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MONOPOLY OF TAXATION WITHOUT A MONOPOLY OF VIOLENCE: THE WEAK STATE'S TRADE-OFF BETWEEN TAXATION AND SAFETY

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Abstract— We propose a new perspective on the challenges of building a state. We show in the context of a weak state that asserting the state can give existing armed actors incentives to plunder. We combine data from 239 villages of eastern Congo with quasi-experimental variation induced by the completion of a large military campaign which asserted the state's exclusive right to tax in specific villages. After the campaign was complete, and during three years, members of the armed factions previously taxing those villages regularly attacked them. The rise is driven by violent theft operations targeting wealthy households, and is muted for retaliatory attacks, conquest operations, as well as for attacks by other perpetrators. The targeting is consistent with a destruction of their incentive to refrain from violent theft. Asserting the state's exclusive right to tax increased household material welfare, by decreasing overall tax burdens, but it increased sexual violence and abductions as a byproduct of the theft operations. An alternative state building strategy based on bargaining with armed actors rather than attempting to diminish their ability to tax does not create this incentive. Our findings suggest that asserting the state by removing armed actors who have established themselves, tax, and protect, can induce a temporary trade-off between growth and safety and challenge conceptions of state power based only on monopoly of violence. **JEL Codes:** H11, P48, D74. **Keywords**: State, State Capacity

We thank Gauthier Marchais, Jean-Paul Zibika, and all members of the Marakuja Kivu Research team, for invaluable support. Sánchez de la Sierra thanks Robert Bates, Christopher Blattman, Pierre-André Chiappori, Donald Davis, Avner Greif, Macartan Humphreys, Suresh Naidu, Nathan Nunn, James Robinson, Bernard Salanié, Noam Yuchtman 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 organization of which Mugaruka, and Sánchez de la Sierra are co-founders.

"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

This paper provides a detailed description of the effect of a military campaign, designed to assert the state's territorial control in the context of a state unable to hold a monopoly of violence. The campaign made it impossible for non-state armed actors to tax. We empirically characterize the transition from stationary to roving bandit (Olson, 1993) by these non-state armed actors. The evidence we present is largely descriptive, and supported by hundreds of qualitative interviews with individuals who experienced the campaign and its aftermath, and lived with the armed actors. It is also supported by quantitative analysis drawing on a unique original dataset on violence that we gathered through years of fieldwork in each community. The evidence paints a coherent picture of the non-state armed actors' ability to tax as a crucial deterrent for their incentives to use violence.

Weak states pose many of the greatest development challenges. Motivated by the notion 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 in the territory they control (Weigel, 2020, Balan et al., 2021, Bergeron et al., 2021). However a central feature of weak states is that they do not even control most of their *de jure* territory. For instance, recently, nonstate armed groups controlled one of the largest and most mineral-rich districts of eastern Democratic Republic of the Congo (DRC) (Stearns, 2013). How can a weak state even tax those areas? This article studies how attempts to remove nonstate armed actors can backfire, precisely because they disrupt the very source of their stability, their ability to tax.

We examine this question in the context of a large operation in South Kivu province, DRC, in 2009, Kimia II, aimed at gaining territory over the Forces de Libération du Rwanda (FDLR). Some characteristics of this campaign make it well-suited to elicit the response of armed groups to curbing their ability to tax. First, Kimia II targeted the Basile Chiefdom and ended in December 2009, enabling us to exploit spatial variation and separately analyze violence before, during, and after Kimia II. Second, Kimia II was the product of a high-level political concession made by the DRC to Rwanda, triggered by negotiations

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

related to a rebellion in another province, making reverse causality unlikely. Third, the campaign originally sent 30,000 Congolese and UN soldiers to remove at most 6,000 FDLR soldiers. Experts have documented that it swiftly weakened the targeted factions' ability to tax in the targeted villages, encountering little resistance in those villages (Sawyer and Van Woudenberg, 2009). This allows to shut down immediate effects due to fighting. Finally, the targeted factions' revenues were self-managed by each FDLR company (Florquin and Debelle, 2015), allowing us to isolate the effect of the disruption of the ability to tax in a village, from any immediate nationwide strategic FDLR response.

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. We show that, despite their tendency to attack civilians in the region, violence was almost absent in those villages. Consistent with existing accounts of the conflict (Sánchez de la Sierra, 2020), they ran fiscal administrations and collected taxes daily. We show that, within the year, Kimia II succeeded in making it impossible to tax in the targeted villages.

Reconstructing and triangulating the history of violent events in eastern DRC, we then show that, during each of the three years after Kimia II was complete, the targeted armed factions attacked the same villages they formerly protected. We use an event study analysis and a Differences-in-Differences framework to estimate the effect of Kimia II on the incidence of attacks by the targeted factions against the targeted villages. We find that completion of Kimia II is associated with an increase in 267% in the propensity that a targeted village is attacked by the targeted factions for the three years after. We then manually correct the location of each event in public data (ACLED, 2020), and use the corrected data to replicate this analysis. Our results are *consistent with* Kimia II completion causing a rise in FDLR attacks. We examine potential confounds: 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 public data on infrastructure in a Differences-in-Differences framework. We also perform various specification checks. 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 FDLR attacks in the targeted villages indirectly through its effect on the industrial organization of violence. Specifically, Kimia II completion could have triggered armed group activities in the targeted area, leading to a rise in violence in that area, but not specific to the targeted factions. Kimia II completion may also have destabilized eastern DRC, making all armed groups more violent in the areas in which they operate. Given potential concerns about this assumption, we use data on violence by all other armed actors. First, we find that violence by other armed actors in the targeted villages is unaffected. Second, we find that it is also unaffected in the villages the other armed actors controlled the year prior to the campaign. This suggests that Kimia II *directly* fostered the rise in attacks.

Our analysis then turns to the potential channel. Our qualitative interviews suggest that the attacks are driven by expropriation of household assets. Namely, following Kimia II, the armed actors formerly controlling 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, such as when they taxed. We use our information on each attack 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 tax collection in the village. We find that Kimia II induces only pillaging attacks by FDLR. Consistent with this conjecture, using retrospective household panel data, we find that the rise is driven by attacks that target the wealthiest households. The targeting of the attacks induced by Kimia II is consistent with those being motivated by expropriation. Supported by our qualitative interviews, our data is consistent with Kimia II causing violence by disrupting their expected ability to frequently expropriate, reducing the return on refraining from (violent) maximal per period expropriation.

We then consider leading alternative explanations for the rise in violence. First, we analyze whether the rise in attacks could be explained by fighting with the Congolese army. We find that Kimia II had no effect on attacks for which the purported intentions included territorial control. Furthermore, the rise in attacks is not driven by attacks that target the Congolese army positions. Second, we analyze whether the rise could be explained by the targeted factions' incentives to punish villagers who potentially cooperated with the state or took the opportunity to forgo debt payments with the factions. We find that Kimia II also had no effect on attacks for which the purported intentions included punishing villagers. Furthermore, the rise in attacks is not driven by attacks that target households of the village political elite, former armed group or army members—or who likely accumulated debt with the factions during the factions' governance episode. This pattern is inconsistent with conquest or punishment as mechanisms for the effect of Kimia II on FDLR attacks.

We complement this analysis by examining the implications of Kimia II on household well-being. We find that Kimia II decreased the total value expropriated per household, from 25.5% to 15.5% of yearly per capita income and the fraction of unemployed, and increased the fraction of respondents who get married and who are in school, normal goods, indicating a rise in income. However, this improvement came at the cost of intensified sexual violence, theft, and abductions (all as a by-product of the pillages). Given the violations of human dignity caused by Kimia II, utilitarian criteria based on material outcomes are insufficient for making welfare comparisons. It is important for positive analysis of those policies that civilians in the targeted areas, not state officials nor civilians in other areas, bear the external cost indirectly created by military operations.

If asserting territorial control is not a desirable strategy for a weak state, then what is? We describe a 2004 agreement in which armed actors, rather than being made unable to tax, were given positions inside the state army, and analyze its effects in the data. Consistent with the rich existing historical evidence of that episode, we find that this agreement led to a drastic reduction, rather than increase, in violence. This analysis suggests that, omitting political motivations that sometimes prevent these types of agreements, building states through bargain rather than through force and relinquishing the sacred view of the state in the international state system is likely a safer path for the citizens of weak states.

Our findings provide a critique of territorial conceptions of power that underpin the idea of a "*monopoly of violence*." Weber (1946)'s formulation about the state specifies that it is the organization that enjoys such a monopoly within a given territory. Consistent with Herbst (2011) argument about power in Africa, this paper demonstrates the pitfalls of this notion in weak state settings. If judged from the traditional territorial lens, then the campaign was a success. The rebels were ousted and the territory was reclaimed. The economic welfare of villagers increased. But when we consider general equilibrium effects of the campaign, the picture becomes murkier. In fact, the rebel group does not just disappear but responds by transforming from stationary to roving bandit and thus pillaging, causing a range of assaults to human well-being and dignity. The toll on well-being from the pillaging is inescapable to assess the impact of Kimia II. A monopoly of violence is, in fact, a rare occurrence in the historical record, and this critical juncture illustrates a limitation of conflating the monopoly of taxation with the monopoly of violence.

The paper empirically documents the transformation of a stationary bandit into a roving bandit (Olson, 1993). Although people sometimes attribute this to Sánchez de la Sierra (2020), that is not accurate in fact. This paper demonstrates that transformation of stationary to roving bandit. Our explanation based on time horizons, which has never been tested before in the context of stationary bandits, is the mechanism first described in Olson (1993).

A growing literature in economics and political science is concerned with how building states by force may backfire (Magaloni et al., 2020). It is well established that state efforts to assert control over its territory can result in more violence. In economics, Dell et al. (2018), showed that building states by force may increase violent rebellion. Acemoglu et al. (2020) show that high-powered incentives for state agents to combat nonstate armed actors lead to excessive violence by state agents. Regaining territory can also increase the attempts by gangs to control the population (Blattman et al., 2021). In political science, a large literature suggests that, when groups that exert governance are challenged by the state, they react violently to hold on to their strongholds. For instance, Calderón et al. (2015) find that removing criminal gangs leaders can increase inter- and intra-cartel fighting. In a landmark study of the conflict between the state and gangs, Lessing (2017) provides rich qualitative evidence across contexts in Latin America to show that attempts by the state to fight cartels incentivize cartels to fight back, driving spirals of violence and corruption.

Our paper complements this literature in two ways. First, we introduce a new mechanism for rise in violence. This mechanism is intuitive only once we account for the pre-existing relationships between armed actors and society. We show that when the local governance of an armed group is challenged by the state, violence increases as a tool to plunder, for various years. The evidence we provide to discriminate between an income effect and the expectations of future expropriation frequency is only suggestive, and the time horizon has also been proposed as an explanation for behavior in macroeconomics (Gollier, 2002), corporate governance (Shleifer and Vishny, 1990), corruption by bureaucrats in democracies with the rule of law (Ferraz and Finan, 2011), and agricultural investment (Yamasaki, 2020). However, our finding that the policy causes violence by disrupting pre-existing relationships of expropriation is conclusive. This finding is in line with Acemoglu and Robinson (2013) argument that policy interventions can disrupt "political equilibria." Second, our study is the first to empirically analyze, jointly, the long-run economic benefits of the state, and contrast those to the short- costs arising when removing nonstate armed actors.

Our results are also of urgent policy importance. Given the desire to build states around the international state system, 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 disruption caused by removing nonstate armed actors, because it is difficult to obtain systematic data on the relationships between armed actors and society (Staniland, 2012). It is also not obvious that armed groups could seamlessly be removed by force. The recent Taliban conquest of Afghanistan, which took some observers by surprise, and the costly years of violence against civilians that preceded it, illustrates the importance of understanding how attempts to build a state can influence the relationships between these armed actors and society, affecting societal welfare (Malejacq, 2020, Mukhopadhyay, 2014).

2. THE KIMIA II CAMPAIGN

The Front De Libération du Rwanda (FDLR) emerged in 2000 from Hutu Rwandan armed forces and militia members that perpetrated the 1994 Rwandan genocide. They are known as one of the most brutal groups in DRC (KST, 2021, Florquin and Debelle, 2015).[2]

Origins of Kimia II. A crucial feature of Kimia II is that it was decided privately as part of deal between the DRC and the Rwandan Presidents. Specifically, in 2008, there was a one year rebellion in North Kivu, the Congres National de la Defense du Peuple (CNDP). The CNDP benefited from cross-border support (and were orchestrated) by Rwanda. Rwanda's aims included hunting down one of the major armed groups of eastern DRC, the FDLR, considered a national security threat in Rwanda due to their anti-Tutsi ideology and national ambitions. The CNDP served Rwandan interests to fight the FDLR in DRC. The end of the CNDP rebellion in 2009 was implemented by Rwanda and the CNDP leadership as part of a deal between the Presidents of the DRC and Rwanda. DRC President Kabila agreed to integrate the CNDP commanders into the DRC army in order to fight the FDLR. In exchange, the Rwandan government agreed to arrest Laurent Nkunda, a major CNDP commander, whose resilience had become a burden on Kabila's legitimacy. As a result

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

of the withdrawal of Rwandan support that was agreed in the deal, the CNDP collapsed swiftly. Overall, by the start of 2009, 6,000 CNDP former combatants were integrated into the DRC army in an accelerated fashion. Benefiting from the integration deal, some former CNDP commanders were assigned to mineral-rich areas of North Kivu. There, they gained controlled over illicit revenues, possibly as a way to provide informal compensation part of the deal. Bosco Ntaganda, a former CNDP commander, became Deputy Commander of a newly created military operation inside the DRC national army, operation Kimia II. Thus, the conditions of Kimia II's timing and geographical focus are unlikely to be related to those that led to FDLR violence in South Kivu after Kimia II.

Kimia II. Unlike the state did in the Sun-City peace agreement of 2004, in which various armed groups (excluding the FDLR) were integrated into the Congolese army, the state opted for asserting the state's territorial control rather than integrating the FDLR into the state. The reasons for this decision are political: it not an option to integrate the FDLR due to pressure from Rwanda and from the local population. Thus, in March of 2009, the Congolese army and the UN peacekeeping forces (MONUSCO) launched Kimia II, a joint operation against the FDLR. Kimia II had a spatially designated target: the FDLR factions stationed in parts of the Province of South Kivu, especially the chiefdom of Basile. 22,000 Congolese soldiers, and 8,000 MONUSCO soldiers participated. The FDLR as a whole was believed to have at most 6,000 soldiers in 2008 (Florquin and Debelle, 2015). The asymmetry in their power endowment could explain why the DRC army resorted to restrained violence in those villages (unlike in other parts of eastern DRC). As the DRC army advanced, the FDLR retreated. From March to December 2009, DRC army and UN forces advanced from north to south and made it impossible for the FDLR to exert a stable presence in the villages of the Chiefdom of Basile. Kimia II ended in December 2009, but DRC army operations continued through 2012 aimed at holding territory.[3] Maps of armed groups show that the FDLR no longer exerted a stable territorial control in the villages of the Chiefdom thereafter (Vogel, 2021).[4]

[3]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.

[4]See Vogel (2021) 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.

Immediate Nationwide Response to Kimia II. Immediately upon the launch of the offensive against the FDLR, and directly in response to the government's decision to go after the FDLR, observers of the eastern DRC conflict have documented that the FDLR leadership ordered its units across DRC to use violence against civilians, presumably to exert pressure on the central government. Human Rights Watch (2017) indicates, for instance, that:

> "Many of the killings have been carried out by the FDLR militia who are deliberately targeting civilians to punish them for their government's decision to launch military operations against the group."

This immediate retaliatory response was nationwide, affecting villages outside of our sample, predominantly in North Kivu, far away from the chiefdom of Basile.[5] The United Nations Group of Experts (UNSC, 2009) documented that the FDLR leadership issued military instructions for deliberate attacks on civilians. Between February and October 2009, UNSC (2009) documented 1,199 FDLR human rights violations across eastern DRC, most of which were concentrated in the province of North Kivu, and northern South Kivu. All of the cases reported in UNSC (2009) are in North Kivu, Bunyakiri, or Kalehe.

Based on the anecdotal evidence, it is thus impossible to tell if Kimia II may have caused a nationwide rise in FDLR violence in retaliation, if FDLR violence may have risen after Kimia II regardless, or if it may be explained by other mechanisms. However, a peculiar feature of this setting enables isolating the effect of Kimia II on violence through the ability to tax. First, conditional on the common affront to FDLR nationwide through the government's decision to dismantle them, Kimia II affected the ability to tax of different factions of the FDLR differently. Notably, those in Basile that were directly targeted, and that maintained semi-autonomous command structures already prior to Kimia II (Florquin and Debelle, 2015), lost their ability to tax citizens. Other did not. Second, the immediate nationwide retaliatory response ended with the Kimia II operation in 2009. This allows to isolate the effects of making it impossible for *some* units to tax, *above and beyond 2009.*

Long Term Local Effect of Kimia II. Prior to Kimia II, the units that were directly targeted by Kimia II had acquired an autonomous financing system based on providing protection and collecting a sophisticated system of taxes (Florquin and Debelle, 2015). Starting around

[5] For instance UNSC (2009) showed: "The beginning of Umoja Wetu operations in January 2009 led to an upsurge in reprisals against the civilian population in North Kivu. Many of these reprisals were targeted at those villages perceived to have supported RDF [Rwandan Defense Forces] or FARDC in their attacks on FDLR bases."

2005, these units began to settle in the Chiefdom of Basile, an area of 3,113 km^2 in South Kivu. By 2009, they controlled the Chiefdom and provided protection (henceforth, "FDLR state"). They expropriated taxing markets, collected transit toll fees, poll taxes, and mining taxes. Florquin and Debelle (2015), using internal FDLR documents, show that the revenues from taxes were self managed by each FDLR's battalion. After Kimia II, they settled in the neighboring Itombwe forest. In the following years, based in the forest, they acquired resources through violent operations, attacking precisely the villages that they formerly protected (Sawyer and Van Woudenberg, 2009, Levine, 2014):

> "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" (Source: ICC (2012))

Kimia II preserved the autonomy of the factions. UNSC (2010) found that through Kimia II, the FDLR's structure of command over some of their factions remained weak:

> "[Kimia II operations] have disrupted FDLR communications and led to the dispersal of units and the armed group's development of a more complicated command and control system. . . . have helped to exacerbate pre-existing rivalries . . . The Group is investigating this matter, and also whether a new organization or organizations could be evolving from FDLR structures."

From Anecdotal Evidence to Village-level analysis. There are extensive anecdotal examples of the immediate violent response by the FDLR, but there has been little in the way of systematic quantitative analysis to isolate the effects of making it impossible for factions to tax. Complicating this analysis is the fact that the local response of the targeted factions was preceded by a nationwide strategic response by the FDLR as a whole. In what follows, we attempt a more rigorous, quantitative approach. We take advantage of the dramatic historical accident created by Kimia II's geographical targeting for exploring the causal mechanisms of removing armed factions who tax and provide stability. We construct a household-level retrospective dataset to estimate the effect of the targeted factions' ability to tax on the type of violence that they used, who they targeted, and why they did so. We compare that to other areas of DRC and to other armed groups, where the ability to tax is unaffected by Kimia II. We first show that the completion of Kimia II caused FDLR attacks to rise in the FDLR state villages. Then, we propose an explanation for this effect.

3. CONSTRUCTING A DATASET ON ARMED GROUP VIOLENCE IN CONFLICT

We have developed a comprehensive database of village-level armed group violent operations in South and North Kivu, the two most conflict-affected provinces of the DRC.[6] Our team had hundreds of conversations with former and current armed group members. They visited hundreds of villages in South and North Kivu to reconstruct their history and conducted qualitative interviews, household surveys, and cross-validated sources.[7]

The core sample comprises interviews conducted in 1,537 randomly selected households, in 133 villages of South Kivu and 136 villages of North Kivu.[8] The sample comprises data from 144 households and 36 village experts in the Basile Chiefdom, sampled from 18 villages, constituting 324 village-year observations in Basile. The remaining 1,393 households are from outside Basile. To address challenges arising from random sampling, we replicate the entire analysis using data from ACLED (2020) aggregated to the chiefdom.

The data include a detailed description of the violent operations on the villages since 1995 (henceforth, attacks). This includes the following information, for each of the attacks: a. armed group affiliation of the individuals perpetrating the attack; b. purported motivation for the attack, as perceived by the villagers; c. actions the perpetrator took during the operation. The classification into purported motives for the attack was not imposed to the respondents (i.e., it was not prompted) but the researchers had a list with multiple options, including "other" followed by open text, after having an in-depth conversation with the respondent. The predominant responses given are pillaging motives (i.e., operations led by economic incentives aimed to expropriate assets as fast as possible, and which often include forced labor such as sexual violence and abductions for transporting stolen asset), punishment (i.e., operations led by the intention to harm the villagers in response to an alleged offence), or conquest operations (i.e., operations aimed at regaining territory). In various cases, there were multiple purported motives. For those cases, we allow the multiplicity of

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[6] 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 E.

