Monopoly of Taxation Without a Monopoly of Violence: The Weak State's Trade-Offs From Taxation

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Abstract— This study presents a new economic perspective on state-building based on a case study in the Democratic Republic of the Congo's hinterland. We explore the implications for the state of considering rebels as stationary bandits. When the state, through a military operation, made it impossible for rebels to levy taxes, it inadvertently encouraged them to plunder the assets of the very citizens they previously preferred to tax. When it negotiated with rebels instead, this effect was absent, but negotiating compromised the state's legitimacy and prompted the emergence of new rebels. The findings suggest that attempting to increase taxation by a weak state in the hinterland could come at the expense of safety in the medium term and of the integrity of the state in the long term.

This study is the outcome of a ten-year collective endeavor, in the course of which extremely significant contributions were made by Amani Lameke, Gauthier Marchais, Jean-Paul Zibika, and the members of Marakuja Research. The authors are irreparably indebted to each of them, especially to Gauthier Marchais. Based on a result first presented in Sánchez de la Sierra (2014), Chapter 2, which drew on data collected by Marchais, Mugaruka, Sánchez de la Sierra, Zibika during 16 months of fieldwork in 2012-2013, the analysis in this paper also draws on data collection led by Amani Lameke, Henn, Marchais, Mugaruka, and Sánchez de la Sierra in 2015 and 2022 and conducted by Marakuja (nonprofit co-founded by Henn, Mugaruka and Sánchez de la Sierra) and is funded by the ICTD, Newcastle University Business School, and the Pearson Institute. Eloisa Alvarez-Uribe, Christian Bazuzi, Desire Basibuhe, Ellie Chen, Erica Hogan, Christelle Inema, Akanksha Kala, Emmanuel Kandate, Frederic Koleramungu, Eustache Kuliumbwa, Simeon Lukeno, Floribert Lwaboshi, Evariste Seba, Eddy Swiate, and Yinghui Zhou provided excellent research assistance. Sánchez de la Sierra thanks Christopher Blattman, Pierre-André Chiappori, Donald Davis, Macartan Humphreys, Suresh Naidu, Bernard Salanié and Noam Yuchtman for advice, and we thank Ana Antolin, Reza Arabsheibani, Nils Braakmann, Ian Gregory-Smith, Lena Janys, Joseph Melkonian, Nathan Nunn, Harry Pickard, James A. Robinson, Jake Shapiro, Shivan Viswanathan, Matthias Weigand, Diego Zambiasi, participants at ASSA and 2022, Newcastle University, Harvard's 2023 ECON2623 students, and three anonymous referees for invaluable comments. We use the term rebels to refer to non-state armed actors. Our use of the term does not imply acceptance of its normative baggage. While their very existence challenges the state's *de jure* claim to a monopoly of violence granted by a coalition of organizations of violence often called the international state system, non-state armed actors' objectives rarely include rebellion against the state. A recent Congolese law, which recognizes "mayi-mayi" fighters as "wazalendo" (patriots) is an example. All that is relevant is that they can monopolize violence.

"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."
Interview with villager, South Kivu, 2013.

1. INTRODUCTION

Many states struggle to provide basic public goods and even to protect human rights. Sometimes conventionally referred to as "weak" or "fragile" states, they represent a significant development challenge and host 24% of the world's population (OECD, 2022). Motivated by the notion that taxation is the "hallmark of the state," a growing literature has examined the various ways in which such states can improve the efficiency of tax collection (e.g., Weigel 2020; Bergeron et al. 2021), with the hope that more fiscal revenue could support improved provision of public goods. These efforts have tended to focus on testing technical improvements of the effectiveness to tax on the margin and, importantly, on the administrative infrastructure within the territories that these states control.

However, throughout history, *de facto*, states often did not exert control over significant portions of the territory over which they have *de jure* sovereignty. These areas, commonly denoted the "hinterland" (Herbst, 2011), often fall under the authority of nonstate armed actors instead (henceforth, *rebels*), who sometimes provide stability. These areas can also be valuable; thus, the lack of *de facto* control over them represents a significant loss of fiscal revenue for the state. For example, in the eastern Democratic Republic of the Congo (DRC), recently, rebels controlled and taxed one of the largest and mineral-rich districts (Stearns, 2013). Yet, given the possible inadvertent effects on the rebels' behavior, it is not obvious that asserting the state's territory in those areas is socially desirable.

