The R-squared is 25.2%. In Panel B, the dependent variable is an indicator or whether the village is only accessible by foot. The R-squared is 35.8%. There are 63 and 56 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.

Table E.2: Implications for Household Welfare: Expropriation Indicators

	τ^P	τ^T					
	Pillage	Taxation					
	Theft	*Any*	*Market*	*Mill*	*Toll*	*Poll*	*Mine*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FDLR State$_i$ x $I(t > 2009)$	0.24	-0.29	-0.11	0.04	-0.32	-0.46	-0.09
	(0.09)	(0.11)	(0.10)	(0.02)	(0.11)	(0.12)	(0.06)
Village FE	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y	Y
Observations	1,480	1,480	1,480	1,480	1,480	1,480	1,480
R^2	0.29	0.67	0.63	0.51	0.72	0.60	0.77
Village Clusters	185	185	185	185	185	185	185
Chiefdom-Year Clusters	168	168	168	168	168	168	168
Mean Dep. Var.	0.04	1.00	0.28	0.00	0.83	0.94	0.38

Notes: This table reports the coefficient estimates from Equation 2. The dependent variables in columns (1)-(7) are indicator variables taking value one if any of the outcomes listed in the headers is recorded in the village and year, and zero otherwise. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II. While we are unable to estimate the total tax payments at the mines with precision, this table reports whether tax payments at the mine took place. Since the effect is not significant and goes in the expected direction, our main estimates of tax payments are an under-estimate of the effect of Kimia II of total tax payments.

Table E.3: Effect on Household Savings

	Cattle				
	Net s^*	*Purchase*	*Sale*	Theft τ^p	Stock $\bar{S}$
	(1)	(2)	(3)	(4)	(5)
$\mathbf{FDLR}_{\text{i}}$ x $\mathbf{1}(t > 2009)$	0.24	0.08	-0.16	0.53	0.05
	(0.08)	(0.08)	(0.06)	(0.06)	(0.04)
Observations	4,153	4,153	4,153	6,519	4,152
R^2	0.04	0.04	0.05	0.08	0.11
Village FE	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y
Village Clusters	187	187	187	187	187
Chiefdom-Year Clusters	160	160	160	160	160
Mean Dep. Var.	0.05	-0.03	-0.08	-0.08	-0.01

Notes: This table reports the coefficient estimates from Equation 2. The dependent variable is continuous variable aggregated at the village-year level (by averaging across households within village-year) and standardized to mean zero and standard deviation one for: (1) Net acquisition of cattle, (2) Purchase of cattle, (3) Liquidation (sale) of cattle, (4) Amount of cattle stolen by armed actors from the village households through pillage attacks (5) Imputed stock of cattle. Standard errors, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in two-way Village and Chiefdom-Year Clusters, respectively. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II.

F Mathematical Appendix

F.1 Simple Model of Expected Frequency of Expropriation

Time, indexed by t, is discrete and runs forever. Consider that one unit of time is a day. The economy is populated by a bandit, who controls a village. Each period, the bandit may be able to expropriate in the village with exogenous probability p, otherwise cannot expropriate. This captures the security of the bandit's property rights over the village. The village yields expropriable wealth $a_t \in \mathbb{R}$, with law of motion $a_{t+1} = R(a_t - \tau_t)\theta(s_t)$, where $R > 0$ is an exogenous rate of wealth reproduction, τ_t is the bandit's expropriation in period t, $\theta(s_t)$ is state functions, with $\theta'(s_t) > 0$, $\theta''(s_t) < 0$.