[7] The historical village data for this paper was first presented in Sánchez de la Sierra (2014, 2020).

[8] In South Kivu data were collected between June 2012 and September 2013. Before, the research team spent weeks in the districts' (Chiefdoms) capitals and in the lower-level districts (Groupements) to draw lists of all villages by consulting state and customary authorities. In the lists, we identified villages with a natural resource and a matched sample—the rest typically had less armed group activity. Then, we randomly sampled 133 villages. In North Kivu, the same procedure was implemented in 2015.

motives in the analysis. The actions taken during the attack include theft, abduction, sexual violence, killings, as well as estimates of the amount stolen per village.

Our study's core contribution hinges heavily on the reliability of our measure of violent operations and, specifically, the actions during those operations as well as the motives. We thus devote particular emphasis to document, assure, and verify, the reliability of the violence data in this section in light of the existing literature on measuring violent events.

3.1. *Challenges in Collecting Conflict Data*

There is an extensive literature, developed by Bayesian statisticians, demographers, and some political scientists, on the challenges of counting conflict-related events. For example, Johnson et al. (2008), Marker (2008), Ball and Price (2019), Hoover Green and Ball (2019), Price and Ball (2015), Ball et al. (2003), Silva and Ball (2007), Hagan et al. (2015), Verwimp (2003), Brück et al. (2010), Asher (2013), Spiegel and Salama (2000), summarized in Seybolt and Fischhoff (2013).[9] This literature has elicited a range of errors—from missingness, event size bias, survivor bias, acquiescence bias, reporting bias, recall bias, event visibility. Those have been shown to affect event estimates in conflict settings. The data collection that we describe in what follows was designed to address, to the best of our ability, errors arising from recall error, missingness, survivor bias, and reporting bias.

First, the simplicity of the measure we aimed to gather provides reassurance against some of the most severe concerns raised in this literature. The literature has focused on the difficulty of counting violent events in conflict settings, that affect individuals. The particularly difficult tasks include counting the number of fatalities, as well as obtaining reliable information from a victim of the type of violence that she experienced, and by whom. In this paper, the analysis hinges on a much simpler outcome, for which the sources of error are likely to be lesser than determining individual victimization.

In particular, the key variable of interest is determining whether, in a given year, in a given village, there was at least one violent operation. Violent operations typically inflict harm on multiple individuals, and would thus be counted as multiple violent events in some contexts. In our case, using the methods that we describe in what follows, we are only interested in the aggregation of information from various sources allowing us to conclude

[9] See, in general, the debates around the body count in Iraq (Asher, 2013).

whether at least one operation occurred in a given year. This provides for a harder test, but also one that is less suitable to the sources of errors emphasized in the literature.

Second, our data collection took place exactly at the end of the three year period FDLR's violent response in Basile. This has the advantage that the entire effect we measure in this paper took place in the three years preceding the interview. This makes it much more difficult that respondents have forgotten events or details about those.

In what follows, we describe in detail the study procedures that we developed to enable this type of triangulation, and present the outcome of the triangulation exercise.

3.2. *Study Procedures for Reconstructing Violence History From Multiple Sources*

Village-level Activities

In each village, a team of two male researchers lived and worked for one week, reconstructing the history of the village since 1995. In the first day, they built ties with the most knowledgeable persons they identified in the village (henceforth, history specialists). For reasonable compensation, the history specialists were trained in the first day for the reconstruction of key variables in the village history. The researchers provided specific forms that were specifically designed to that effect and instructed them on how to complete them. Then, the history specialists reconstructed, every day of the week, the history of the village for these variables. At the end of each day, the researchers met the history specialists, observed the preliminary output and provided feedback. Given that the knowledge of the researchers grew every day in the village—as they conducted additional tasks that we describe in what follows—they were able to detect issues in the data collection, omissions from the history specialists, and correct them over time. In the last day of the week, they held a day-long meeting with the history specialists, where they reviewed, item by item, the history of the village, and contrasted that history with an array of sources the surveyors collected in the village during each day.

Household Survey

In each randomly selected household, the researcher randomly matched to that household conducted a day-long conversation with one randomly selected adult respondent. Respondents were all male by design because our survey also included conversations related to their participation in armed groups, a predominantly male activity in this context.[10]

The conversation between the researcher and the respondent 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 of all the measurements we were able to collect in the household survey.

In a household attack history module, each respondent was 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 against the household; the 99^{th} percentile is seven events. Thus, reporting limit did not lead to loss of data. For each event, we observe the perpetrators' group, the perceived intention, 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. The survey does not include follow up questions about "easy to forget" members of the household since we are only interested on whether the household was ever attacked, independently of the identity of the victims.

In addition, for each household member, the respondent reported (1) up to three events in which armed actors victimized the household member, and (2) of these, up to three events in which the armed actor perpetrated sexual violence on the member. For each of those events, we identify the year in which they took place.

Overall, 874 village-level attack operations were recorded in our sample of 239 villages between 1995 and 2013, associated to 2,513 victimized households. Of all village-level operations, 273 are perpetrated by the FDLR, corresponding to 1,187 victimized households. This implies that four households are victimized per FDLR attack on average. Most of these are pillaging operations. In contrast, there are 48 Congolese army village-level violent operations in the sample, corresponding to one household victimized on average, most of which are conquest operations. All other actors, which include all nonstate armed

[10]It was also informed by our experience in neighboring districts (Tanganyika) where it was discovered that male researchers interviewing female respondents was inappropriate and caused conflict.

groups, are accountable for 553 of all remaining attacks and 1,256 of all remaining victimized households. Most of the FDLR attacks in the sample are pillaging operations, and their attacks target on average a large number of households compared to attacks by other actors. This is consistent with anecdotal evidence in the region about the FDLR presented in Section 2, painting a picture of FDLR attacks as being predominantly motivated by expropriation of household assets. Figure F.1 in the Online Appendix presents these counts.

Qualitative Reports

Complementing this procedure, the researchers conducted qualitative interviews aimed at gaining an understanding of the history of violence in the village and its relationship with armed groups. In each visited village, researchers prepared a five page qualitative report about the village history, based on conversations with selected individuals. Specifically, they identified additional individuals who were knowledgeable about the militia that operated in the area, often village authorities, militia leaders, and former militia members. In what follows, we refer to this body of information as our *qualitative data*.

The qualitative data were used for two reasons. First, they exposed the researchers to different ways of accessing information about the village history, which enriched their knowledge of the village history and enabled them to better correct the history specialists when needed. Second, we used the qualitative reports to substantiate the mechanisms proposed in this paper. We provide some excerpts from the qualitative interviews in the the paper.

The strength of the qualitative data is curtailed by the potential filters, biases, and perspectives of the interviewed individuals and by the conscious or unconscious attempts by the entire research team to look for confirming evidence in qualitative interviews, and discarding of disconfirming evidence. We put in place two strategies to prevent this error. First, the qualitative reports were designed in conjunction with an academic ethnographer, who used the reports for his dissertation (Marchais, 2015). The ethnographer verified the validity of the design, and personally trained each of the field researchers to ethnographic data collection for his dissertation. Second, at the time of the data collection, neither the authors nor anyone in the research team intended to study the effect of Kimia II. The impact of Kimia II in the visited communities was discovered in passing as part of the logistics of the project, and only once Basile data collection was already complete. As a result, it is impossible that anyone in the team has any bias related to the hypothesis.[11]

Section C in the online Appendix discusses how we addressed ethical challenges in our data collection, including the problems with gender, researchers' security, as well as conducting interviews on experience with traumatic events.

3.3. *Recall Error*

Reconstructing a household's history based on recall is subject to classic measurement error. To address this challenge, we used established methods in recall studies from eyewitnesses and, based on three months of piloting, tailored them to the cultural context.

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, we built in the surveys 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 priori about the year.[12]

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

[11] The hypotheses the study was originally designed to test are in Sánchez de la Sierra (2020).

[12] For instance, respondents easily answered whether they had acquired a cow before or after their marriage.

3.4. *Survivor Bias, Missingness, and Reporting Bias*

Respondents may omit attacks their village or household experienced due to social desirability pressure such as shame, or simply due to memory loss or mistakes in years.

Survivor Bias and Missingness. Random samples of violence reports miss events in part due to the fact that persons who were killed cannot be interviewed. This source of error could affect the frequency of events reported, but also the types of events respondents report. It is thus natural to expect that the count of events is likely to be artificially low, and that pillage, a potentially less lethal type of violence, may be over-represented. The central output of our data construction are not individual-level indicators for whether a given person was victim of violence. Instead, we use this procedure to reconstruct information for whether a given *village* experienced a violent operation perpetrated by an armed actor. Of course, a victim who died in an attack is no longer there to recount that attack, and our strategy is doomed to under-estimate the count of attacks in the region. Yet, since we ask respondents to report on attacks that took place in the village, our strategy can recover the attacks that take place in a village independently of survivor bias that may affect the set of attacks that respondents have experienced. While there are grounds for survivor bias on directly targeted individuals, the village as a whole rarely disappears.

Reporting Bias. Individual respondents, especially for attacks that affected them directly, may have an incentive to mis-report some of the attack information, especially as it pertains to their individual experience with it. Since we are interested in events that occurred in the village predominantly, and since we interview multiple sources, we can ensure that individual-level motivations for mis-reporting that may arise due to the targeting of an attack can be circumvented through cross validation. Of particular importance for our estimation, all of the villages in the sample of Basile had been liberated by the Congolese army, reducing concerns that they would mis-report violent operations by the FDLR. However, at the time of the data collection, the Congolese army was also not in these villages, even if they exerted influence and were able to deter operations from their nearby bases. This helps also to reassure that respondents did not mis-report violent Congolese army violence.

Addressing Survivor Bias, Missingness, and Reporting Bias. Anticipating these concerns, we used the following strategies to triangulate the village violence history. First, we reconstructed the history of village-level violent operations that took place in each village in each of 8 household interviews, obtaining 8 different sources for the same common events. Second, we also reconstructed the history of village-level violent operations during the village-level data collection process. In the field, the surveyors used the household reports and the qualitative reports to confront and correct the village-level data collection process. But this also allows us to verify whether reports of village victimization by one household in a given year and by one perpetrator is corroborated by at least one other household, by the village-level source, by both, by either of both, and ACLED.

Figure F.2 presents the outcome of this exercise for 2005–2012.[13] There are 990 household-year observations in which a household reports an attack taking place in the village. In 840 of those, at least one other household also reported an attack in the village, in the neighborhood of the year of the reported attack (in year t, $t-1$ or $t+1$ where t is the year in the corresponding household report). In 878 of these 990, either another household or the village source reported an attack, in the neighborhood of the year of the reported attack. This contrasts with only 122 for which an attack within 50km of the village was recorded in ACLED.

A limitation with this analysis is that it uses the GPS information from ACLED to verify our data with that of ACLED. Yet, the source in ACLED lacks GPS, and thus the GPS information in ACLED is often imputed. In order to verify the quality of ACLED's actual information to benchmark it to our data, we inspected, one by one, each of the 68 2005–2012 events in ACLED who satisfy one of the following two conditions: a. the assigned GPS coordinates fall within the boundaries of the chiefdom of Basile; b. the administrative information indicates they are located in the district to which Basile belongs, Mwenga. Analyzing the notes in each event, we identified 28 events that were wrongly associated to Basile. Interestingly, all of those are reports of conflict between the Congolese army and the M23—which never existed in the anywhere province of South Kivu.[14] We also found that 18 events were wrongly categorized in the neighboring chiefdoms in Mwenga. Almost all events in the district of Mwenga were assigned to the town of Mwenga center

[13]This number is larger than the total number of attacks reported since it is at the household report level and there are multiple reports of directly victimized household per attack.

[14]Figure F.4 shows that M23 never were anywhere near the chiefdom in Basile. Inspecting of the details reported in each ACLED event reveals that most of them took place in the town of Kibumba, North Kivu province. The error in ACLED arises from the fact that the chiefdom of Basile also has a town called Kibumba.

with no information on the chiefdom in which the event took place. This town is administratively located in Basile, but the GPS coordinates in ACLED falls in the chiefdom of Wamuzimu.[15] Figure F.3 in the online appendix compares attacks recorded in our sample of villages in Basile to those in ACLED that really took place in Basile. Even as our sample of 324 village-year observations constructed through the reports from 144 households and 36 history specialists in 18 villages of Basile is just a subset of Basile, the set of attacks recovered through our procedure is three times larger than that in ACLED for *all* of Basile.

3.5. *Bias Arising From Researcher Interaction*

The number of reported violations has been shown in other studies to correlate with attributes of the interviewer. Notably, female interviewers have been shown to obtain responses with higher number of conflict violations (Asher, 2013). Our decision to exclude women from research roles can thus artificially lower the number of events reported. Similarly, the linguistic and ethnic match between respondents and researchers in Basile might a priori also have shaped responses. First, although researchers lived in the city, several of them were originally from the district to which they were assigned. This ensured that they spoke the local language and were able to build trust. Our qualitative data, the village qualitative reports, and cross-verification suggest that this curtailed motives for concealing participation. Second, within culturally feasible matches, researchers were randomly assigned to villages. Within each village, researchers were then randomly assigned to households. This allows us to include researcher fixed effects when analyzing within-village variation. All the results that we report in what follows are unaffected by researcher fixed effects.

3.6. *Additional Data Collected*

The data also include the following yearly information for each armed actor: whether they controlled the village, the taxes they raised, and whether they held a fiscal or a judiciary administration. Armed groups in eastern DRC always raise 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

[15] We were also able to locate three events into a different chiefdom of the district, Lwindi, and 5 events do not have any information in the written details to locate them (they only indicate Mwenga district).

individual enters or exits the village), mill taxes (taxes for using the local mill), as well as taxes on mining activities. Section B.3 describes the details of all other variables collected. Section C.1 provides a summary of the main non-conflict related variables collected. We combine these with geographical data from RGC (2010).

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.[16] Since we reconstructed detailed inventories of goods and assets stolen in each village during each reported attack that took place in the village, as well as yearly asset prices, we could reconstruct the yearly assets lost to theft per household.[17]

4. DESCRIPTIVE STATISTICS OF THE "FDLR STATE" IN BASILE, IN HISTORICAL CONTEXT

Before analyzing the effect of Kimia II completion on the incentives of the FDLR factions in Basile, we present basic descriptive statistics of our data. First, we present the times series of different types of violent operations by the FDLR and by other actors in our data before, during, and after, Kimia II . Second, we describe the activities of the FDLR factions in the FDLR state prior to Kimia II. Third, we show the successful completion of Kimia II.

4.1. *Violent Operations Across Eastern DRC, in Historical Context*

We first describe the patterns of violence in our data by the FDLR factions and by other actors, irrespective of where they are located. Figure I shows the fraction of villages in the sample in which an attack operation takes place, by type, based on the purported motive. Panel A shows it for attacks perpetrated by other armed actors than the FDLR. Panel B shows it for FDLR-perpetrated attacks.

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[16]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.

[17]The researchers, with the help of the village history specialists, established detailed lists of stolen assets in each attack. Separately, they also reconstructed the history of prices of basic assets each year.

Violent operations are high during the Congo Wars (1996–2004). Those attacks include pillages, punishment operations, and conquest operations, and are perpetrated by all actors. In the period of the "FDLR state" (2004–2009), violence by the FDLR is low on average in our sample. As we show later, this low level of violence is driven by Basile. Notably, in 2008, there is a spike in non-FDLR related attacks: this is the one year CNDP rebellion, which preceded, and triggered the creation of Kimia II as part of the deal between Rwanda and DRC. Kimia II is marked with a red vertical line. After Kimia II is complete, FDLR violence skyrockets. The spike is driven by pillages. As we show in the section that follows, it is entirely driven by Basile. The spike in 2012, unrelated to the FDLR, reflects the formation and expansion of a new militia, the Raia Mutomboki in other areas.

Having shown that the period 2004–2009 had remarkable and unusal low levels of violence by the FDLR, we now investigate the types of actions that the FDLR was doing in the FDLR state and in other villages in what follows.

4.2. *Descriptive Statistics of the "FDLR State" in Basile*

We now analyze the FDLR activities, and notably their expropriation strategy in the villages of Basile prior to Kimia II. Table I presents the village mean characteristics, FDLR expropriation strategy, and security outcomes for FDLR state villages (*FDLR State*) and the rest (*Rest*), the two years leading up to Kimia II.

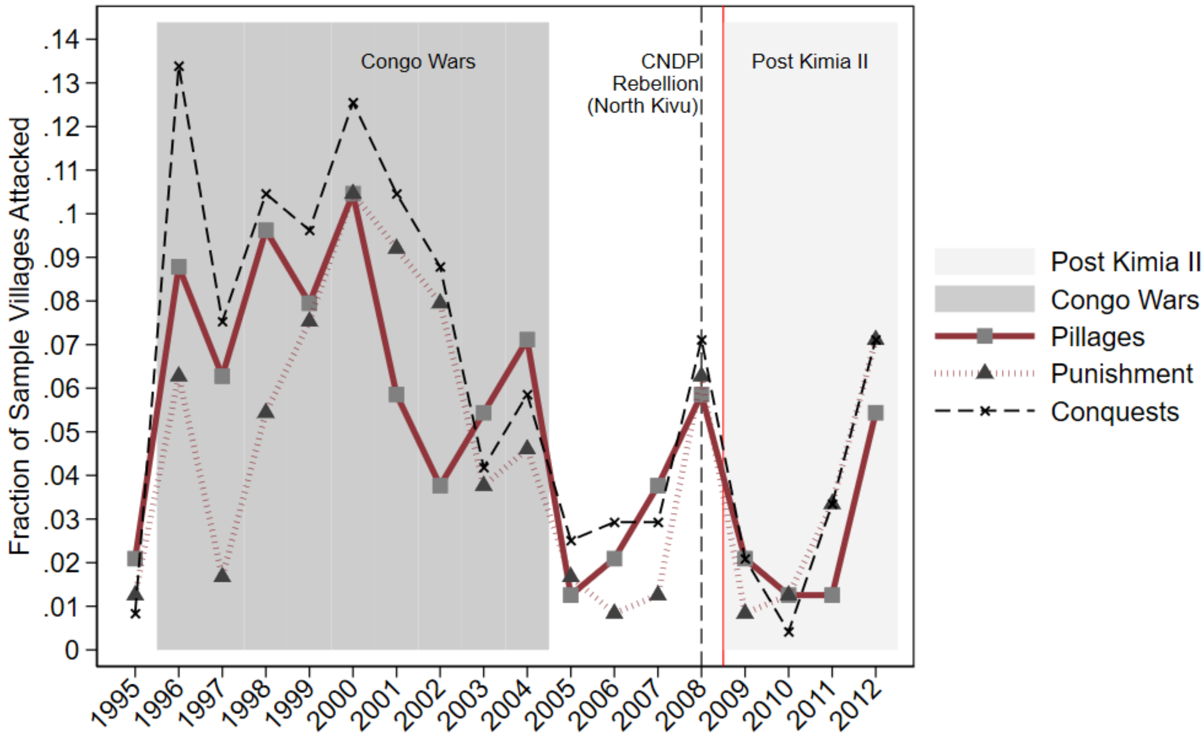
Panel A shows that none of the villages in the FDLR state were accessible by car, and 78% were only accessible by foot. They were 29 pp. less likely to have phone coverage. The closest river and road were 1.35 km and 1.29 km farther away than the rest, respectively. They had lower levels of migration. The FDLR conducted at least one expropriation (pillaging or tax payment) in 348 days per year on average, nearly zero in the rest. Given the relative remoteness of the FDLR State villages compared to the rest, we account for those differences in the analysis that follows.[18]

The relative remoteness of the FDLR state villages could have played a role in enabling them to settle since 2005, and tax. This interpretation is supported by our qualitative data. For instance, the qualitative report of the village of Musingi, in the Chiefdom of Basile, indicates:

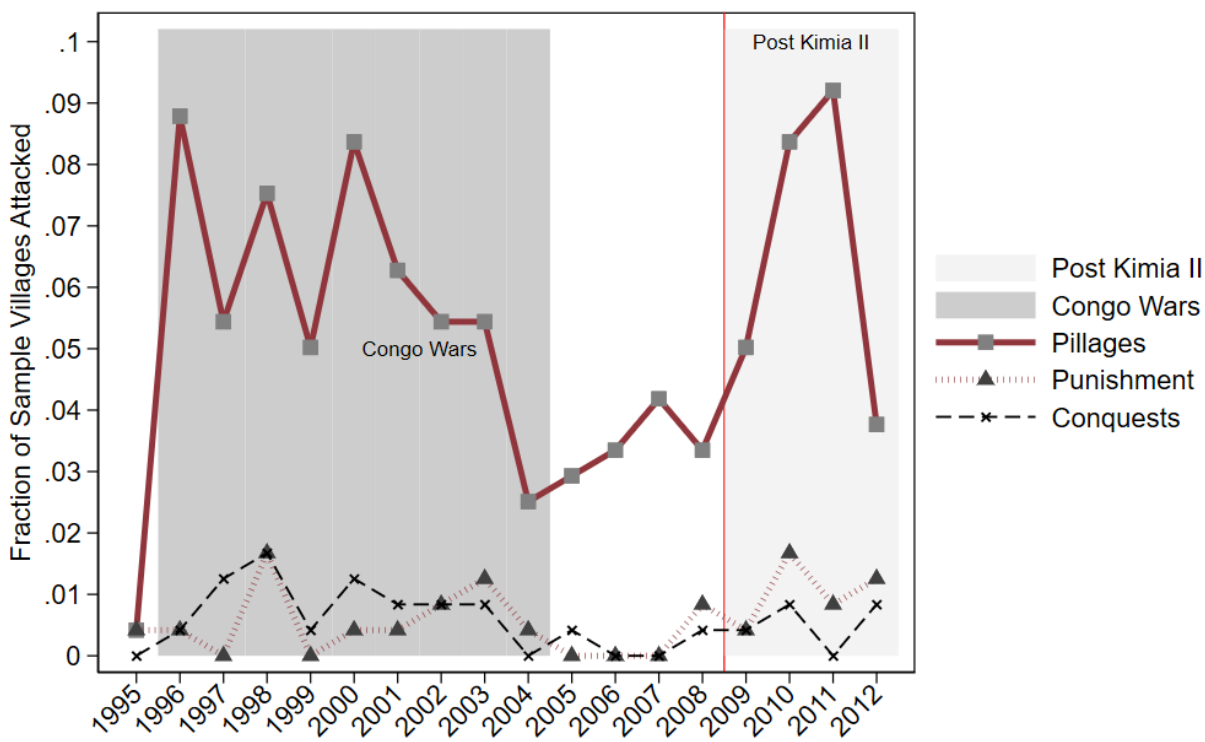
[18]See, for instance, Table II.