This paper aims to empirically analyse the effect of state-building attempts to increase the ability of the state to tax in areas it does not *de facto* control, while accounting for the source of the rebels' stability. We motivate our analysis by examining the experience of the postcolonial African states that attempted to regain the right to tax over their hinterland. By the second half of the twentieth century, states generally did so through two strategies.

On the one hand, they have used military power to reclaim their right to tax. This strategy resembles in many ways the experience of the formation of the modern European state (Tilly, 1985). However, it also creates an important challenge: the rebels' *ability to tax* the areas they control is sometimes believed to be their very incentive to *refrain* from arbitrary and violent expropriation of the assets owned by those they tax—the idea that the rebels are "stationary bandits" (Olson, 1993). If this logic is present, unless the weak state is able to fully detain the rebels, it could inadvertently incentivize plundering. This is not a mere hypothetical possibility. In an interview we conducted with a villager from Mwenga, DRC, in 2013 after a campaign that made it impossible for the rebels to tax his village, he pointed out: "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." While the notion of stationary bandit is well-understood and has been shown to apply to rebels in the DRC itself (Sánchez de la Sierra, 2020), its implications for state-building remain to be explored. Using publicly available data, we provide suggestive evidence that, consistent with this possible inadvertent effect, the aftermath of military attempts to regain territory in Sub-Saharan Africa has unusually high violence by rebels on civilians.

On the other hand, states have sometimes negotiated with the rebels. The nature of the state-rebel negotiated agreements may vary, but the agreement often involved concessions, such as lucrative positions in the state and the army. This strategy of negotiating instead of confronting has historical precedents outside of Europe (Newbury, 2000). However, this strategy is plagued by commitment problems that are also not new: What ensures that the state and the rebels can commit to the promises they make during the negotiation? What ensures that the state can commit not to negotiate in the future if other rebels challenge the state? These commitment issues may undermine the promise of this approach, incentivising future rebellions. The consequences are illustrated in the sack of Rome in 410 (Norwich, 1988). Notwithstanding those challenges, we document that, unlike for military operations, the aftermath of intra-country peace agreements is typically not violent for civilians.

A limitation of these cross-country correlations is that, without disaggregated data, it is unclear if they are causal. To address this challenge, our analysis then uses the DRC as a case study. The DRC is the archetypal example of a modern state that is unable to control large parts of its territory, and that has tried various strategies to regain such control. The two predominant strategies the DRC state has attempted also include military efforts to replace rebels by force and negotiation (Stearns et al., 2013). We analyze the effect of the military operation known as Kimia II, conducted in 2009, which aimed to gain territory over the Forces de Libération du Rwanda (FDLR), notably in the Basile Chiefdom. There, it is considered one of the most militarily successful operations and thus provides the opportunity to examine how, when successful at pushing back the rebels, such a strategy alters the incentives of the rebels. We then compare this approach with one of the most significant peace treaties since the DRC's independence from Belgium, the 2004 Sun City agreement.

We first analyse the effect of Kimia II on the behaviour of the rebels targeted. We take advantage of three features of the setting. First, the army quickly ousted the rebels, making it impossible for the rebels to tax. Second, Basile is surrounded by a hilly forest, where rebels could endure—a common feature of the hinterland—and the state was too weak to provide security in newly acquired villages (Stearns et al., 2013). Third, Kimia II was a concession to Rwanda, making reverse causality unlikely. Kimia II is thus well suited to isolate the implications of Olson's (1993) notion of stationary bandits for state-building.

We analyze the *effect* of Kimia II in four steps. In a first step, we provide evidence that Kimia II *caused* a rise in reported attacks by the FDLR against the villagers they previously taxed. In a second step, we characterize this effect. We find that the increase in reported attacks purportedly perpetrated by the FDLR is unlikely to reflect an increase in reporting, and reflects instead an increase in true attacks; we find that this increase is specific to the targeted FDLR factions, and is absent for any other rebels in the east of the country; and we find that the effect lasts between four and five years after Kimia II. Drawing on qualitative interviews with perpetrators, we document that after that period, the state consolidated its monopoly of violence, and the FDLR factions migrated from the forest where they had settled to faraway places in search of income. In a third step, we provide evidence for the mechanisms. Consistent with the qualitative interviews we conducted with both the villagers and the rebels, we provide empirical evidence that this effect is driven by violent theft operations (henceforth, *pillage*), not retaliation or conquest, and that its timing and location is consistent with Kimia II destroying the rebels' incentive to protect a village they can tax. In a fourth step, we analyze implications for welfare. For each targeted village, as a by-product of pillages by the FDLR, Kimia II increased kidnappings, plundering, and rape by 24 pp., 10 pp., and 18 pp. *per year*, respectively, for the duration of the rise in pillages. At the same time, for each targeted village, it *permanently* reduced the yearly tax from $17,902$ USD to $10,620$ USD, which was now informal tax paid to state officers instead.