Expropriable wealth in period $t+1$ is a function of state functions in period $t+1$, $\theta(s_{t+1})$, which the bandit can invest in through actions s_{t+1} that increase wealth in period $t+1$, such as protection and courts, and actions that increase ability to expropriate in period $t+1$, such as fiscal administration. Taking those actions is costly to the bandit. The bandit consumes τ_t net of the cost of investing in state functions, yielding $u(\tau_t - s_t)$, where $u'(\tau_t - s_t) > 0$, $u''(\tau_t - s_t) < 0$. He chooses $\{\tau_t, s_t\}_{t=0}^{T=\infty}$, to maximize $\sum\limits_{t=0}^{\infty} \delta^t p^t u(\tau_t - s_t)$, where $\delta \in (0,1)$ is time preferences. $p\delta$ is the effective discount rate. Recursively,

$$V(a_t) = \max_{\tau_t, s_{t+1}} \{u(\tau_t - s_t) + \delta V(a_{t+1})\}, \tag{3}$$

with $a_{t+1} = R(a_t - \tau_t)\theta(s_{t+1})$. This leads to the following two Equations:

$$\frac{u'(\tau_t)}{u'(\tau_{t+1})} = \delta p R \theta(s_{t+1}) \tag{4}$$

$$\frac{\theta'(s_{t+1})}{\theta(s_{t+1})} = (a_t - \tau_t). \tag{5}$$

Proof: envelope theorem and first order condition of the Bellman Equation and some algebra.

Equation 4 is the Euler Equation for τ_t. Equations 4 and 5 imply that p decreases the level of expropriation, τ_t^*, and increases the investment in state functions, s_{t+1}^*. If $p = 0$, $\tau_t^* = a_t$, the bandit expropriates everything.

Implications. The bandit's level of expropriation decreases in the degree of security of property rights of the bandit over the revenues from expropriation of the village. This effect arises because, with weaker property rights over the return on their investment from reducing the level of expropriation today, for instance when the state holds a monopoly of violence, the bandit internalizes a lower share of its effect of expropriation today on village growth, resulting in a higher level of optimal expropriation today. While we do not explicitly model the bandit's decision to use violence to implement the desired level of expropriation, in reality, it is intuitive that the higher the level of expropriation, the more violence is required to implement expropriation—intuitively, when the level of expropriation is high, villagers will resist, and violence can induce compliance.[44] Similarly, the ability to frequently expropriate can sustain state functions by the bandit.

F.2 Framework for Analyzing Household Welfare

A unitary household chooses:

$$\begin{aligned} \max_{c,s} \quad & v(c) - \theta P \\ \text{s.t.} \quad & c + s = (1 - \tau^T)y \\ & s - \tau^P \geq \bar{S} \end{aligned} \tag{6}$$

where $v(c)$ is continuously differentiable and $v'(c) > 0$. P is an indicator function for whether a pillage occurs and $\theta \in \mathbb{R}^+$, y is exogenous income, τ^T captures taxation household income. The constraint $s - \tau^P \geq \bar{S}$ is the household survival constraint, where $\bar{S} > 0$ is the minimum savings required to survive and τ^P is the value of savings lost through pillaging.

[44] Formally, this intuitive result can be obtained when violence is in the bandit's choice set and citizens can choose effort. For instance, Acemoglu and Wolitzky (2011) show that violence (coercion) is complementary with the agent's effort. For a classical treatment of violence in a principal agent framework, see Chwe (1990)'s model of worker whipping.

Proposition 1. *Household welfare, $v(y-(\tau y+\tau^P)-\bar{S})-\theta P$, decreases in P and $\tau^T y+\tau^P$ but increases in y. Savings, $s^* = \bar{S}+\tau^P$, is increasing in τ^P.*

Proof of Proposition 1. Substitute c into the objective function. We get:

$$\begin{aligned} \max_{s} \quad & v((1-\tau^T)y - s) - \theta P \\ \text{s.t.} \quad & c + s = (1-\tau^T)y \\ & s - \tau^P \geq \bar{S} \end{aligned} \tag{7}$$

It is clear that the objective function is decreasing in s, thus the survival constraint binds with equality, $s - \tau^P = \bar{S}$. This yields the household optimal savings and consumption choices, $s^* = \tau^P + \bar{S}$ and $c^* = (1-\tau^T)y - \tau^P - \bar{S}$. Substituting s^*, c^* into the objective function yields the expression for household welfare.

□