Figure I.: Times-Series of Violent Operations Across eastern DRC, in Historical Context

A. Non-FDLR Attacks, by Type (Full Sample of Villages)



B. FDLR Attacks, by Type (Full Sample of Villages)



Notes: This figure presents the fraction of villages in the sample in which a violent operation is recorded, over the period for which we collected data. Panel A includes only attack operations perpetrated by other actors than FDLR, including all other armed groups and the Congolese army. Panel B does so only for attack operations perpetrated by the FDLR. Red, vertical lines indicate the timing of Kimia II. The black, dashed vertical line indicates the timing of the CNDP rebellion, which took place in North Kivu. The CNDP Rebellion ended in late 2008, with a deal that led to the Kimia II operation in South Kivu.

> "These FDLR came to settle in the village because it is a village very remote in the forest and far from the city and also to impose their authority, they became absolute masters in this village"

Indeed, they governed, and taxed. Panel B shows that, prior to Kimia II, the FDLR units ran justice and fiscal administrations in these villages. They collected poll taxes, toll fees for transit of persons in and out of the village, taxes on the local market in 94%, 83%, 28% of the FDLR state villages, respectively (col. *FDLR State*). They collected none of those taxes outside the FDLR state in the villages in the sample (col. *Rest*). Each household paid, on average, 64.97 USD yearly to the FDLR in taxes. Using World Bank (2021), this implies that each household paid 25.5% of national yearly p.c. income in taxes to the FDLR. Panel C shows that the frequency of violent operations against the village (henceforth, attacks) by any armed actor (6%) was half of that in the rest, suggesting the FDLR provided security.

The qualitative reports from the villages in the Chiefdom of Basile, the FDLR state, provides rich background to better interpret the type of stability that they created, and why they did so. For instance, the report of the village of Pohe, Mito and Wimbi, indicates:

> "After the departure of the Mayi Mayi militia [in 2005], the FDLR multiplied their attacks on the population of villages in the area. Then, the chiefs of these villages got together, and decided to go find the chiefs of the FDLR to negotiate with them so that they come to provide security in the villages of the groupement of Bawanda. The latter accepted to send their fighters (5 to 6 in each village) and the staff rotated each week."

The report of these villages also indicates how this stability was financed through taxes:

> "They agreed on the villages' contributions. The FDLR lived in the villages from 2005 to 2010, when [Kimia II] removed them. According to the inhabitants of the villages secured by the FDLR (Kyunga, Pohe, Mito, Kalambo, and Wimbi), in this period, there were no attacks in the villages, so to say the FDLR really provided security . . . The FDLR asked rations [poll tax] (also for their survival) in the households in collaboration with the village chiefs and the populations. They also inherited the old roadbloacks initially erected by the Mayi-Mayi militia that preceded their arrival."

The reports of all the villages of Basile provide rich background to the type of taxes that citizens paid, and of those taxes being very high for the population. For instance, the report of the village of Kakanga contains the specific levels of complex tax systems:

> "[The FDLR, in the period 2005–2009] asked to pay 30 USD each mine each week. In this village, the cost of the tax was really high. For example, for the FDLR, the household tax was 2000 FC, the transit tax was 1 USD per week, the market tax 1000 FC per seller or 1000 FC per merchandise sold "

The main public good provided was security, at a high cost for the populations of Basile. However, in some cases, we also recorded anecdotally other types of services provided by the FDLR. For instance, the report of the villages Pohe, Mito, Wimbi indicates:

> “At the time of the FDLR, when they provided security in these villages, they went to pillage villages far away from here, and brought everything they pillaged from those villages into the villages they protected (cows, goats, clothes, and other valuable goods).”

4.3. *Success of Kimia II in Removing Targeted Armed Actors*

Kimia II swiftly interrupted the FDLR factions ability to tax in the Basile villages. Figure II shows the location of the the FDLR and the Congolese army in 2008 and after Kimia II. While the FDLR state villages in our sample were all under the control of the FDLR in 2008, all but one are “liberated” by the Congolese army by 2011. The map only reports whether an armed group controls a given village, but does not track the militia into the forest. Anecdotal evidence indicates that, for the factions of Basile, they remained active locally but were simply moved to the neighboring forests.

Kimia II was an attempt to assert the state’s exclusive right to tax. But, like in many other weak states, the state does not have monopoly of violence: crime is a recurrent feature of states, and especially of weak states. The qualitative reports of Tubindi and of Mito indicate, respectively, why the army struggled to deter FDLR crime:

> “The Congolese army controlled this village in 2011–2013. It is important to note that the Congolese army was not always permanent in this village, which enabled the frequency of multiple pillages by the FDLR.”

> “In 2010 to 2013, the Congolese army provided security but were not permanent [understaffed], which favored the attacks by the FDLR any day”

5. ANALYSIS

We now examine the relationship between Kimia II’s completion and FDLR attacks.

5.1. *Times-Series*

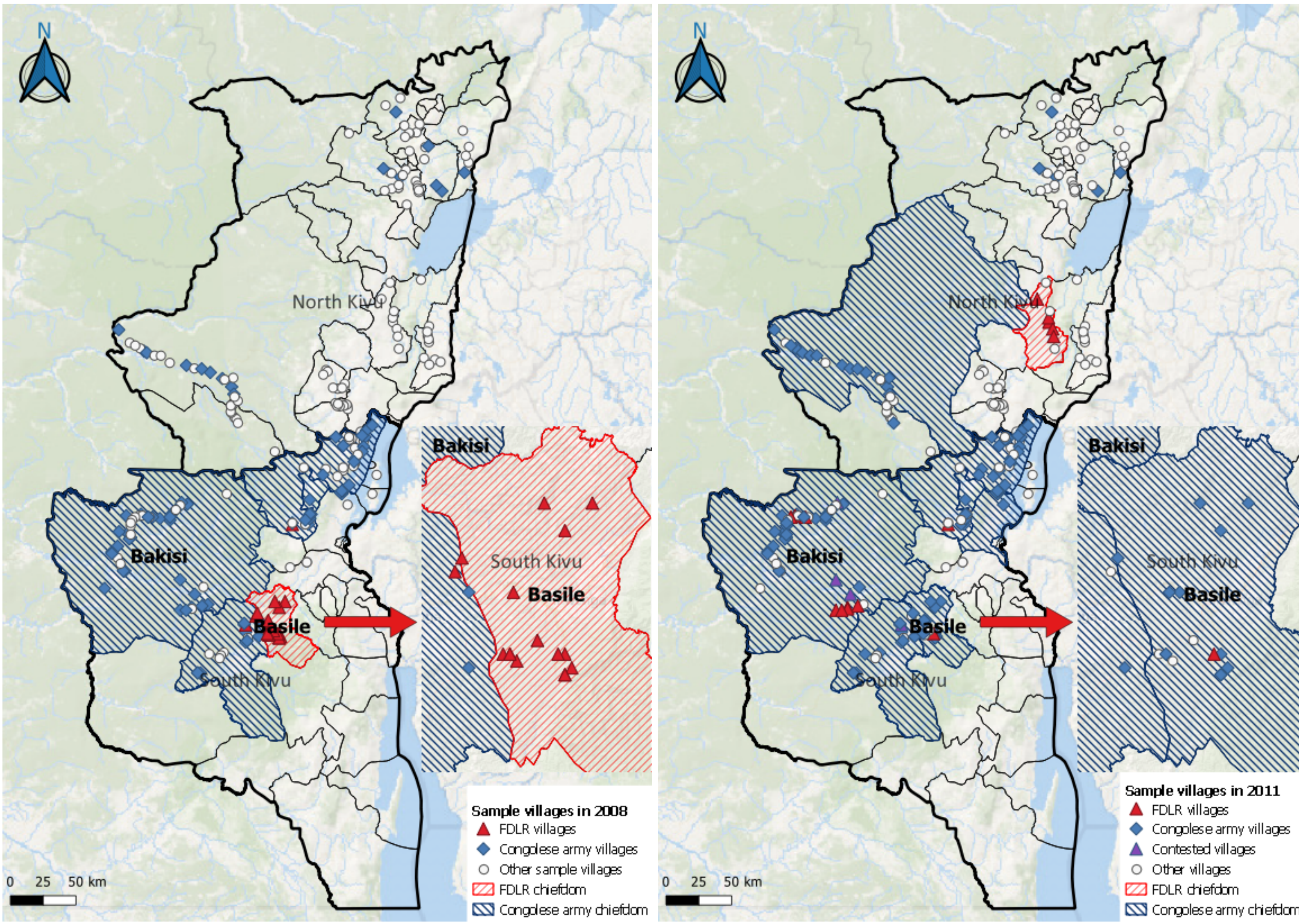
We first visually examine the relationship between the completion of Kimia II and the attacks perpetrated by the FDLR in the cleared villages. Figure III shows the trends in territorial control by the Congolese army and the FDLR, as well as the attacks perpetrated

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 Expropriation Strategy						
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)	6.18	64.97	(53.85)	0.13	(1.74)	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. The data is aggregated at the level of village-year observations. We use two years of pre-Kimia II to construct this table, hence there are 36 observations in FDLR State, reflecting two years for the averages of 18 villages, and 350 in the rest, reflecting 175 villages for two years.

Figure II.: The FDLR State and the Rest, Before and After Kimia II



Notes: This figure shows villages controlled by the FDLR in the sample, covering the provinces of North Kivu and South Kivu. The left quadrant does so for 2008, which is the year before Kimia II. The right quadrant do so for 2011, which is a year after Kimia II was complete. The red triangles are the villages where FDLR had control, blue squares are those where the Congolese army had control. Red striped areas indicate the Chiefdoms where the FDLR holds more than 50% of villages in the sample. Blue striped areas indicate Chiefdoms in which more than 50% of the villages in the sample were held by the Congolese army.

by the Congolese army (Panel A) and the trends in FDLR violent operations dis-aggregated by type over the study period. The figure clarifies three aspects of our analysis.

First, Panel A confirms that Kimia II was successful in *removing* the FDLR from the Basile state villages: after 2009, the Congolese army gained control over most FDLR state villages. Simultaneously, while the FDLR controlled 100% of the FDLR state villages in 2008, they only did so in 10% of those villages by 2011.

Second, Panel A also shows that this transfer of territory in the FDLR state was not accompanied by attacks by the Congolese army.

While violence by the Congolese army has been documented across the DRC territory, the seamless removal of the FDLR factions in the FDLR state could be afforded precisely because the allocation of power was extremely imbalanced. As we documented in Section 2, the campaign was staffed by 30,000 soldiers, while the FDLR as a whole, across all of eastern DRC had at most 6,000 fighters. The factions in the FDLR state were even smaller in numbers, hence the FDLR mostly retreated in Basile. This, and the short-lived nature of the Kimia II campaign, provides reassuring evidence that fighting by the Congolese army does not, in our case, confound the analysis of the FDLR violence after Kimia II.

Third, Panel B documents that Kimia II was associated with a subsequent rise in FDLR attacks in the FDLR state. Before Kimia II, attacks by FDLR in the FDLR state villages were decreasing. This is consistent with the progressive creation of incentives to refrain from violence through securing a protected environment where they could freely tax. After Kimia II, the FDLR's attacks skyrocketed in FDLR state villages, and remained constant in the rest. The rise in FDLR attacks is entirely driven by attacks motivated by pillaging.

Yet, as Figure F.2 showed, our data successfully recorded punishment and conquest-driven violent operations during the Congo wars, reassuring that the rise in FDLR economically motivated attacks is not an artifact of measurement. Furthermore, in targeted villages, there was a decline of attacks during 2005–2009, while in non-treated villages there was an increase. This provides reassuring evidence that our empirical strategy does not capture increasing linear trends in the targeted villages. We now examine this relationship formally.[19]

[19]Even though Congolese Army presence in a village was able to prevent the FDLR from taxing, it could not prevent them from pillaging. Figure F.7 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

5.2. *Event Study Estimates: Kimia II completion and FDLR Attacks*

We then examine the relationship between the completion of Kimia II and subsequent FDLR violence by estimating the following Equation with two-way FE (village and year):

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

where α_i, β_t, β_t^{NK} are respectively village, year, and year-province fixed effects.[20] The indicator *FDLR*$_i$ takes value 1 if village i was controlled by the FDLR *in 2008*.[21] The indicator $1(t = 2009 + k)$ takes value 1 if 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. The standard errors are clustered two-ways at the village level (239 clusters) and the Chiefdom-Period level (42 clusters), where Period indicates whether the year is before (or after) Kimia II.

At this point, the choices made in the core estimation strategy deserve some comments. First, the results are unchanged with OLS. This is obvious, since assignment to post Kimia II is not staggered. Second, our estimated standard errors are immune to autocorrelation of violent events in each village over years, and to common within Chiefdom shocks that could coincidentally occur within time periods before or after Kimia II.[22] Third, we include province-year fixed effects in all specifications, which ensures that our results are not driven by diverging conflict dynamics occurring in different provinces. Finally, to shield the non-FDLR state comparison group from contamination arising from possible displacement of the FDLR into other areas, in our main analysis, we exclude the nearby villages of Shabunda (the Chiefdom of Bakisi, in our sample). These were vacated by the Congolese army after Kimia II, leading to a security vacuum that created the potential for interference if the FDLR units were displaced to those areas. We henceforth refer to those as *Spillover Villages* and to the sample as the quasi-experimental sample. In the analysis that follows,

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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.

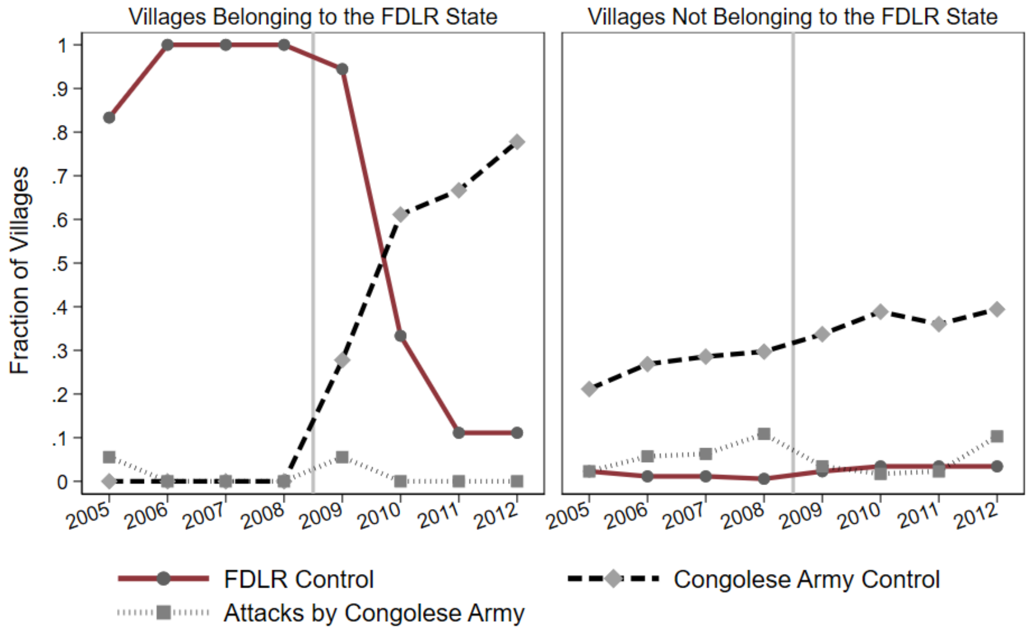
[20] That is, we estimate year effects separately for both provinces for robustness to province specific shocks.

[21] 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.

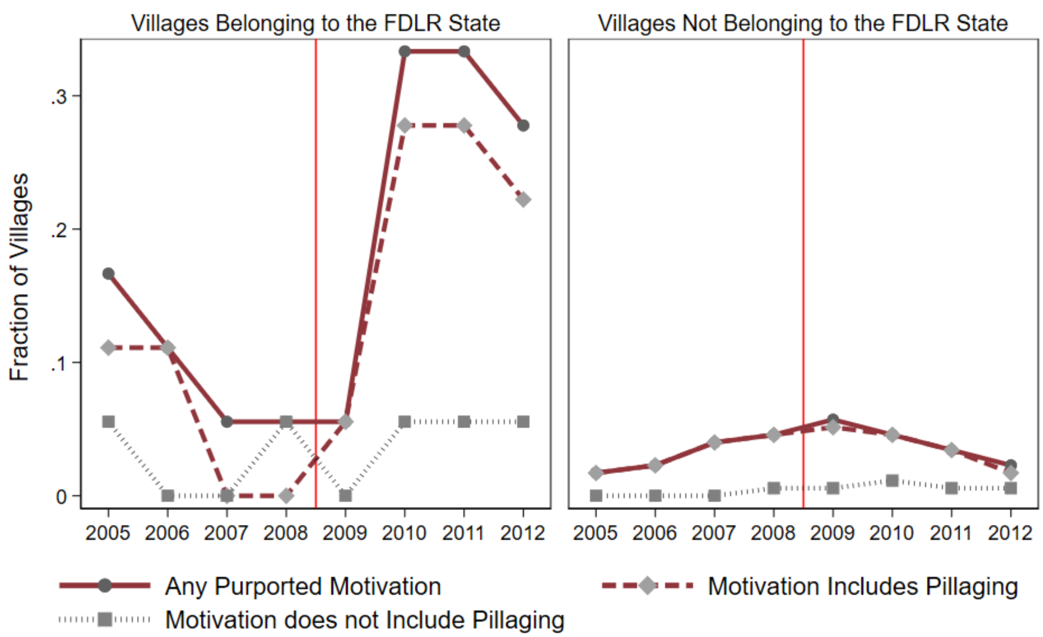
[22] We use Borusyak et al. (2020) user-ready package, and update the program to allow for two-way clustering.

Figure III.: Kimia II Campaign and FDLR Violence, for FDLR State Villages and the Rest

A. Kimia II Campaign: Regaining Territorial Control With the Threat of Violence



B. FDLR Attacks



Notes: Panel A presents the times-series of the fraction of villages for which the following indicators take value 1: territorial control by the FDLR, territorial control by the Congolese army, and whether the Congolese army perpetrates an attack, by whether the village belongs to the FDLR state. Panel B presents the times-series of the fraction of villages for which the following indicators take value 1: whether the FDLR perpetrates a violent operation (independently of the motive), whether the FDLR perpetrate a violent operation purportedly motivated by pillage, and whether they perpetrate a violent operation purportedly motivated by other reasons excluding pillage.

we also show that all results follow through when we include the spillover villages. To test whether Kimia II increased FDLR attacks, we seek whether $\beta_k > 0$ if $k > 0$. Our analysis focuses on the years 2005 through 2012, which contains the time window after the Second Congo War for the creation of the FDLR state, the Kimia II campaign, and all the years after the campaign until the end of our sample. In robustness analysis, we also include a longer pre-period (see Figure F.5).