A priori, it seems hardly surprising that, when rebels are pushed out, they fight back (e.g., Dell et al., 2018; Lessing, 2017). Indeed, in war, it is common for the losing side to target the location where the enemy seeks refuge in order to weaken them. A purported quote from a US major during the Vietnam War, reported by journalist Peter Arnett, highlights this logic: "In order to save the town, we had to destroy it." The US military purportedly justified destroying the town of Ben Tre as a means to uproot the Viet Cong. However, our findings are that the rebels neither fight back nor retaliate. They even avoid villages harboring the army. Instead, they engage in theft operations, using informants to target the village's wealth. This response can instead be rationalized by a relationship where the ability to regularly expropriate citizens had dis-incentivized the rebels to arbitrarily steal from them. This incentive is often not "legible" to the state (Scott, 1998), who, in our context, did not anticipate it. It exemplifies the argument by Acemoglu and Robinson (2013), that the impact of a policy depends on its influence on political equilibria. Interestingly, this implies that allowing rebels to tax citizens can be a source of the citizens' own protection.

We then contrast this effect to that of the peace treaty. While it included most active rebel groups at the time, many villages were not under the control of rebels, or were controlled by rebels not included in the treaty. Thus, the treaty affected only a subset of the villages (henceforth, *affected villages*). Consistent with the existing historical evidence (Stearns et al., 2013), we find that while the treaty expanded the state's control and tax revenue in the affected villages, in contrast to Kimia II, it did not increase violence in the short or medium-run in these villages. However, through survey and qualitative data, documentation, and past research, we provide evidence that the treaty led to the proliferation of armed groups, parallel rebel command structures in the army, mutinies, and lower state legitimacy in the medium- to long-run. This is consistent with the interpretation that this strategy is plagued by limited commitment problems that undermine it. While the treaty takes place in a different time period and affects a different set of armed groups, it nonetheless provides a useful possibility result against which to benchmark the disruption created by Kimia II.

Extensive research in economics has investigated the factors that give rise to state functions (e.g., Allen et al., 2023; Mayshar et al., 2021). Building on this body of knowledge, our study makes three contributions. First, it provides empirical evidence that the time horizon of expropriation is a requirement for armed actors to develop an "encompassing interest," as proposed by Tullock (1974) and formalized in McGuire and Olson (1996). Our study relies on Olson's (1993) notion of "stationary" and "roving" bandits to rationalise the response of the rebels. Olson's (1993) argument is that a longer time horizon in which a bandit can expropriate a location can induce the bandit to develop an encompassing interest in that location, lowering its expropriation rate. Sánchez de la Sierra (2020) finds that an increase in the value of what can be taxed makes it more likely to have a stationary bandit. However, the value of what can be taxed is a bundle: it will also create contestation, tending to *decrease* the time horizon—that is, it is not a test of Olson's (1993) stationary bandits' *raison d'être*. Our study isolates it by leveraging features of our context that are common to weak states: the state has military superiority, hence can shrink the bandits' horizon of taxation; there is a hinterland, which allows stationary bandits to endure as roving bandits. Second, we introduce the notion that the emergence of the state can have adverse effects on the behavior of bandits. In our context, when the state establishes territorial control, bandits become nomadic, relocating to *hard-to-control* areas where they consume through plundering (Scott, 2009). The introduction of hierarchy disrupts their encompassing interest, leading to more expropriation in line with Shleifer and Vishny (1993), which reduces the returns to investment. These dynamics may intensify the challenges associated with escaping the "civilisational paradox" (Dal Bó et al., 2022), and complement Mayshar et al. (2021), which highlights the incentivising role of hierarchy for investment. Third, our findings offer empirical support for existing theories on conflict and state formation. The violence observed in a region rich in minerals aligns with the concept of rapacity as described by Dincecco et al. (2022). The emergence of the FDLR state in a mineral-rich area surrounded by hilly forests exemplifies the role of environmental circumscription for state formation (Carneiro, 1970), for which Schönholzer (2023) presents a test. Furthermore, the challenging accessibility of a rich area resembles the conditions postulated by Dal Bó et al. (2022) that facilitated the rise of Ancient Egypt on a larger scale.

These findings have implications for state-building. First, we find that bargaining can be successful in the short-run to regain territory, but that, if the state cannot commit to future negotiations, and if rebels cannot commit not to use the state for their interests, bargaining can weaken the state's integrity in the long-run. Second, our findings challenge applications of weberian conceptions of the state as a territorial monopoly of (legitimate) violence (Weber, 1946). Judged from the territorial lens, Kimia II was a success: the state reclaimed the territory and collected taxes there, albeit informal ones. But when we de-couple territorial control, which allowed the state to assert its *monopoly of taxation*, from *monopoly of violence*, the picture becomes murkier: the rebel group transforms from stationary to roving and starts plundering, causing a range of assaults to dignity. Consistent with existing arguments about the pitfalls of this notion in postcolonial Africa (Herbst, 2011), this finding underscores a limitation of the notion of territorial sovereignty—popularized in Tilly (1985) and influencing current legal doctrine towards state-building in postcolonial Africa.

By the second half of the 20th century, many postcolonial African states were on the brink of collapse (see, e.g., Zartman 1995; Bierschenk 2010; Bates 2008; Van de Walle 2001; Blundo 2006). States had generally weak capacity by international standards. Many states lost control (or never had such control) of areas over which they had *de jure* sovereignty to military organizations that did not. These areas were often in the "hinterland," hard to reach for the state and offering a safe haven for rebels (Herbst, 2011).

In response, governments developed two strategies to assert their states' territorial authority. On the one hand, when they had the means, they used military power to oust the rebels, reclaiming some of their hinterland. The success of this approach depends on the state first being able to mobilize resources to deploy sufficient military power to regain the territory they had lost. It is also not clear that this approach is even desirable, as the state is often unable to provide security after retaking territory, as rebels often simply hide in the hinterland. This issue is pervasive: for example, it is present for Al-Shabaab in Somalia, but also in the fight against the Taliban in Afghanistan, or against armed groups in Colombia. On the other hand, governments negotiated, offering the rebels spoils from the state in exchange for retaking control of the territory over which they had sovereignty.

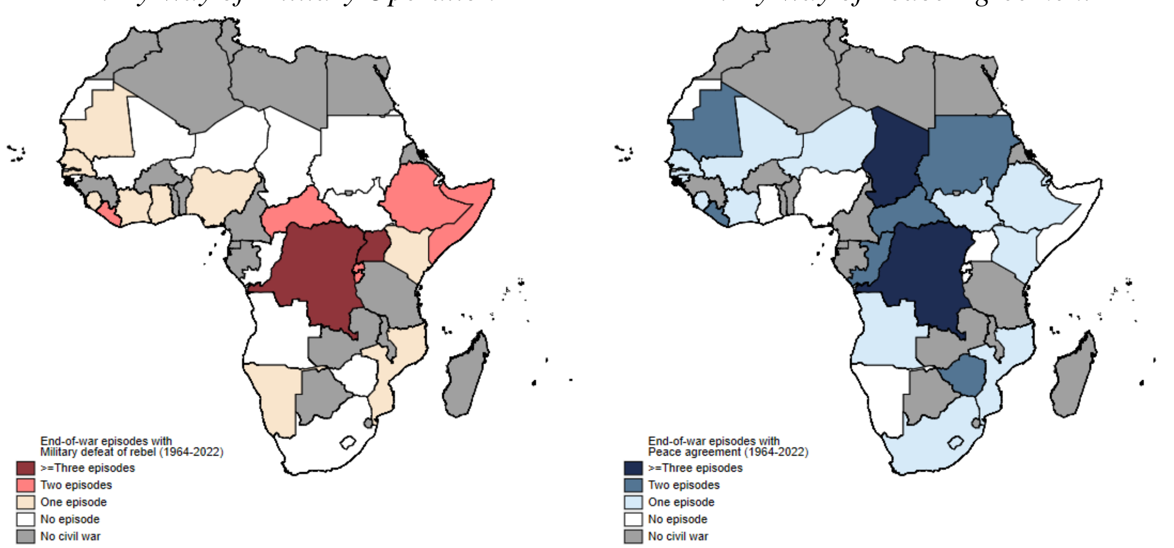
Since independence, civil wars ended with the state regaining its territory, except in rare cases (e.g., Sudan). Details of the end of these wars allow us to provide some numbers of these strategies. Figure I uses data we consolidated covering all civil wars in Africa since 1961 to document the prevalence of either strategy. We list all these wars in Section 1 of Authors' Personal Supplemental Appendix I. Most countries experienced a civil war, but the strategy by which the state regained territory varies. Some wars ended with military defeat, such as the First Liberian Civil War and the Ugandan Civil War. Others in peace agreements, such as Mozambique, Sudan, Sierra Leone, and the Second Liberian Civil War. The DRC stands out as having among the largest number of endings of either type.