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 after 2009. The difference is statistically significant at the 1% level for each year after 2009. There is a mild decreasing pre-trend, although it is not statistically significant and goes in the opposite direction to the finding that Kimia II led to more FDLR violence in the FDLR state villages. This suggests that the completion of Kimia II, which successfully made it impossible for the FDLR factions to tax had a large positive association with subsequent attacks by FDLR on the villages they previously controlled. Figure F.5 shows that this effect is preserved with a longer pre-period.[23]

The qualitative reports shed light on the nature of these attacks:

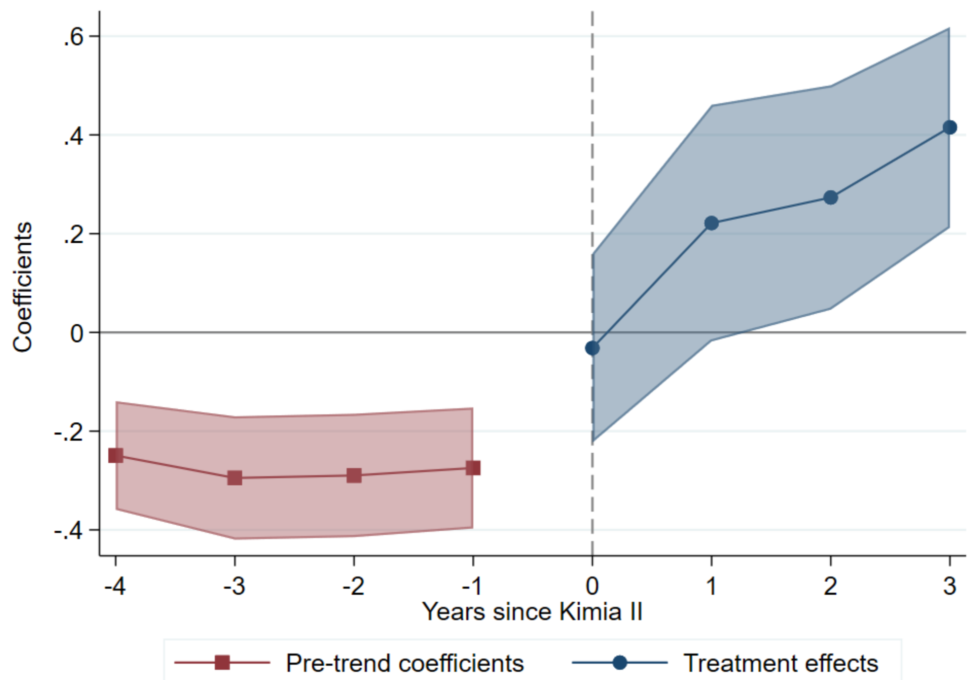
> "In 2011, the FDLR had taken the village chief hostage, requesting 6 goats for his liberation. Unfortunately, they already had killed the chief of the state office nearby in the same procedure. In the same year, the FDLR racketed 20 USD from each sub-village of the village. In 2013, the FDLR came to attack this village, they pillaged, committed sexual violence, and 5 people were taken hostage." **Source**: qualitative report of the village of Tubindi.

This analysis has shown that the completion of Kimia II is associated with a long-term (3 years long) increase in attacks by the FDLR against the FDLR state villages, precisely the villages that the FDLR formerly protected. However, this association could simply reflect a general rise in violence after Kimia II. We analyze these concerns in the next section.

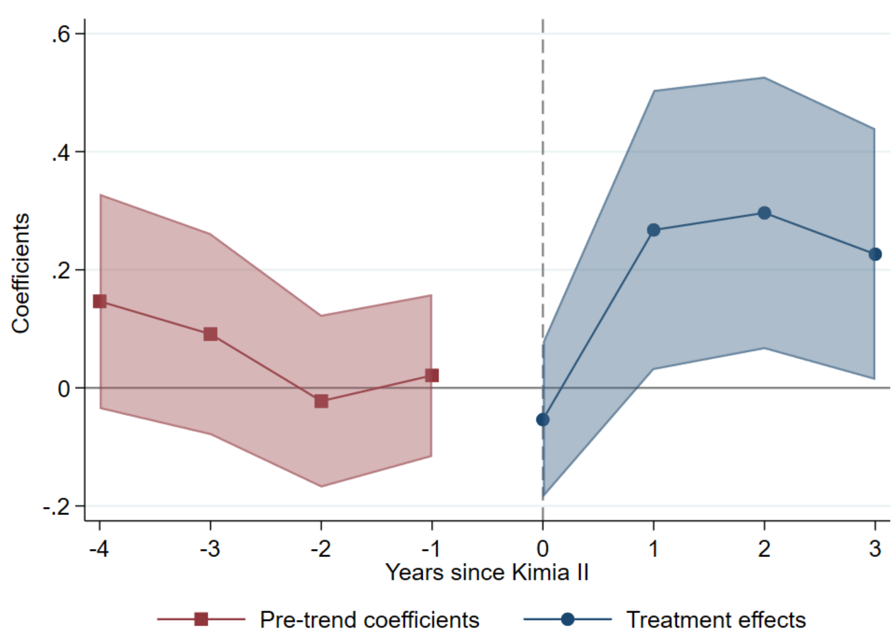
[23] The data that we constructed ends in 2013. To analyze the long-term effect of the policy, we use data from ACLED (2020) spanning through 2020. Although, as we documented in the description of the data, there are three times fewer events in the entire period in ACLED (2020) in the chiefdom of Basile, we can expand the catchment area of our sample villages to include all FDLR events that were reported in up to 50km away from each village. Adding observations may also improve the estimation of the standard errors and fixed effects. Appendix E estimates Equation 1 using data from ACLED (2020). The analysis using ACLED (2020)'s extended times series shows that the increase in violence lasts three years.

Figure IV.: Kimia II and FDLR Violence, FDLR State Villages vs. the Rest (Event Study)

A. Kimia II Campaign and Congolese Army Control



B. Kimia II Campaign Completion and FDLR Attacks



Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020) robust and efficient estimator. The regression includes village and year fixed effects. The year 2009 is the omitted category. 1,544 village-year observations were used in the estimation. Standard errors are clustered two-ways at the Village level and the Chiefdom-Period level. In Panel A, the dependent variable is an indicator for whether the Congolese army controls village i in year t. In Panel B: The dependent variable is an indicator for whether the FDLR attacks village i in year t.

5.3. *Falsification Exercise: Effect of Kimia II on Violence Perpetrated by Other Groups*

One concern with the results from Section 5.2 is that Kimia II completion could have been associated to a change in the dynamics of violence in the region for other reasons than the FDLR's expropriation trade-offs. First, Kimia II completion could have riled up armed group activities in the targeted area, leading to a rise in violence not specific to the targeted factions. Second, Kimia II completion may have destabilized eastern DRC, making all armed groups more violent in each of their respective areas. Third, Kimia II may have had no causal effect at all, but simply coincide with a change in the dynamics of violence.

Given potential concerns about this assumption, we use data on violence by all other armed actors to provide evidence that Kimia II completion 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 completion 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 completion does not predict a rise in attacks in the FDLR state by armed actors who were not targeted by Kimia II. This suggests that Kimia II completion did not directly affect attacks by other actors in the FDLR state and thus that Kimia II completion could not have affected FDLR attacks by first affecting the dynamics of violence in the FDLR state.

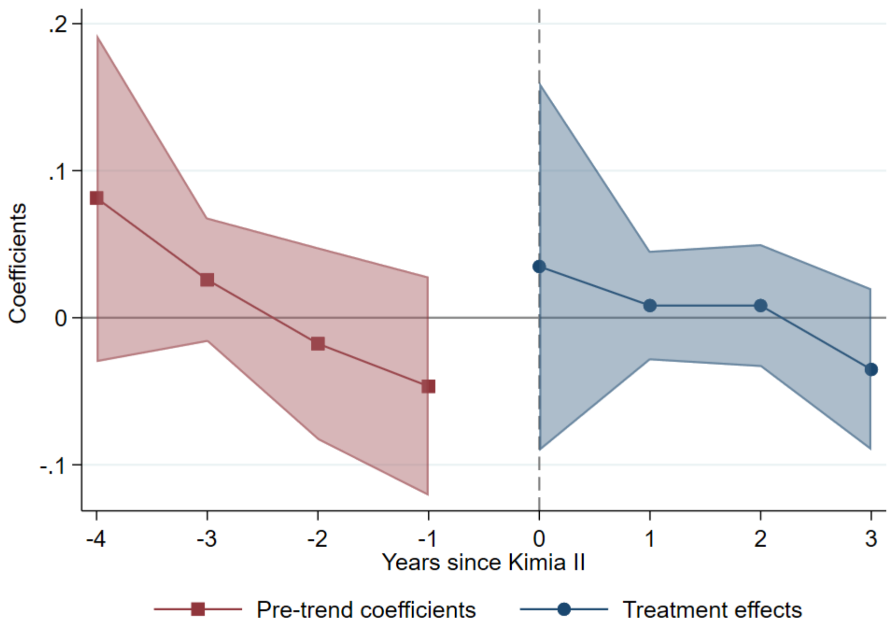
As an additional falsification, we examine whether Kimia II completion led to a rise in attacks by other armed actors on the villages they controlled. If it only affected the incentives of the FDLR to use violence against its former villages, it should have no predictive power for attacks by other armed actors in the villages previously controlled by other armed actors. Panel B of Figure V presents the event study coefficients.[24] Thus, Kimia II completion is *directly* associated with violence by the targeted FDLR factions.[25]

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

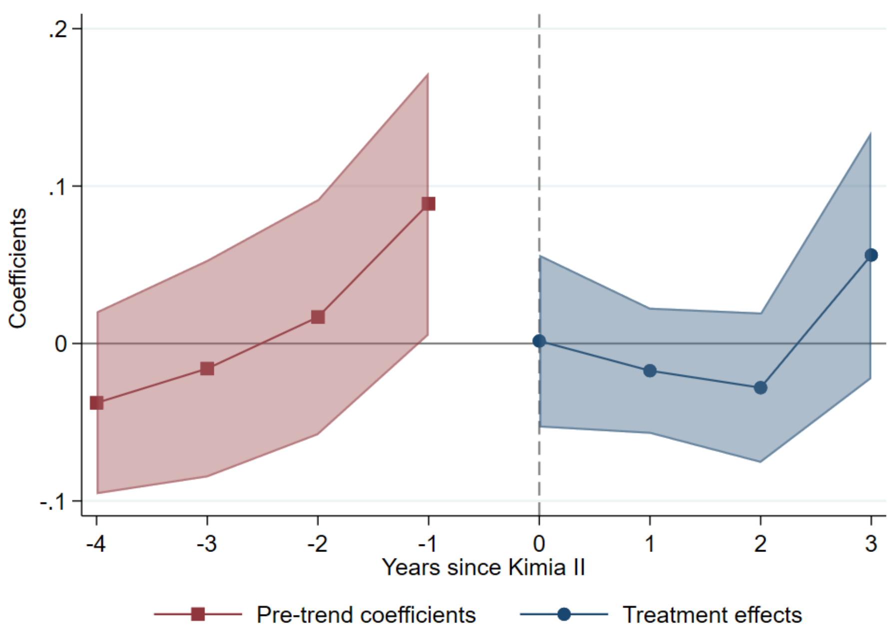
[25]Panel B shows mild upward pre-trends in non-FDLR violence in non-FDLR state villages. It is entirely driven by 2008, which is the CNDP rebellion in North Kivu.

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 using Borusyak et al. (2020)'s robust and efficient estimator. 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. 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.

5.4. *Robustness to Alternative Specifications and Controls*

In this section, we present the Differences-in-Differences coefficient, and use it as benchmark to examine the validity of alternative explanations for our finding. We first estimate the differences-in-differences coefficient β^{DD}, where α_i^{DD} are village fixed effects, and β_t^{DD} and $\beta_t^{DD:NK}$ are the North Kivu province year fixed effects, using OLS with standard errors clustered two ways at the village (239 clusters) and chiefdom-year (169 clusters) levels in the sample of 2005–2012, from the following linear equation:

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

Our choice to present the OLS coefficients is for convention and, since the timing of Kimia II is identical for all targeted villages, is also without loss. The results using Borusyak et al. (2020)'s robust and efficient estimate are almost identical.[26]

We present the estimates for β^{DD} in Table II. The sample is composed of 1,544 village*year observations in 2005–2012. Column (1) indicates that completion of 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 FDLR attacks incidence.[27] This suggests a large effect of Kimia II.

However, both for the event study and the Differences-in-Differences, this does not necessarily identify the causal effect of Kimia II completion on FDLR attacks. It is possible that the assumptions of the event study and Differences-in-Differences estimation do not hold. 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

[26]Replacing chiefdom-year clusters with chiefdom-Period clusters is inconsequential.

[27]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.

Table II: Differences-in-Differences Estimates

	Dependent Variable: *Attack by FDLR*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDLR$_i$ x $\mathbf{1}(t > 2009)$	0.24***	0.18**	0.24***	0.40***	0.21**	0.23***	0.46***	0.50***	0.43***
	(0.07)	(0.07)	(0.07)	(0.08)	(0.10)	(0.07)	(0.16)	(0.14)	(0.13)
Village FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Spillover Villages	N	Y	N	N	N	N	N	Y	N
District-Year FE	N	N	Y	N	N	N	Y	Y	N
Village Year Trends	N	N	N	Y	N	N	Y	Y	N
Pre-treatment Controls $\times$ Year	N	N	N	N	Y	N	Y	Y	N
Control for Migration	N	N	N	N	N	Y	Y	Y	N
Conditional on Gold Village	N	N	N	N	N	N	N	N	Y
Observations	1,544	1,912	1,528	1,544	1,376	1,246	1,104	1,422	390
R^2	0.29	0.25	0.31	0.30	0.29	0.32	0.35	0.32	0.38
Village Clusters	193	239	193	193	193	193	193	239	49
Chiefdom-Year Clusters	168	176	168	168	168	168	168	176	80
Mean Dep. Var.	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0

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. *Pre-treatment Controls* $\times$ *Year:* 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. *Conditional on Gold Village:* includes only villages in the sample that have gold. *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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

on all variables presented in Table I.[28] Column (6) adds the number of immigrants and emigrants as control. Columns (7) and (8) include all of the previous controls for the main sample and the sample including the spillover villages, respectively. Column (9) includes only villages that have gold, to account for the fact that Basile is a gold rich area and that perhaps the rise captures gold specific shocks. The coefficient is unaffected across any of these modificaitons to the main specification.[29]

Appendix D presents robustness tests. Table D.1 replicates the fanalysis of the predicted probability that a village belongs to the FDLR state for each covariate. In Table C.1, 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 being displaced to the local forest of Itombwe and attacking all neighboring villages. In Table C.2, we replace village fixed effects with the lag of the dependent variable. Figure C.1 conducts two falsification exercises for 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. Only the year of the campaign produces the result.[30]

Appendix E estimates Equations 1 and 2 using data from ACLED (2020) including various robustness checks for ACLED. The findings are robust to each of these specifications.

[28] 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.

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

[30] 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 D.1 presents the distribution of coefficients from the simulations.

6. MECHANISMS

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

6.1. *Disruption of the Targeted FDLR Units' Expropriation Strategy*

Available qualitative evidence suggests that the late 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). 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. While the purported motives can vary and be combined, these are generally accepted classes of attacks by the population of the region (and the nomenclature comes from the villagers themselves).

Table F.1 validates this classification using observable characteristics of each attack. Pillages tend to be shorter and take place at night, which, based on our qualitative data, is a strategic choice allowing to evade (rather than confront) state forces. Indeed, two thirds of pillages take place at night, against 20–26% of other attacks, and the fraction of pillages lasting under three hours is twice as large. Pillages have more kidnapping of village men (typically for transporting stolen goods), and with the stealing of cattle.[31] The average market value of stolen goods in a pillage is 5,464.82 USD, against 2,764.99 and 3,258.20 for conquest and punishment, respectively.[32] This implies that 3.2 pillages, which purportedly destroy future expropriation potential, yield the same as one year of taxation.[33]

With this data, we now examine whether the rise in FDLR attacks could indeed be explained by pillaging. Table III presents the Differences-in-Differences estimates from Equation 2.[34] Column (1) includes, as dependent variable, an indicator for whether the FDLR conducts a pillage operation.[35] The magnitude of the coefficient is unchanged. In Column (2) the dependent variable is again whether the FDLR attacks the village, but we include, as control, an indicator for whether the FDLR pillages. The coefficient is indistinguishable from zero. This suggests that the rise in attacks following the successful completion of Kimia II can only be explained by pillaging operations conducted by the FDLR.

One concern with these results is that the information that the attack is motivated by pillaging is constructed from subjective reports by the villagers. Given potential concerns about this assumption, we complement this data with the household-year level retrospective 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.[36] 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

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

[32] 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.

[33] 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.

[34] Figure F.8 presents the event study estimates.

[35] 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.

[36] 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.

Table III: 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***	0.01**	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 Column (1), 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). In Column (2), the dependent variable is an indicator variable taking value 1 in village i and year t if the village is attacked by the FDLR (irrespective of the motive). In Columns (3)-(8), the dependent variable is an indicator for whether household j in village i is directly 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 pillage*: includes as control an indicator taking value 1 if village i is attacked by the FDLR in year t with the motive to pillage 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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

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 completion should lead to a disproportionate increase in attacks by FDLR on 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 comes, in this case, exclusively from the household survey.[37] Columns (4)–(8) include, as control, $\mathbf{FDLR}_\mathrm{i}$ 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 vari-

[37]Indeed, the village-level estimate shows that the likelihood of an FDLR state village experiencing at least one FDLR attack after 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 after 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.

ables $\boldsymbol{HH_j}$ completely explain the rise in household level targeting of attacks after Kimia II. The coefficient on $\mathbf{FDLR}_\mathrm{i}$ x $\mathbf{1}(t > 2009)$ turns to zero and is almost never statistically significant. In contrast, the coefficient on $\mathbf{FDLR}_\mathrm{i}$ x $\mathbf{1}(t > 2009) \times \boldsymbol{HH_j}$ is always large and statistically significant.[38] This indicates that Kimia II completion led to a rise in attacks on rich households, those that would be attacked in attacks motivated by expropriation.

A natural concern is that this assumes the FDLR knew which ones were the wealthiest households. The qualitative reports from the villages of Basile confirm the validity of this assumption, however. For instance, the report of Kyunga explains how the FDLR had information from the village wealth during their attacks:

> "Some villagers let themselves manipulate by the FDLR and worked for them as spies inside this village and other villages."

This type of report is very common in the qualitative data that we gathered in the area. A common strategy for the FDLR to render their short pillaging operations more profitable is to ensure that they have spies from the village helping them identify where wealth is.[39]

Why did Kimia II completion incentivize violent theft? Our qualitative data suggests it did so by decreasing the value of future expropriation in the "liberated" villages:

> "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."
> "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-combatants 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:

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[38] 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, $\mathbf{FDLR}_\mathrm{i}$ x $\mathbf{1}(t > 2009)$ interacted with other household characteristics. 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.

[39] The name given to those spies in this setting is "eclaireurs." It means, those who "provide light," as attacks are perpetrated at night.

> "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 completion could have altered an inter-temporal trade-off that, prior to Kimia II, disincentivized pillaging. Section G.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 low, the bandit expropriates everything.

We begin our examination of this trade-off by verifying that Kimia II did succeed in decreasing expropriation frequency. We combine taxation and pillaging events. Using the information we gathered for the frequency of the collection of each episode 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 recover the frequency of expropriation events per year. Figure F.12, Panel A, shows the frequency of expropriation estimated this way. It confirms that Kimia II is associated to a reduction in the frequency of FDLR expropriation frequency in the FDLR state, from 350 to 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 F.12, 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 the successful completion 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 F.1 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 this interpretation is that Kimia II completion may have increased pillaging because it reduced the contemporary income of the targeted factions as a whole, an income effect.[40] 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. We define high frequency as any frequency higher than 1 event per year, which is the 90$^{\text{th}}$ percentile of the distribution of frequency in the sample.[41] Figure F.6 reports the 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 is consistent with Kimia II completion affecting the FDLR pillaging because it reduced its expected frequency of expropriation.[42]

6.2. *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 completion, but it could also reflect that the FDLR chose to attack the

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[40]Indeed, including only our sample of villages, revenue by the FDLR as a whole in Basile decreased from 300,000 USD to 100,000 USD in one year.See Figure F.11.

[41]The results are unaffected by changing the cutoff.

[42]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. Since 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 F.14 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).

villagers they formerly taxed as retaliation or punishment. For instance, they may strategically target specific villagers in those villages for collaborating with the army 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 punish villages and specific villagers, we first use our data on reported motive for the attacks. Figure F.10 presents the event study estimates for this regression. Table IV, 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 without this control.

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 would 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. Those who would 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 debt formation. If the rise in FDLR attacks is associated to retaliation, these FDLR attacks should 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

Table IV: 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 $\mathbf{HH}_j$				0.00	0.05	0.04	0.23	0.01
				(0.03)	(0.03)	(0.04)	(0.23)	(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
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 in Column (1) is an indicator variable taking value 1 for village i and year t if the village is attacked by the FDLR *with the purported intention to punish the villagers* (FDLR Punishment Attack). The dependent variable in Column (2) is an indicator variable taking value 1 if the village is attacked by the FDLR. The dependent variable in Columns (3)–(8) is an indicator for whether 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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

baseline of zero. Columns (4)–(8) include, as control the main explanatory variable, **FDLR**$_i$ 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 **FDLR**$_i$ 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.