A few case studies motivate our question. First, some of the most significant wars that ended in military defeat were followed by rebel violence. For example, the First Liberian Civil War (1989–1997), which ended with the government's military success—even if a peace agreement was signed in Abuja in 1995 for the end of hostilities by 1996 (Hoffman, 2007)—was followed by violence as Taylor entered the capital by force in 1997; within two years, the second civil war broke out. For example, in the Ugandan Civil War (1980–1986), which ended when the rebels took the capital establishing a government, various rebel factions remained active and continued to exert violence thereafter. Second, on the other hand, some of the most significant wars that ended in peace agreement were not followed by such violence. Such was the case of Mozambique's 1977–1992 civil war, which ended with the Rome General Peace Accords and was subsequently followed by a long period of peace until 2013; the Second Sudanese Civil War of 1983–2005, which ended with the Comprehensive Peace Agreement of Nairobi in 2005; the Second Liberian Civil War (1999–2003), which ended with the Accra Comprehensive Peace Agreement in 2003, followed by riots but no additional violence by rebels (BBC, 2018); or the Sierra Leone Civil War (1991–2001), which ended in a peace agreement after a military defeat, where there was no subsequent rebel violence.

For the subset of wars that ended after 1997, using ACLED (2022) records of rebel violence against civilians, it appears that, when a state regains territory by military defeat, rebel violence tends to increase or stay high, and by peace agreement, it does not. These correlations are reported in Figure B.1 in the Online Appendix.

Figure I.: Postcolonial African States' Endings of Civil Wars

A. By Way of Military Operation *B. By Way of Peace Agreement*



Notes: This figure presents the incidence of postindependence Sub-Saharan civil wars that ended with the state militarily regaining territory (Panel A), and by peace agreement (Panel B). *Source*: authors' reconstruction using historical publications. Section 1 of Authors' Personal Supplemental Appendix I lists all wars.

However, neither military nor bargained attempts of postcolonial African states to regain their territory are confined to the end of civil conflicts. To make progress on this question, we gathered the universe of events recorded in ACLED (2022) in which a government militarily retakes territory from the rebels, in all existing years in Africa (1997–2022); there are 3,432 such recorded events in Africa since 1997. We also classified all peace agreements signed in intra-state conflict with rebels, recorded in PA-X (2022); there are 555 recorded intra-state agreements in Africa since 1990. At the start of 2023, the data, introduced in Bell and Badanjak (2019), covered the world from 1990 to April 2022, amounting to over 1,959 agreements in 140 peace processes. While most African countries are affected by these events, the DRC stands out. Figure B.2 shows the incidence of intra-state peace agreements and events of government regaining territory. The episodes covered involve rebel bastions in the hinterland, underscoring a pervasive feature of weak states' attempts to assert their monopoly of violence: rebels have access to hard-to-reach areas where they can hide and endure after military operations. We examined the effect of regaining territory by force on rebel violence against civilians in ACLED (2022). The analysis is presented as country-year event studies in Figure B.3. It presents a moderate increase in rebel violence following such events, but no effect on violence by the state. We then analyzed violence around peace agreements with the rebels. In contrast, these events are not followed by rebel violence.

This correlational analysis offers a comparative perspective of the challenges of weak African states to assert their territorial control. However, without dis-aggregated data on the targeting and motives for the violence, it is unclear whether these correlations are causal, or what the mechanisms are. To isolate the causal effect of such policies and their mechanisms, in what follows, we narrow in on two such policies implemented in the DRC.

3. BRIEF BACKGROUND OF THE DRC'S EFFORTS TO ASSERT THE STATE

Faced with vast areas of its territory under the control of armed groups since 1998, the DRC has tried two major approaches to regain territory: a) peace treaties with the rebels in exchange for integration into the army; b) military operations (Stearns et al., 2013).

The largest of such treaties is the Sun City peace treaty, signed in 2004 by the DRC's government to end the Second Congo War (1998–2004). The treaty allowed various armed groups to be integrated into the Congolese army. While implementation of the peace treaty was met with some difficulties, including some armed groups refusing integration (see, for example, Stearns et al., 2013), the government regained half of the country without using or triggering violence. Other peace agreements had smaller ambition and low credibility.

The DRC's government also tried to militarily regain parts of its territory that remained under the control of armed groups. One of the most militarily successful examples is Kimia II. In March of 2009, the national army (by then named Forces Armées de la République Démocratique du Congo, henceforth *FARDC*) and the UN launched Kimia II to oust the rebel FDLR factions settled and taxing in parts of South Kivu since 2005, notably in the Basile Chiefdom—a 3,113 km^2 area. By 2009, FDLR factions controlled *most* of Basile (henceforth, "FDLR state"). It is estimated that 22,000 Congolese army soldiers and 8,000 UN soldiers faced at most 4,000 fighters of the FDLR (Florquin and Debelle, 2015). Militarily, Kimia II in Basile is the most successful of anti-FDLR operations, and the only one with detailed dis-aggregated data. Prior to Kimia II, and after the surrender of the Congres National Du Peuple, in January of 2009, the Congolese armed forces and the Rwandan army launched a joint operation against the FDLR in North Kivu, Umoja Wetu. The operation was a military failure, and the Rwandan army quickly withdrew. Three features of Kimia II in Basile make it suitable to isolate the effect of the ability to tax on violence:

a) Fighting asymmetry. The battalions of the army in Kimia II were better equipped and much more numerous. Reflecting this asymmetry, by December 2009, the FDLR retreated from the villages targeted in Basile without a fight. Kimia II ended in December 2009, having ousted the FDLR from Basile, making it impossible for them to tax the targeted villages in Basile. See Vlassenroot and Verweijen (2017) for more details on this asymetry.

b) No consolidation of the state's monopoly of violence. Basile is surrounded by the hilly forest of Itombwe, where the factions resettled, maintaining semi-autonomous command structures (Florquin and Debelle, 2015) in nearby camps. Maps in Vogel (2021) show that the FDLR no longer exerted territorial control thereafter. From those camps in the forest, they were able to regularly conduct violent theft operations on the villages that they formerly controlled—that is, the army did not consolidate a monopoly of violence (see, e.g., Sawyer and Van Woudenberg 2009 and Levine 2014, Ch. 8). Perpetrators were the same individuals that used to tax. For example, ICC (2012), p. 80 note: "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."

> They dislodg[ed] them from their main revenue bases and forc[ed] them to withdraw into isolated zones. On the other hand, they led to rampant insecurity [. . .] in part as the FARDC did not manage to effectively occupy and secure the zones formerly controlled by the targeted groups [. . .] the FDLR continued to operate from the fringes of its former strongholds, launching attacks to [. . .] compensate for lost sources of income. (Vlassenroot and Verweijen, 2017, p. 113)

c) External causes. The decision to conduct Kimia II against the FDLR in 2009 was the outcome of a high-level concession from the DRC's government to the Rwandan government, in relation to a conflict in North Kivu, a different province.

There are extensive anecdotal examples of the response by the FDLR, but there has been little in the way of systematic quantitative analysis to isolate the spatially targeted effects of Kimia II and analyzing the motives for FDLR violence. Complicating this analysis is the fact that the local response of the targeted factions was preceded by a nationwide response by the whole FDLR. The FDLR leadership issued military instructions for deliberate attacks on civilians, presumably to exert pressure on the central government (e.g., Stearns et al., 2013; Human Rights Watch, 2017; UNSC, 2009). This response, which was nationwide and short-lived, affected villages in North Kivu, far from Basile.