6.3. *War Between FDLR Units and the Congolese army*

Another possibility is that, even if expropriation is a driving factor for the rise in attacks, the attacks themselves could simply be reflecting war between the FDLR and the Congolese army—even if Kimia II was already complete by the end of 2009. If the effect of Kimia II on FDLR's attacks reflects war with the Congolese army, Kimia II targeting should predict whether the FDLR attempts to regain territory. This would be produced if the FDLR attacks the villages retaken by the Congolese army, or if the Congolese army attacks them.

Table V, Column (1) presents the estimates from Equation 2 but uses, as dependent variable, an indicator variable for whether the FDLR conducted a conquest operation.[43] The coefficient is nearly zero, negative, and statistically insignificant. This shows that Kimia II completion had no effect on war with the FDLR in the FDLR state.

In Columns (2) and (3) the dependent variable is again an indicator for whether the FDLR conducts a violent operation. Column (2) adds, as control, an indicator for whether the FDLR conducts a conquest operation. Column (3) includes, as control, an indicator for whether the Congolese army controlled the village in the previous year. The coefficient in Columns (2) and (3) is identical to the main coefficient of Table II, indicating that conquest motivations or war with the Congolese army do not explain the main effect.[44]

[43]Figure F.10 presents the event study estimates.

[44]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.

Table V: Mechanisms — War

	Dependent Variable					
	FDLR Conquest Attack	*FDLR Any Attack*				*Congolese Army Attack*
	(1)	(2)	(3)	(4)	(5)	(6)
FDLR$_i$ 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 in Column (1) is an indicator variable taking value 1 for village i and year t if the village is attacked by the FDLR *with the purported intention to gain territorial control of the village* (FDLR Conquest Attack). The dependent variable in Columns (2)–(5) is an indicator for whether the village is attacked by the FDLR. The dependent variable in Column (6) is an indicator for whether the village is attacked by the Congolese army. 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 = 0 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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

Columns (4) and (5) subset the sample to whether the Congolese army already controlled the village, or did not, respectively. All the FDLR attacks that ensue the completion of Kimia II are concentrated in villages in which the Congolese army is not stationed. This rules out that their attacks were due to territorial war with the Congolese state, in which case they would target villages taken by the Congolese army. 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.

7. IMPLICATIONS FOR HOUSEHOLD WELFARE

We now analyze the effect of Kimia II completion on proxies for household welfare, for the households located in the chiefdom targeted by Kimia II.

We first document the types of violence on households caused by Kimia II completion.[45] Table VI estimates Equation 2 where the dependent variables in Columns (1)–(4) are indicators for whether the village experiences each of the following: sexual violence (1), villagers' killing by armed actors (2), looting of households (3), and abduction (4). Kimia II completion doubles the fraction of villages who experience sexual violence and of plundering by armed actors. It also quintuples the fraction of villages in which armed actors abduct villagers, typically for forced labor including transporting stolen assets.

Columns (5)–(11) analyze the material benefits of Kimia II completion. 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 total amounts taxed by any actor (that is, including by the Congolese state) 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 pillaging or taxation. In our sample, after the FDLR is removed, the only armed actor collecting taxes in the FDLR state is the Congolese army. Those payments are all informal taxes. Thus, the effect on total household disposable income depends on whether the taxation by the FDLR was higher than that by the Congolese army.

[45] Section G.2 presents an organizing framework to analyze household welfare including physical safety and household gross and net income.

Table VI: Implications for Household Welfare: Dis-utility 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* (1)	*Death* (2)	*Looting* (3)	*Kidnap* (4)	*Theft* (5)	*Market* (6)	*Mill* (7)	*Toll* (8)	*Poll* (9)	*Total* (10)	*Total* (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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

Columns (5) - (11) show that the value expropriated through pillage for the average household increases in 1.42 USD following Kimia II (doubling its baseline level), total taxation payments per year per household decreases from 63.84 USD to 37.69 USD, a 41% decrease.[47] This amounts to a 39% reduction in the total per household payments from 64.31 USD to 39.53 USD, a decrease from 25.17% to 15.48% of national p.c. income.[48]

While Kimia II completion 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. To proxy for income changes arising from variation in income across occupations, we analyze the household histories of occupational choice. For unemployment, and subsistence farming, income is low. In mining, income is generally orders of magnitude larger. If the taxation by the FDLR was distortionary so that it decreased labor supply in mining, Kimia II completion should be associated with lower unemployment and subsistence farming, and higher fraction of individuals in mining.[46]

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

8. HOW TO BUILD A STATE WITHOUT A MONOPOLY OF VIOLENCE

We examine the effects of an alternative policy to remove nonstate actors: integrating them into the state. Instead of using threats of force to remove armed groups, the state can integrate armed groups into the army. In contrast to the perils we have shown, this can expand the state without creating violent roving bandits.

[47]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.

[48]We used World Bank (2021) estimates of adjusted net national income per capita in DRC in 2009 in current USD. Table F.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.

[46]We complement this analysis with proxies for household spending, meant to capture variations in household income: schooling and weddings spending (both require payments). The fraction of individuals in school and those who are married increases, consistent with Kimia II completion increasing household disposable income. In Table F.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.

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 active, but inside the state army (Verweijen, 2013).[49] Figure VI shows the evolution of armed group behavior in this alternative strategy to assert the state. In contrast with Kimia II, we see no rise in roving banditry nor violence against civilians.

9. CONCLUSION

Weak states have gained significant attention since 9/11 due to the massive development and security challenges they pose. Yet, we still do not understand well the economics of state expansion and of violence in their environments. A common feature of weak states is that they do not even hold a monopoly of violence. Instead, nonstate armed actors regularly tax and provide protection, which has been documented to discipline their incentives to use violence arbitrarily. A natural question for weak states trying to build state capacity is thus: how can they build state capacity in the areas they do not yet control?

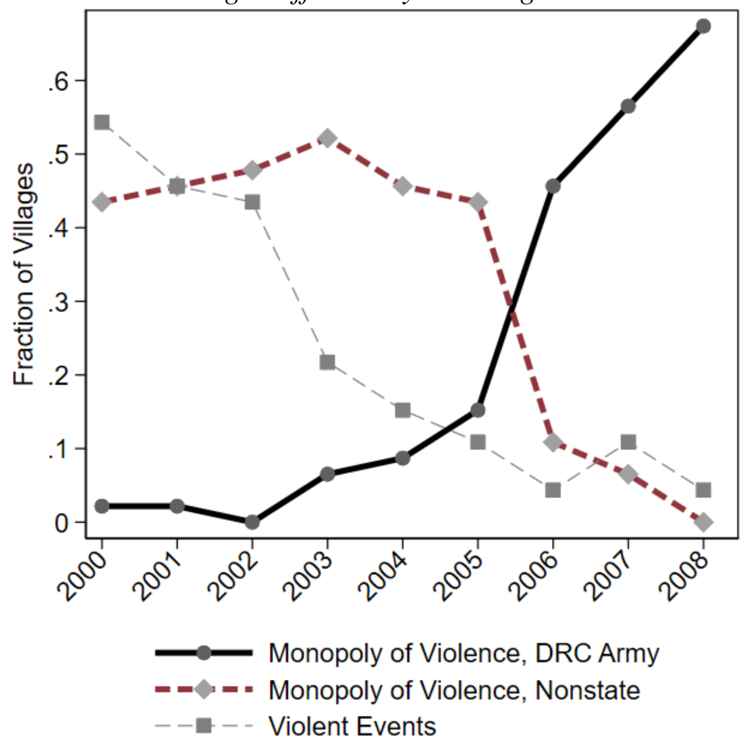
This paper has shown that military efforts to build state capacity by making it impossible for nonstate armed actors' factions to tax succeed in relaxing the affected households' informal taxation burden, but came at incalculable costs to the citizens of these households—none of whom was consulted in the design of the policy, despite being the bearer of a significant social cost. By making it impossible for armed actors to tax, asserting the state can disrupt their public finance, which is often precisely the source of their stability. Without advanced mechanisms for demobilization and re-integration of combatants, and without the ability to significantly deter violent crime, as is the case for most states and especially weak states, this disruption incentivizes violence. In our context, after the campaign to remove the factions from tax schemes they had created was successful, they begun using violence for each of the three subsequent years. The reasons for the spike we identify are economic.

These findings offer a new avenue for explaining the causes of violence, and suggest that the ability to regularly expropriate citizens can be a source of the citizens' own protection. Precisely this link is disrupted when the state uses the threat of force to assert its territorial control making it impossible for them to tax. Expanding the state creates incentives for vi-

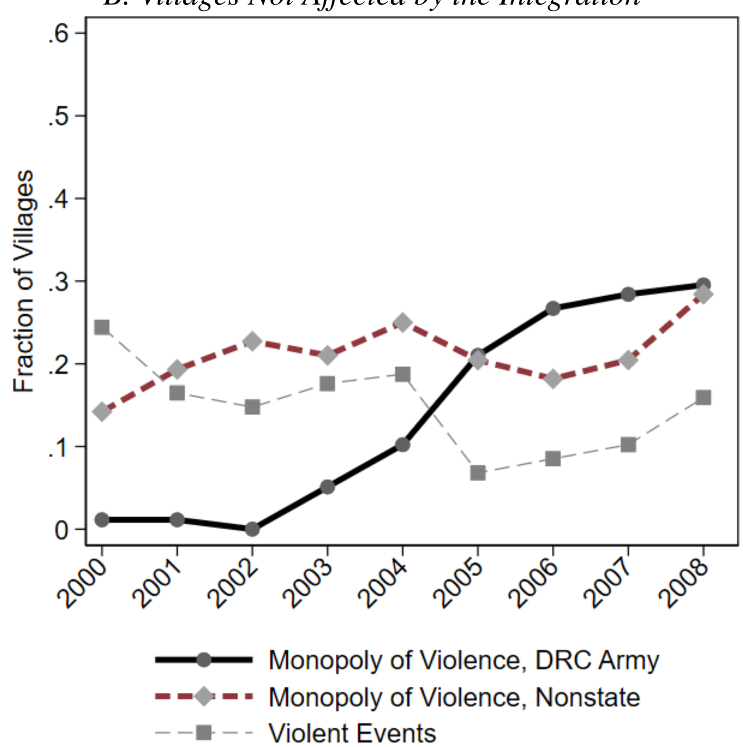
[49]Implementing this "integration" into the state forces was met with a number of challenges that are beyond the scope of this paper (Autesserre, 2006).

Figure VI.: Building States by Bargain

A. Villages Affected by the Integration



B. Villages Not Affected by the Integration



Notes: This figure presents the fraction of villages that are controlled by the DRC State, nonstate armed groups, as well as the fraction of villages where a conquest attempt takes place, by nonstate armed actors.

olent banditry when the conditions to prevent it have not been met. Although we discussed in passing alternative strategies for weak states to assert their territorial control, those remain beyond the scope of our study and encouraging avenues for future research on the economics of state building.

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Online Appendix

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.

APPENDIX 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. *Detailed Description of Specific Survey Measures*

B.3.1. *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.[50] 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 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").

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

C.1. *Summary of Non Conflict Related Variables*

Table C.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.

APPENDIX C: Addressing Ethical Challenges in The Data Collection

We obtained authorizations from provincial, territory, and village state authorities in 2012 for South Kivu and 2015 for North Kivu, as well as customary authorities to whom the project was presented in detail. Ethical guidelines were followed to ensure that respondents did not feel obliged to participate and that their participation did not expose anyone to risk. The study was approved by the Institutional Review Boards (IRB) of Columbia University (Protocol: IRBAAAK0552 (Y1M00)), Berkeley (Protocol: 2016-06-8849; FWA: 00006252), and Harvard University (Protocol: SITE18-0356; FWA: 00004837).

The team of researchers was composed of ten Congolese male University graduates. Our failure to include other genders at the time was a response to the insecurity conditions that researchers would face when travelling through the conflict zone. While it was initially attempted to expand the scope of the research team composition, given the gender-specific risks involved in travelling through areas of complete state absence where various armed groups and bandits operate, the research team jointly settled for this team composition. This outcome was also the product of self-selection by potential surveyors given the risks involved in the type of data collection we were proposing. We believe that this type of non-inclusive access to the production of knowledge can create bias in the way information is processed during interviews and is certainly avoidable in many contexts, with the reverse composition being optimal in a large number of contexts.

Beyond the scope of institutional review, there are two central ethical dimensions that we engaged with. First, we are asking people to tell us about memories that are, potentially, traumatic. While we did not hire professional psychologists to assist respondents during the recounting of potentially difficult memories, ethical guidelines were followed to ensure that respondents did not feel obliged to participate and that their participation did not expose anyone to risk. The team of of ten researchers was trained, and highly skilled, in using empathy during the entire data gathering activities and were instructed to detect when some information could trigger emotional responses associated to trauma, and thus not collect it. We found that respondents were eager to tell us their stories and viewed this survey as an opportunity to be heard. Second, the data collection took place in areas where researchers could potentially be exposed to risk. We were especially concerned that researchers may not feel comfortable communicating to us that a given area was risky, or even felt economically coerced to visit dangerous areas due to the income that they could generate. In order to prevent this risk, we created a position in our team, by one of the associated researchers, in charge of detecting risk. To obtain information on risk, this person was connected daily to INSO, a service offered to all NGO's operating in the same areas as our teams, with recommendations for travel and recounts of all events. INSO uses a broad system of informal networks. In addition, we used our own networks. The researchers were trained into opting out of certain areas if those were deemed risky either by them as individuals (who had better information from the ground than anyone else) or by the person in charge of security. We recorded no violent incident on the study members.[51]

[51] In a few occasions, the researchers were harassed at roadblocks. In one district, which was controlled by an armed group (Raia Mutomboki), the researchers were detained for 2–3 days during which they explained the survey in detail to the armed group leadership, prior to obtaining authorization to proceed.

Table C.1: Alternative Econometric Specifications

	Dependent Variable: *FDLR Attack*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FDLR$_i$ 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)$_i$ 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)**$_i$ 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**$_i$ x $\mathbf{1}(t > 2009)$, is replaced with **Log(Distance to FDLR State+1)**$_i$ 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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

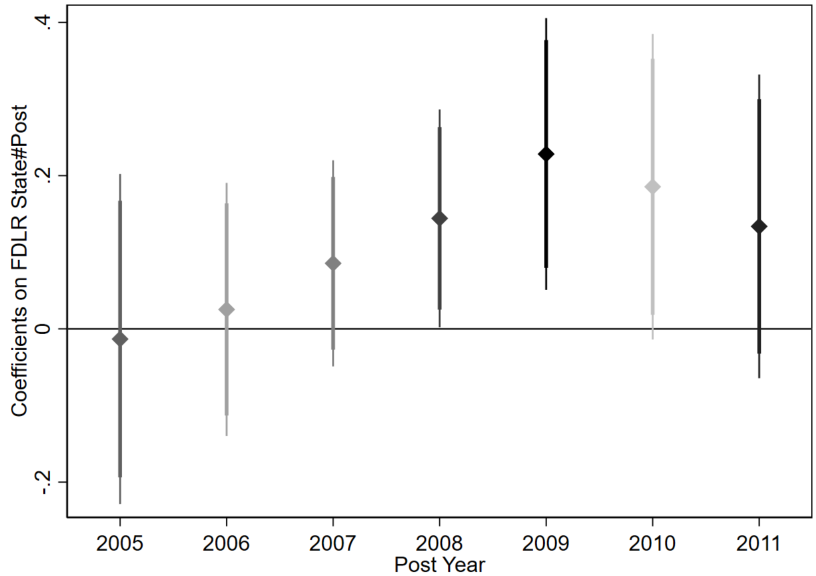
Table C.2: Specification with Lagged Dependent Variable

	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
Pre-treatment Controls × Year	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

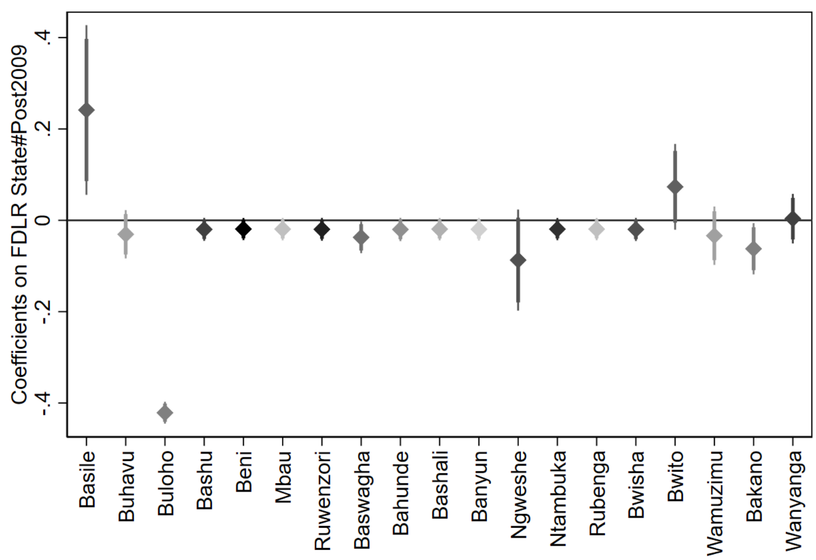
Notes: This table reports the coefficient estimates from Equation 2, but instead of including village fixed effects, we include the lag of the dependent variable. The dependent variable is an indicator variable taking value 1 in village i and year t if village i is attacked by the FDLR in year t, and 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. *Pre-treatment Controls* × *Year:* 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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

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.

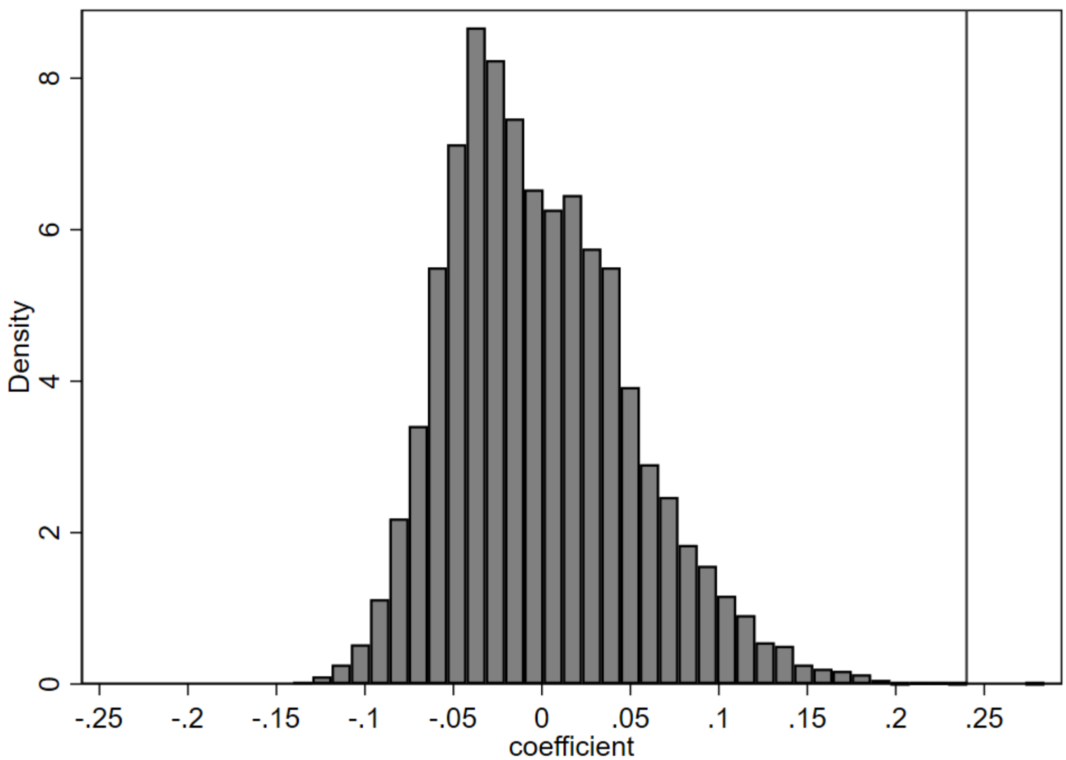
APPENDIX D: ROBUSTNESS

Table D.1: Selection Into the FDLR State

	Dependent Variable: *FDLR Attack*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Targeted$_i$ 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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

Figure D.1.: 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.