In what follows, we take advantage of Kimia II's timing and targeting to estimate its effect on the behavior of the targeted FDLR factions. André and Platteau (1998) and Authors' Personal Supplemental Appendix I, Section 2, provide details on the Rwandan genocide.

4. 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. 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. The historical village data for this paper was introduced and described in Sánchez de la Sierra (2014, 2020). In South Kivu, data were collected between June 2012 and September 2013. 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. We implemented the same procedure in North Kivu in 2015.

The sample comprises interviews conducted in 1,537 randomly selected households, in 239 villages of South and North Kivu. It comprises data from 144 households and 36 village experts in the Basile Chiefdom, sampled from 18 villages, constituting 324 village-year observations. The remaining 1,393 households are from outside Basile.

The data include a detailed description of attacks on the villages since 1995, including: a) perpetrators' affiliation; b) purported *motivation* (predominantly pillaging, punishment, or conquest); c) *actions* taken during attack (predominantly theft, abduction, rape, deaths, items stolen). Answers were not prompted, but researchers had a list of non-mutually exclusive options, including "other" followed by text. Sections 3 and 4 of Authors' Personal Supplemental Appendix I discuss data reliability and variable construction.[1]

We refer to the years 2005–2012 as the *Quasi-Experimental Window*, which includes the years of the FDLR state (2005–2009) and its collapse after Kimia II (2010–2012). We refer to the villages of the district of Shabunda as the *Confounding Villages*. Kimia II was followed by another military policy, the *Regimentation*, which, in 2011, repositioned Congolese army troops from Shabunda in the urban barracks for restructuring but failed to send them back for various months. The regimentation has been documented to create a security vacuum in Shabunda that led to the entry of other FDLR factions, originating from the northern area of Bunyakiri (Stearns et al., 2013). We refer to the rest of villages as the *Quasi-Experimental Villages*. We compare the confounding and the quasi experimental villages' pre-Kimia II characteristics in Table B.1. We refer to the quasi-experimental villages in the quasi-experimental window years as the *Quasi-Experimental Sample*.

5. EFFECT OF KIMIA II ON REBEL VIOLENCE AGAINST CIVILIANS

5.1. *The Causal Effect of Kimia II*

Life before Kimia II and Characteristics of Kimia II

First, our data suggest that, prior to Kimia II, the FDLR acted as "stationary bandits." Indeed, the FDLR expropriated through poll taxes, toll fees, taxes on the market in 94%,

[1]The researchers were graduates in nutrition, pedagogy, economics, and political science, from the Université Officielle de Bukavu and Institut Superieur Pedagogique de Bukavu. In addition, all underwent five months of training to data collection by the PI in 2010 (Humphreys et al., 2019) and had 3 years of data collection experience in the same province for that project. Furthermore, we provided four months of training in qualitative techniques, memory recollection, and the practice of connecting with respondents, which included one week of piloting.

83%, 28% of their villages, respectively.[2] Each household paid, on average, 64.97 USD yearly to the FDLR in taxes.[3] They ran justice and fiscal administrations in these villages. The frequency of violent operations on the village (henceforth, *attacks*) by any armed actor (6%) was half of that in the rest, suggesting the FDLR provided security. Figure II, left panel, maps the FDLR state villages vs. the rest in the year prior to Kimia II. Section A.1 presents qualitative evidence supporting these observations. For example:

> [T]he chiefs of these villages got together and decided to go find the leaders 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. (Qualitative report for the village of Pohe, Mito and Wimbi, 2013)

Second, our data suggest that the villages' remoteness enabled the FDLR state. Indeed, none of the villages in the FDLR state were accessible by car; the closest road was 1.29 km farther than the rest. They were 29 pp. less likely to have phone coverage. We account for these differences in the analysis (see Table B.2). Our qualitative data support this:

> These FDLR came to settle in the village because it is very remote in the forest and far from the city [...] they became absolute masters in this village. (Qualitative report of the village of Musingi, 2013)

Third, the data offer support to two features of Kimia II that are crucial for the analysis, which have been documented based on anecdotal evidence (Stearns et al., 2013):

a) Fighting asymmetry. The data confirm that the Congolese army's military superiority was undisputed. Indeed, Figure II, right panel, shows that the army ousted FDLR from most villages by 2011. Figure III shows that, by 2011, the FDLR had lost 90% of the Basile villages they controlled, most of which were taken over by the Congolese army or left uncontrolled. Emphasizing the undisputed military superiority of the Congolese army, the figure also shows that this shift in territorial control occurs without fighting on the side of the Congolese army in Basile. The combination of a shift of territory to the Congolese army and no fighting by the army is consistent with the qualitative evidence suggesting that the Congolese army had undisputed military superiority in this operation. It also underscores that the "treatment" is not bundled alongside with violence by the Congolese army.