APPENDIX E: REPLICATION USING PUBLIC DATA ACLED (2020)

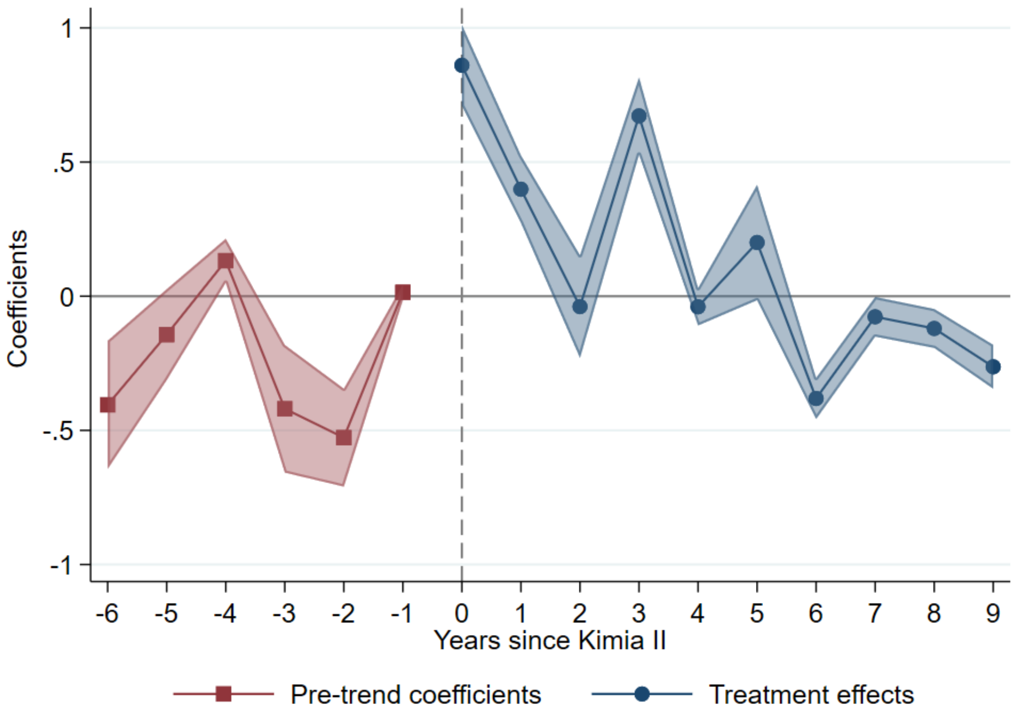
In this section, we present the analysis replication using ACLED (2020) data.

First, we replicate the main event study result. Figure E.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.

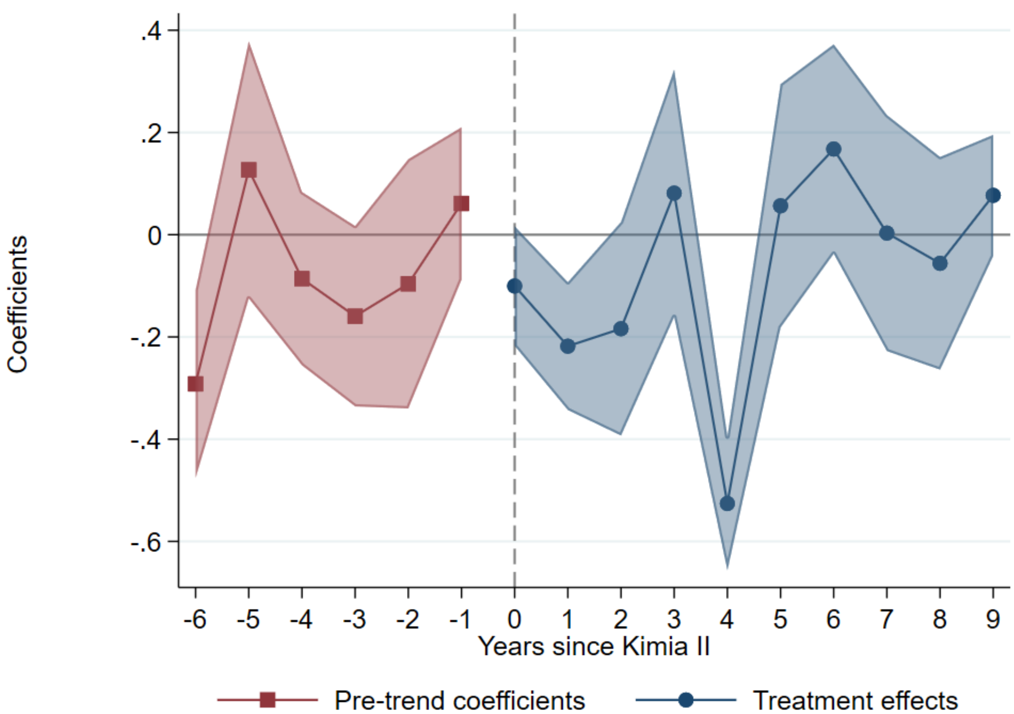
Second, we replicate the Differences-in-Differences estimation and implement robustness checks using the data in ACLED. Figures E.2 and E.3 estimate the Difference-in-Differences coefficients, using different definitions for matching of a violent event. Similarly, Figure E.4 estimates the Difference-in-Differences 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 E.1.: ACLED Data Replication of Main Event Study estimation

A. Main result: Attacks by FDLR

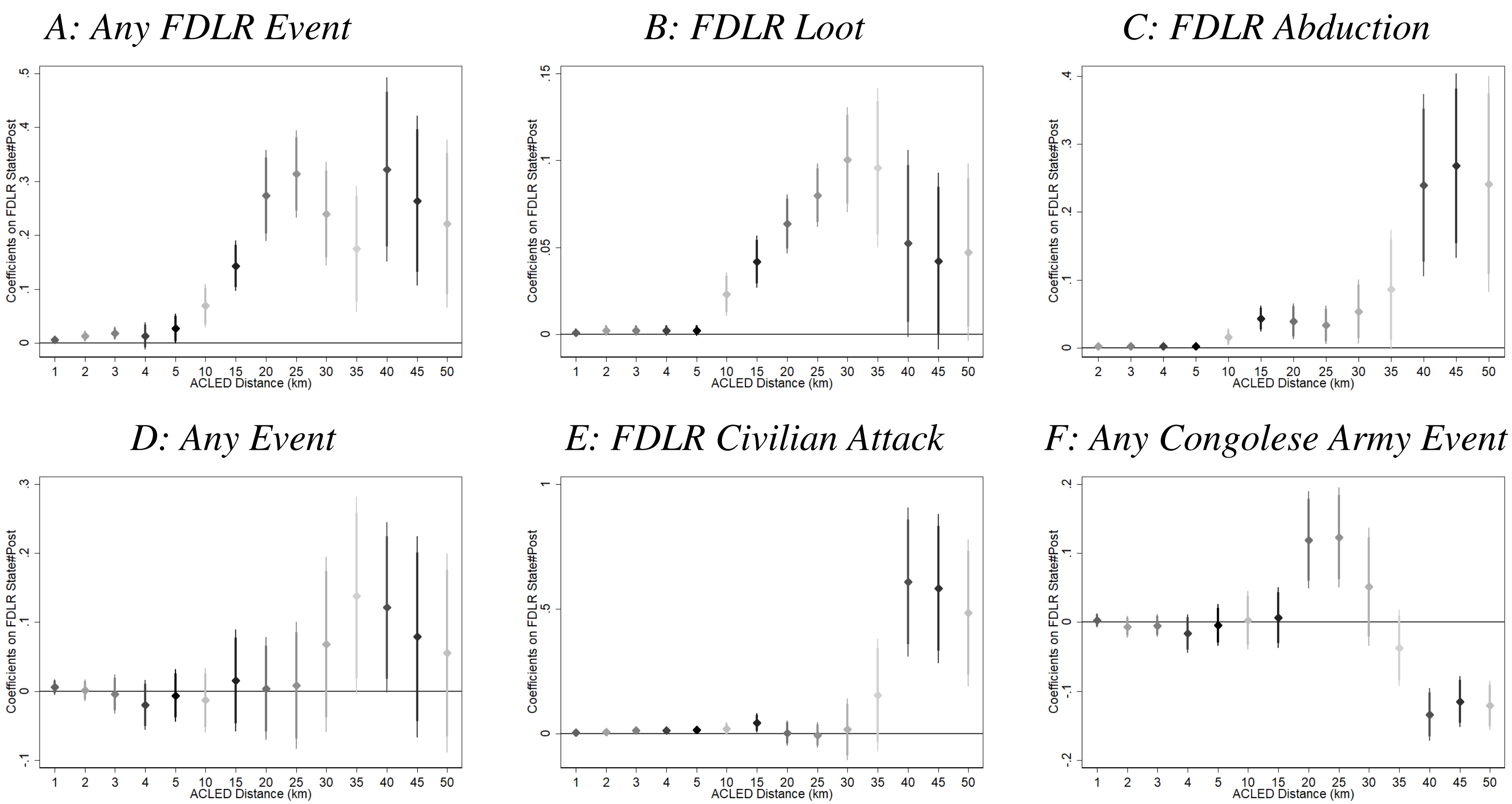


B. Falsification Exercise: Attacks by non-FDLR Armed Actors



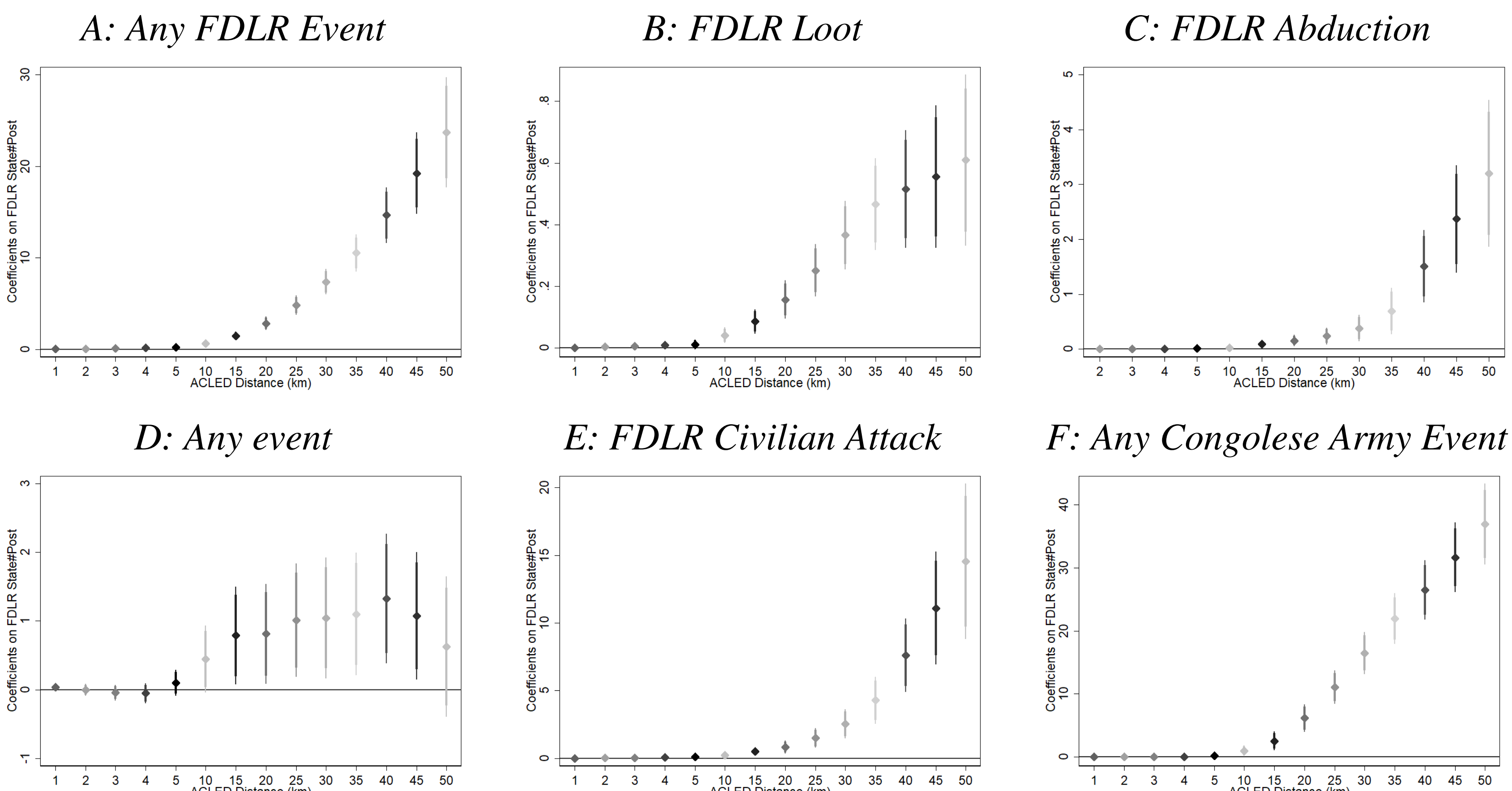
Notes: This figure shows the coefficients on year indicators estimated from Equation 1 using Borusyak et al. (2020) robust and efficient estimator, using data from ACLED (2020). The year 2009 is the omitted category. The sample excludes Spillover Villages. Panel A shows the coefficients for the estimation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

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



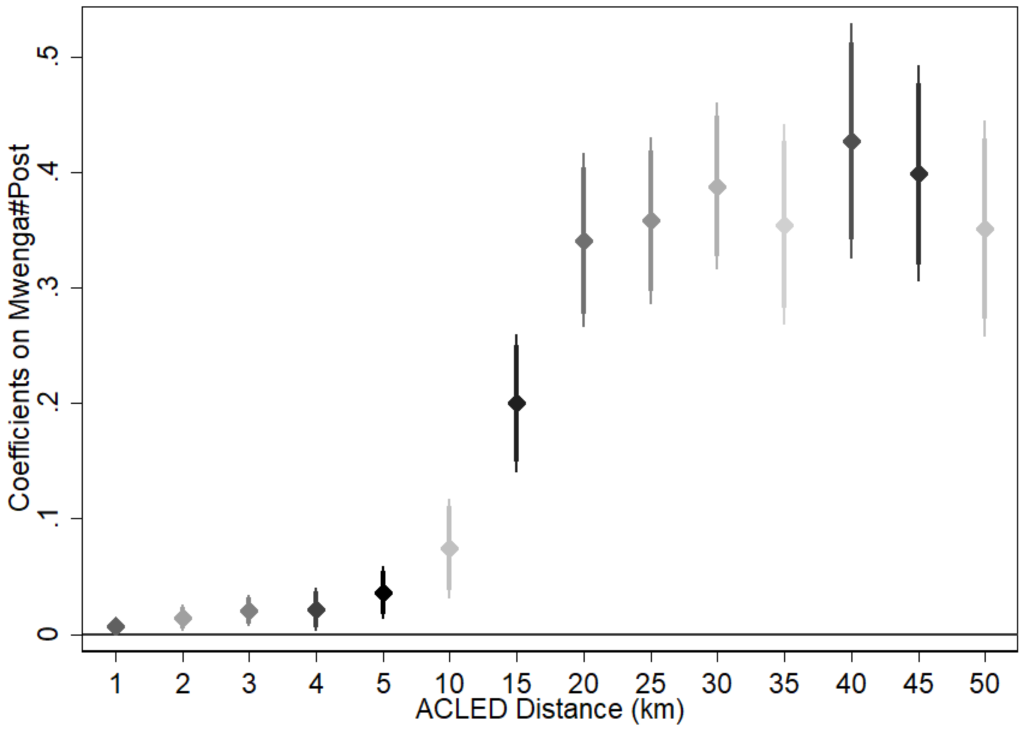
Notes: This figure shows the Differences-in-Differences coefficient using using indicator variables for different types of events different types of events in the ACLED data by the radius around the villages as dependent variables. In all panels, thick lines represent 90% confidence intervals and thin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

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

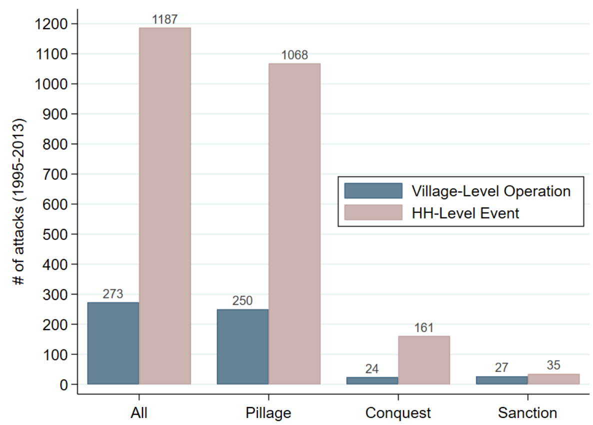


Notes: In this figure, FDLR state is coded as the entire district of Mwenga. The dependent variable is an indicator variable taking value one if there is an FDLR attack as recorded by ACLED in a circle of corresponding radius from the survey village, and zero otherwise. The figure shows the Differences-in-Differences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

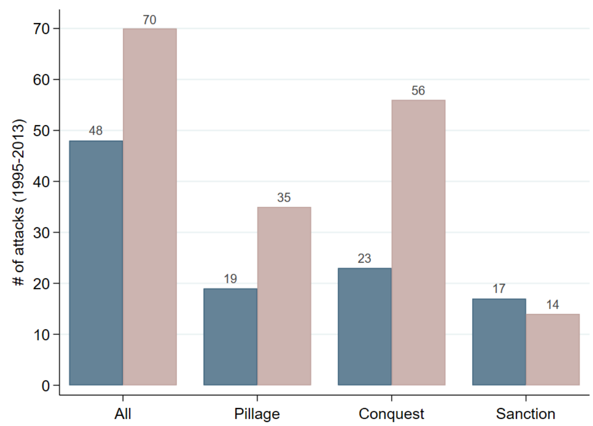
APPENDIX F: ADDITIONAL ANALYSIS

Figure F.1.: Attack Characteristics

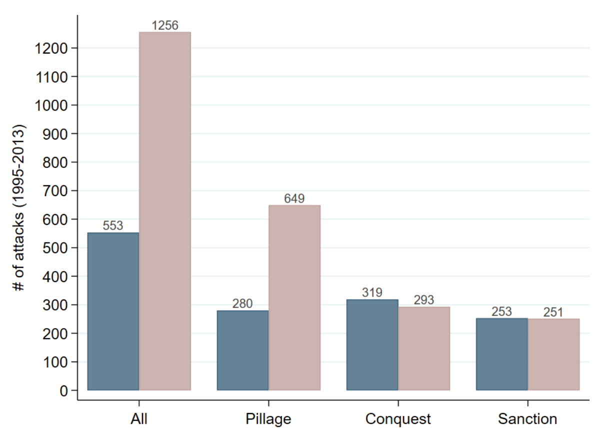
A. Perpetrated by Members of the FDLR



B. Perpetrated by Members of the Congolese Army

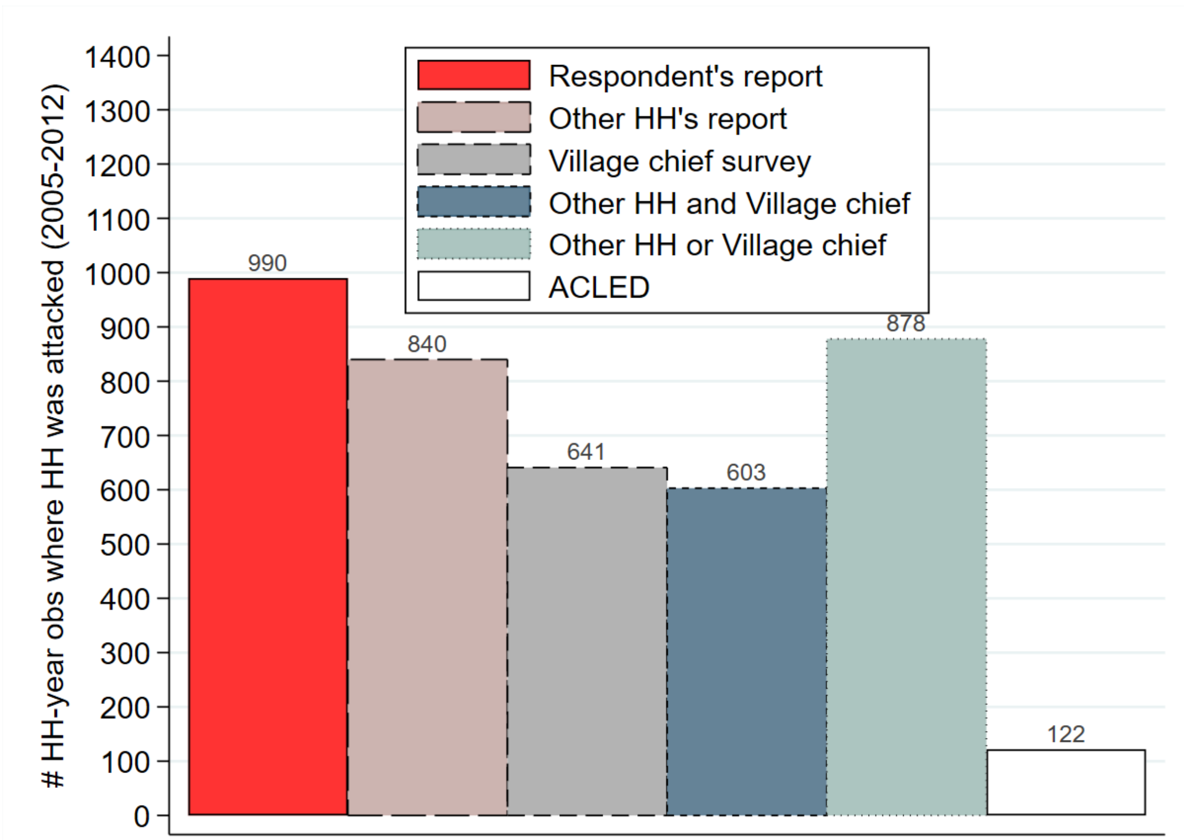


C. Perpetrated by Members of any Other Organization



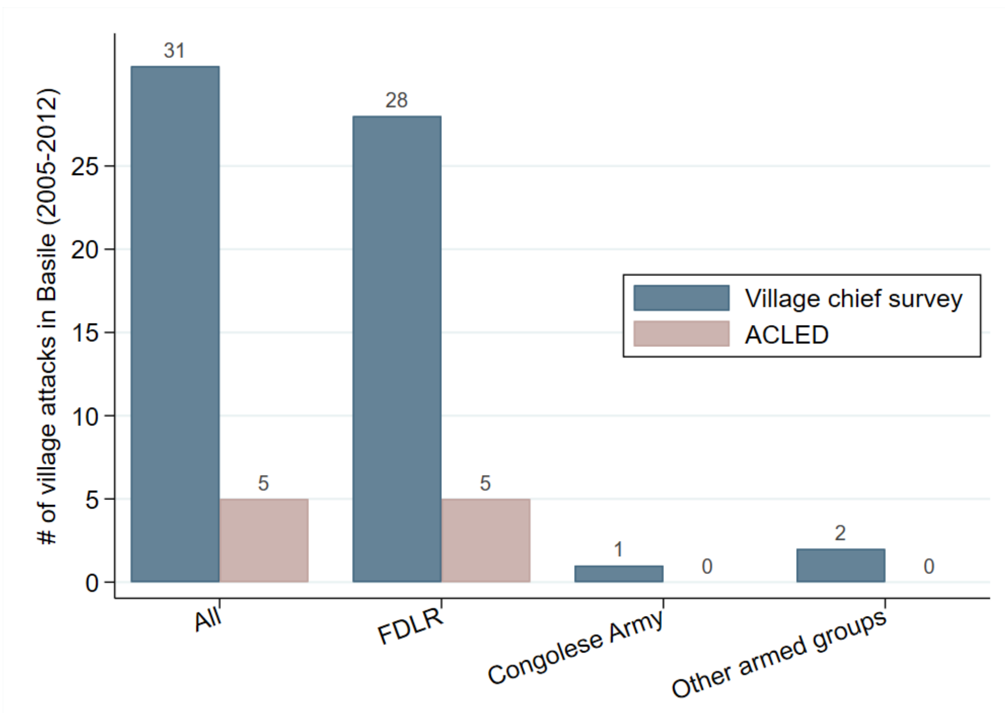
Notes: This figure shows the distribution of village attack observations with different characteristics for the entire period covered by data collection. We present the total number of recorded village-level operations in blue bars. Since in each of these operations, the households can sometimes also be victimized, we also report the estimated number of households in the village in which violence took place during the operation. We estimate the number of households in the village in which violence took place, we use the household-level reports for whether household members were directly victimized for households in the sample and combine this with the information on the number of households sampled and number of households in the village. The rose bars labeled "HH attacked" present the number of victimized households estimated.

Figure F.2.: Cross-validation of Reported Attacks on Households (2005–2012)



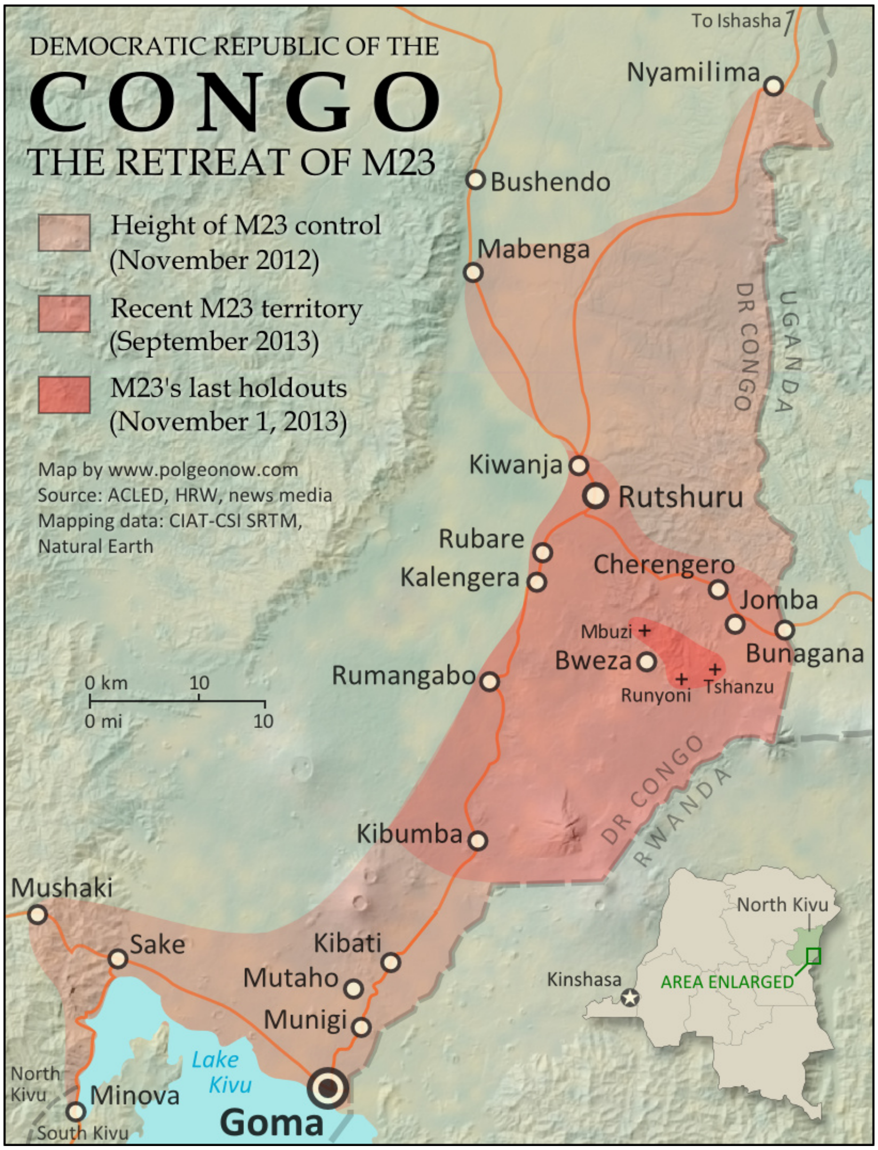
Notes. This figure shows the cross-validation of the household attack reports in the sample across North and South Kivu. The unit of analysis is the household-year observation. For comparability, observations in which the respondent is not living in the village of survey, *i.e.*, the village to which the village chief reports refer, are excluded. The first bar is the number of household-year observations in which the respondent reports that their village was attacked (at least once) in year t. The second bar shows, among the reported attacks in the first bar, the number of household-year observations where at least one other household of the village also reports an attack taking place in the village within one year ($t-1$, t, $t+1$). The third bar is the number of household-year observations among the reported attacks in the first bar for which the village chief survey also reported that their village was attacked (at least once) within one year ($t-1$, t, $t+1$). The fourth bar is the number of observations at the intersection of the previous two sets. The fifth bar is their union. The last bar is the number of violent events recorded in the sample in ACLED.

Figure F.3.: Cross-validation of Reported Attacks Violent Events in Basile: Survey vs. ACLED



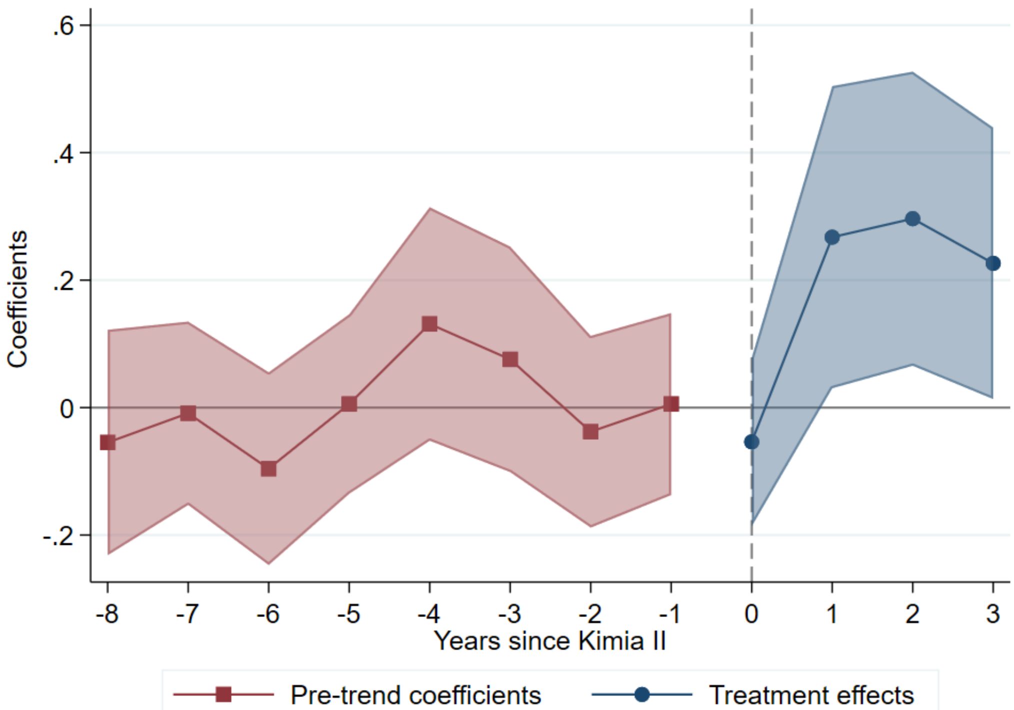
Notes. This figure shows the number of violent operations recorded by our village chief survey (blue bars) and ACLED (rose bars), respectively, including all armed actors, the FDLR, the Congolese Army, or other armed groups, respectively.

Figure F.4.: Correcting ACLED Location Information: The Example of the M23 Events, All of Which Took Place in North Kivu, but Were Wrongly Assigned to Mwenga, Because the Events Took Place near the Town of Kibumba in North Kivu, and Mwenga also has a Town Called Kibumba



Notes: This figure shows the largest territory ever operated in by the M23. Source: www.polgeonow. The M23 armed group operated in North Kivu, showing that the M23 events in our Mwenga ACLED dataset are necessarily miscoded. As the map shows, the town of Kibumba is located near Goma in Rutshuru. Comments for ACLED events show that the attacks of M23 associated to Mwenga in ACLED took place in Kibumba. Yet, in Mwenga, there is also a town Kibumba. This shows that ACLED associated these events to the wrong town of Kibumba, in the wrong district, assigning it wrong GPS coordinates. Based on this investigation, we removed those events from Mwenga in the original ACLED dataset. Once M23 was disintegrated, the members went through demobilization camps and many fled to Rwanda and Uganda.

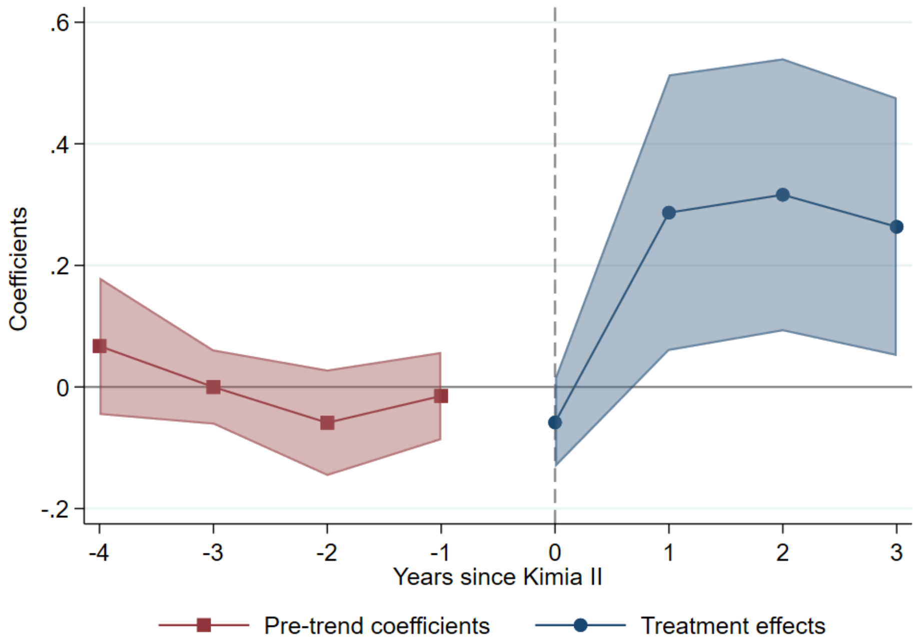
Figure F.5.: Event Study: Kimia II Completion and FDLR Attacks in FDLR State with Extended Pretrends



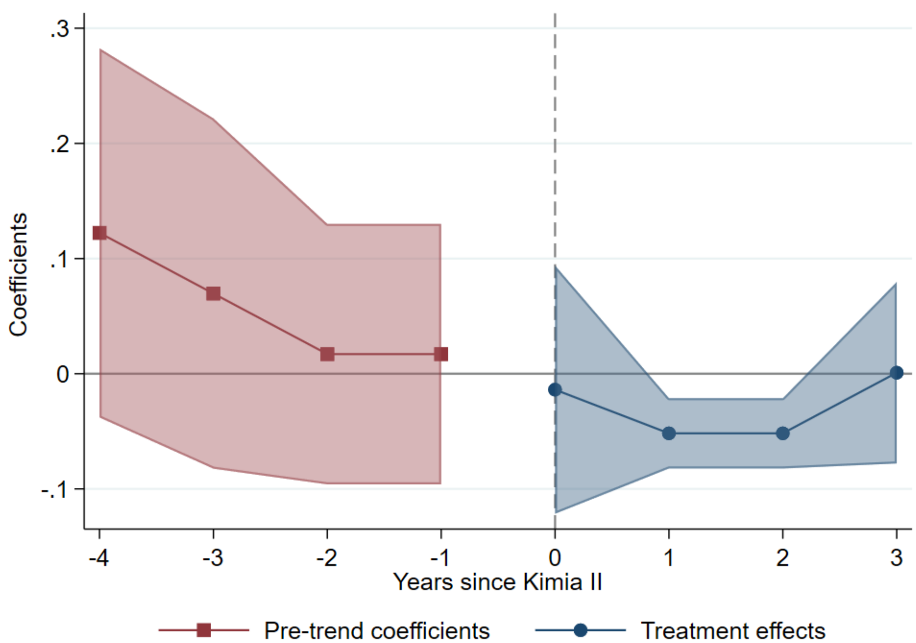
Notes: This figure shows the coefficients β_k, $k = -8, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020) robust and efficient estimator. The regression includes village and year fixed effects. The dependent variable is an indicator for whether the FDLR attacks village i in year t. The year 2009 is the omitted category. 3,474 village-year observations were used in the estimation. Standard errors are clustered two-ways at the Village level and the Chiefdom-Period level.

Figure F.6.: Explaining Expropriation Channel: Loss Frequent Expropriation Ability?

A. Dependent Variable: FDLR Violent Attack And Expropriation Frequency is Low

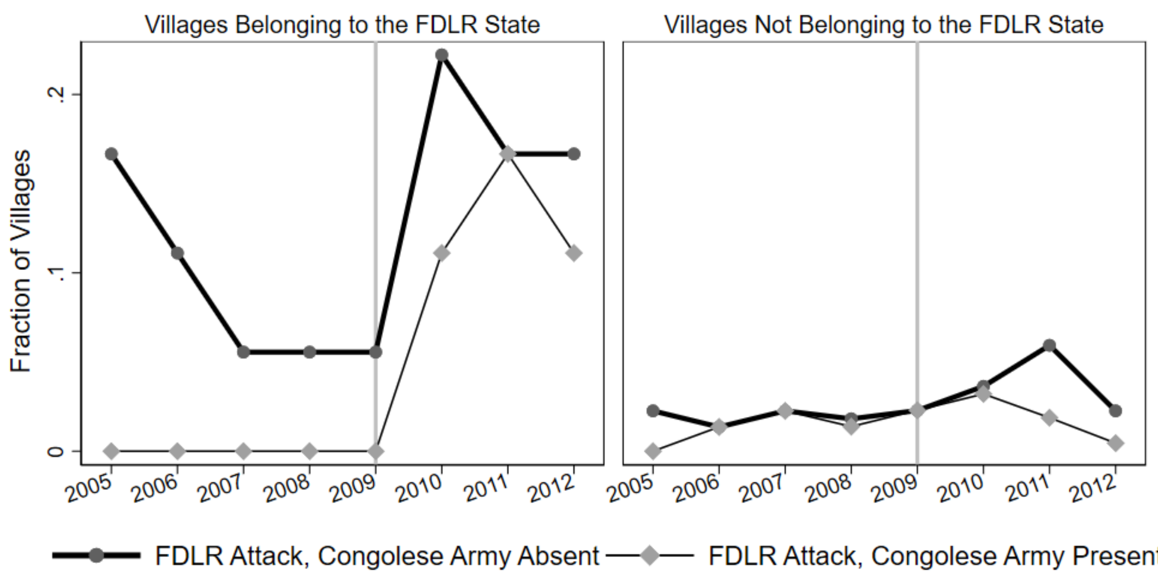


B. Dependent Variable: FDLR Violent Attack And Expropriation Frequency is High



Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020)'s robust and efficient estimator. In the top 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. In the lower 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 year 2009 is the omitted category. 1,544 village-year observations were used in the estimation.

Figure F.7.: Trends of FDLR Taxation and Attacks, by Congolese Army Control



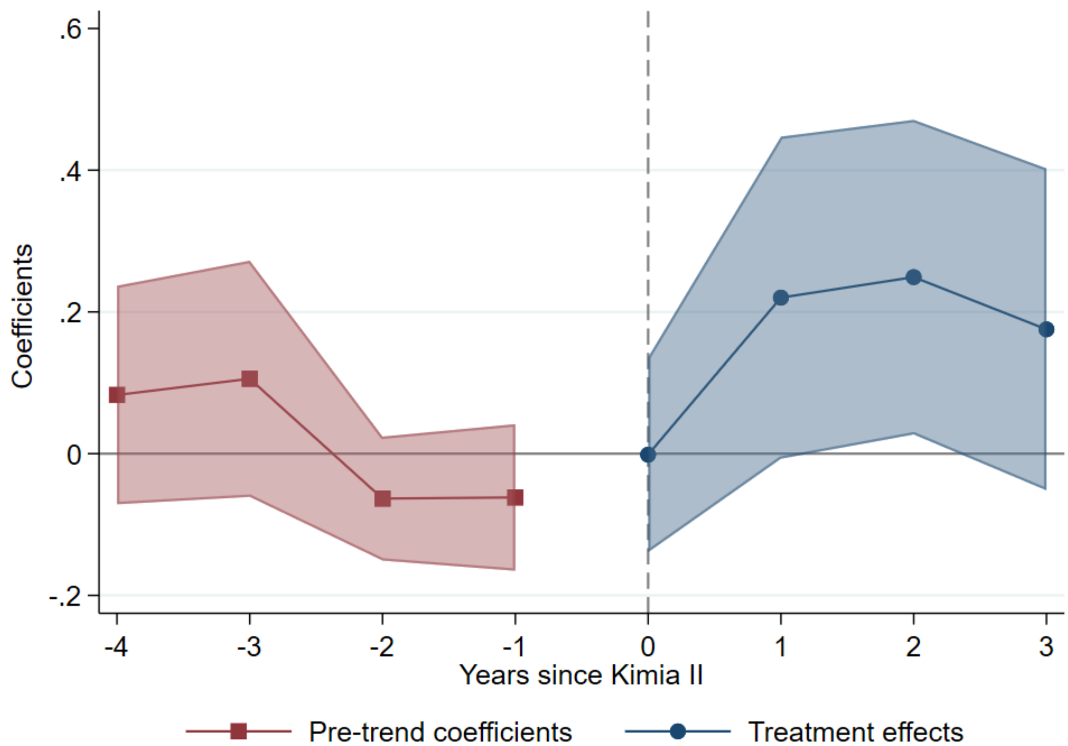
Notes: This figure shows the incidence of FDLR attacks separately for whether the Congolese army controlled the village and was present in the village at the time of the attack (Congolese army present) and whether the reported attack took place in a village either not controlled by the Congolese army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

Table F.1: 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)	

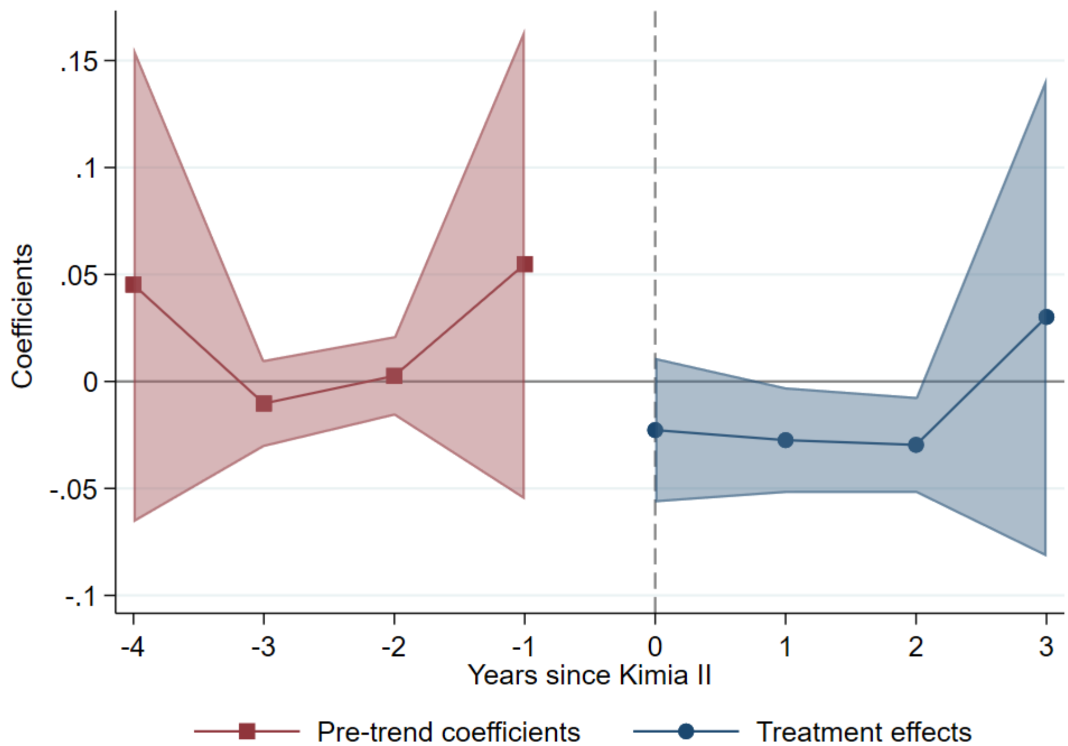
Notes: This table shows the mean of attack characteristics for different types of attacks. The table excludes 93 attacks that have purportedly more than only one motive. *P-value* denotes the p-value for the t test for whether the mean characteristic in a pillage attack is identical to the mean characteristic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

Figure F.8.: Event Study Estimates, Attack with Intention to Pillage



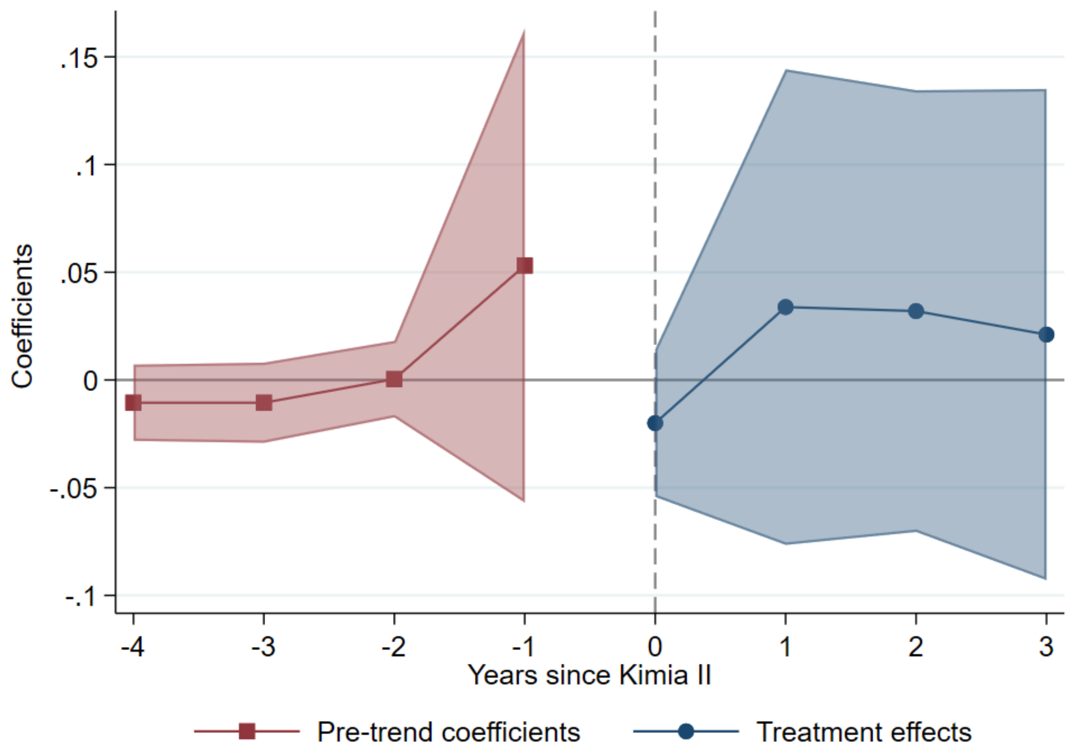
Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020) robust and efficient estimator. The dependent variable is an indicator for: whether the FDLR attacks village i in year t with the intention to pillage. The year 2009 is the omitted category. 1,544 village-year observations were used in the estimation. There are 193 and 168 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.

Figure F.9.: Event Study Estimates, Attack with Intention to Conquest



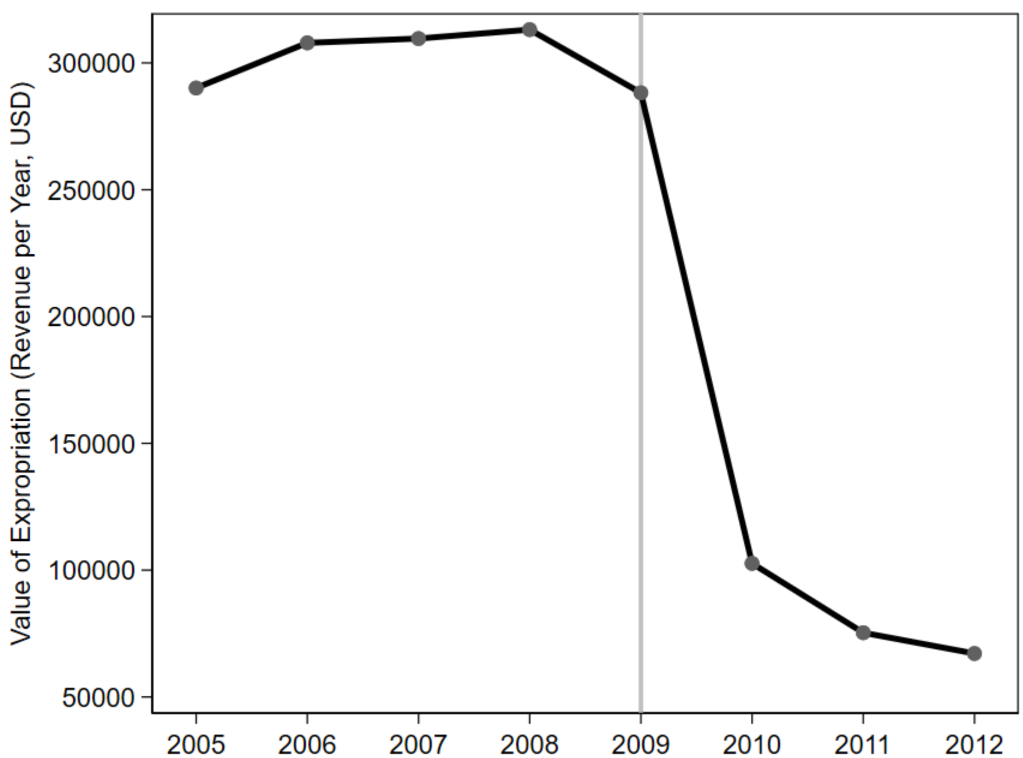
Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020) robust and efficient estimator. The dependent variable is an indicator for: whether the FDLR attacks village i in year t with the intention to conquer. The year 2009 is the omitted category. 1,544 village-year observations were used in the estimation. There are 193 and 168 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.

Figure F.10.: Event Study Estimates, Attack with Intention to Pillage



Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020) robust and efficient estimator. The dependent variable is an indicator for whether the FDLR attacks village i in year t with the intention to punish. The year 2009 is the omitted category. 1,544 village-year observations were used in the estimation. There are 193 and 168 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.

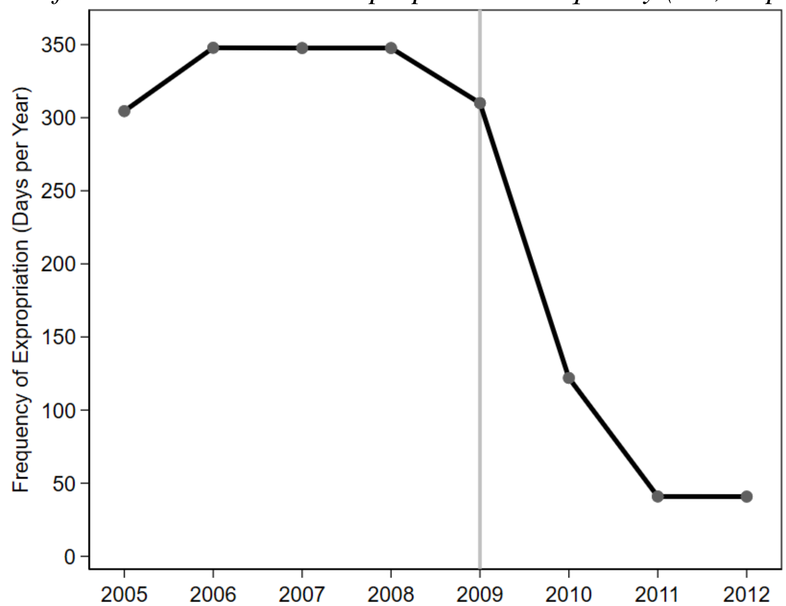
Figure F.11.: Mechanism—Income Effect



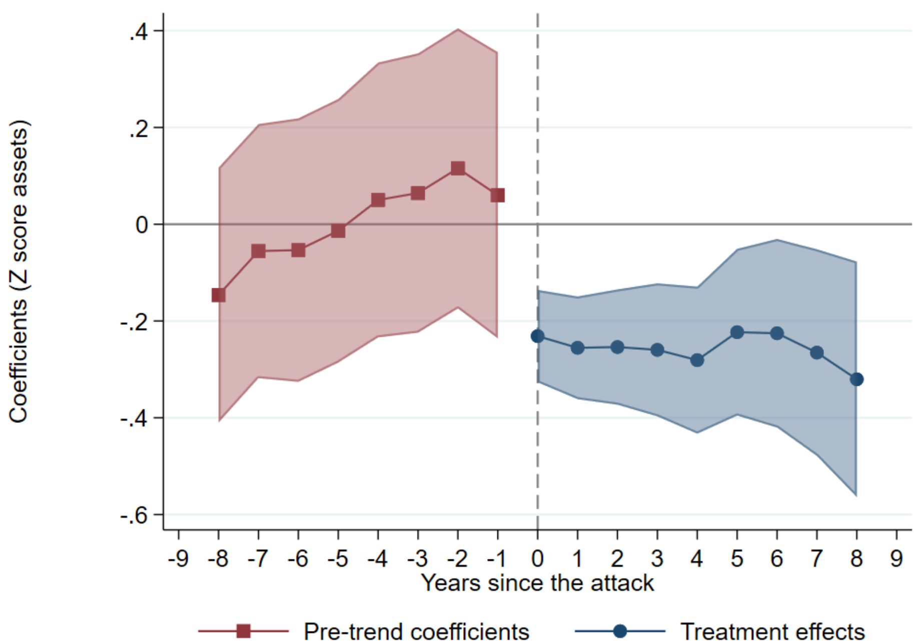
Notes: This figure shows the total income in USD generated by the FDLR in the FDLR state, through taxation and through pillage confounded.

Figure F.12.: Mechanism: Expropriation, Value of Future Expropriations—Validation

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



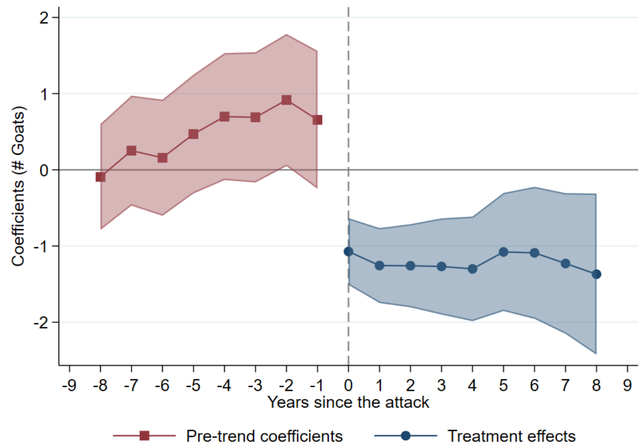
B. Effect of One Pillage on Household Assets



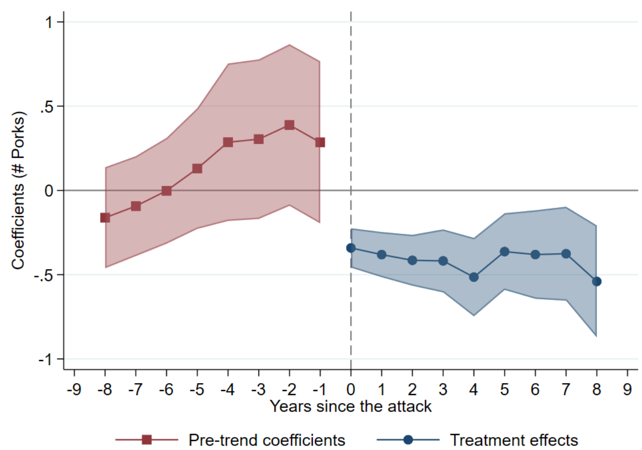
Notes: Panel A shows the trend of frequency of FDLR expropriation in the FDLR state. The frequency of expropriation is the sum of pillaging and tax payment events per year. Panel B shows the coefficients from Borusyak et al. (2020)'s efficient and robust estimator for the effect of a pillage on the household's stock of cattle, standardized to mean zero and standard deviation of one. Figure F.13 shows these coefficients separately for when the dependent variable is the number of goats, porks, cows, respectively.

Figure F.13.: Inter-temporal Trade-offs: Effect of a Pillage on a Household Cattle Stock

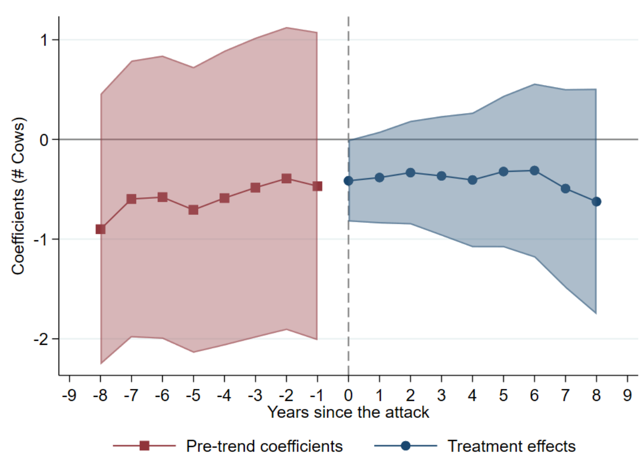
A. On Number of Goats Owned



B. On Number of Porks Owned



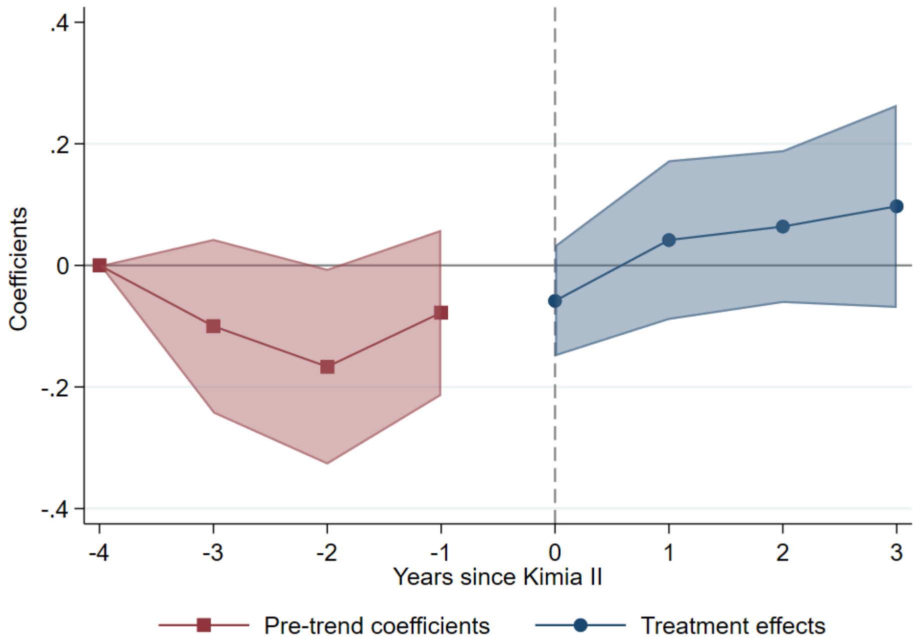
C. On Number of Cows Owned



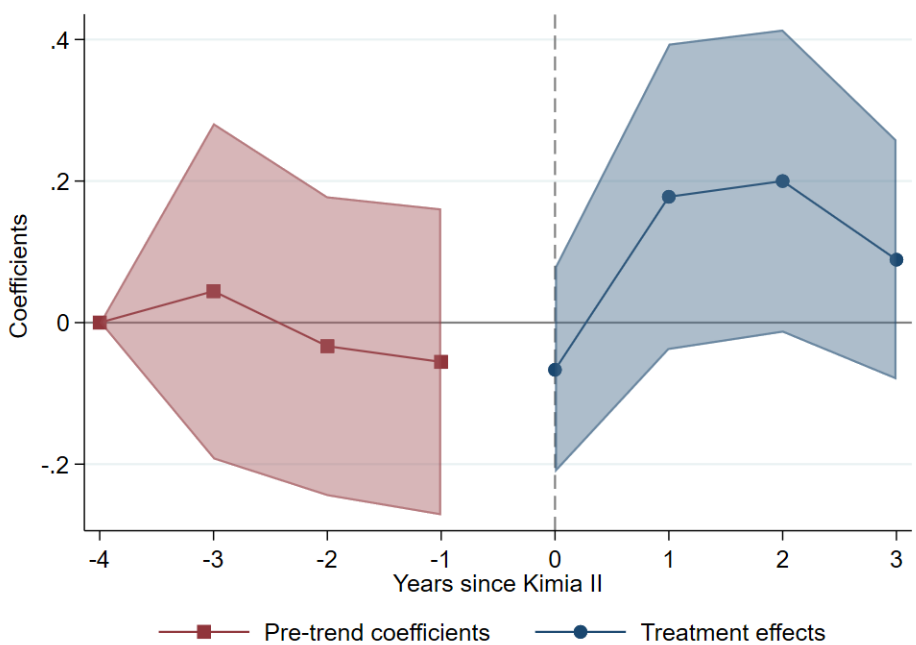
Notes: This figure shows the coefficients from Borusyak et al. (2020)'s efficient and robust estimator for the effect of a pillage on the household's stock of goats, porks, and cows.

Figure F.14.: The Role of Frequency of Expropriation: Effect by Whether the Village is Accessible by Foot

A. Attacks by FDLR in Villages Only Accessible by Foot



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



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 only by foot. The R-squared is 25.2%. In Panel B, the dependent variable is an indicator or whether the village is accessible by car or motorbike.

Table F.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***	-0.21**	0.00	-0.62***	-0.71***	-0.34***
	(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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

Table F.3: Effect on Household Savings

	Cattle				
	Net s^*	*Purchase*	*Sale*	Theft τ^p	Stock $\bar{S}$
	(1)	(2)	(3)	(4)	(5)
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. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5% and 1% levels respectively.

APPENDIX G: MATHEMATICAL APPENDIX

G.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 $\sum_{t=0}^{\infty} \delta^t p^t u(\tau_t - s_t)$, 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 territorial control, 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.[52] Similarly, the ability to frequently expropriate can sustain state functions by the bandit.

G.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.

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*.*

[52] 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.

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.

Q.E.D.

APPENDIX H: APPENDIX BIBLIOGRAPHY

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