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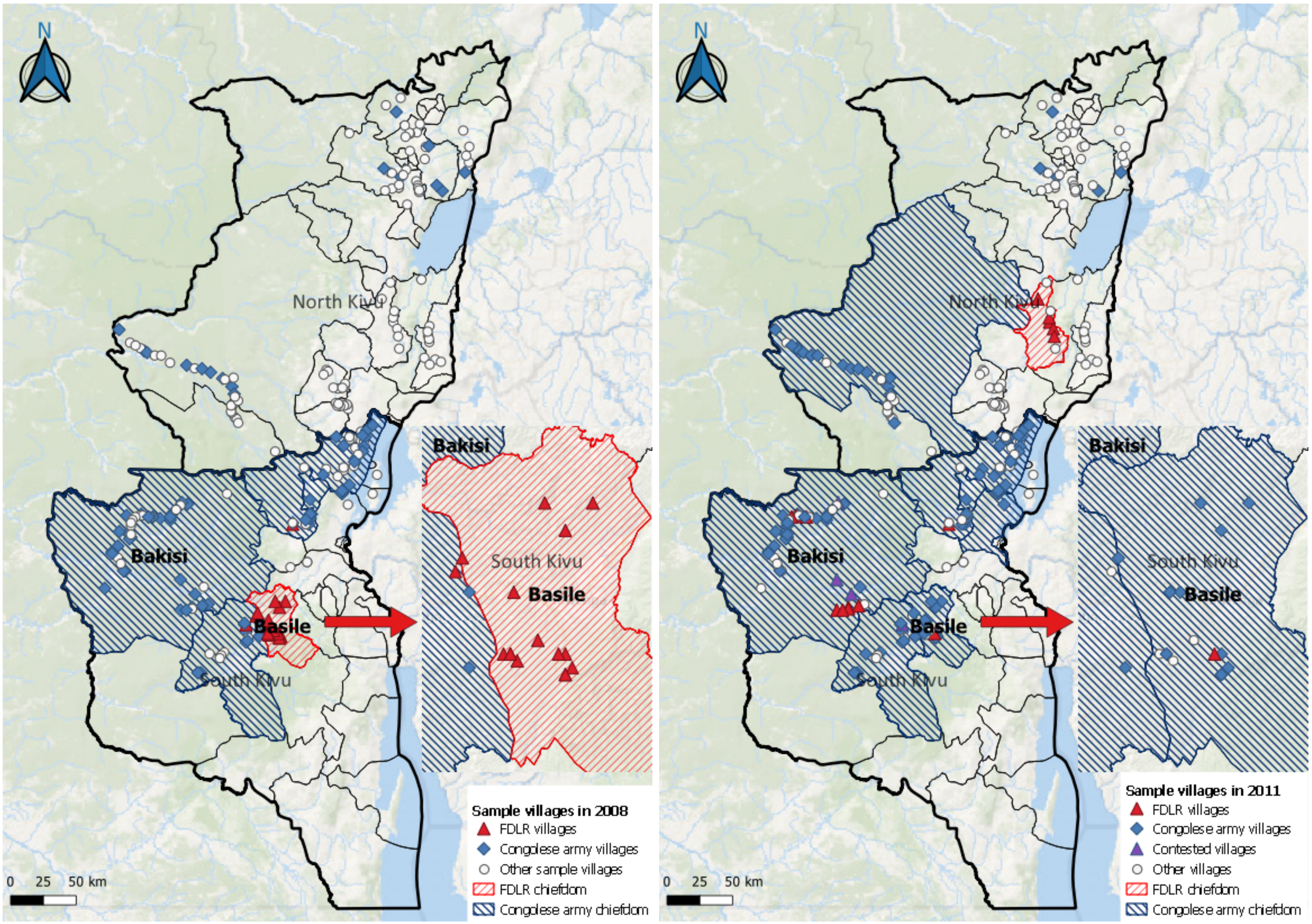
[2] The quantitative description draws from the statistics presented in Table B.1.

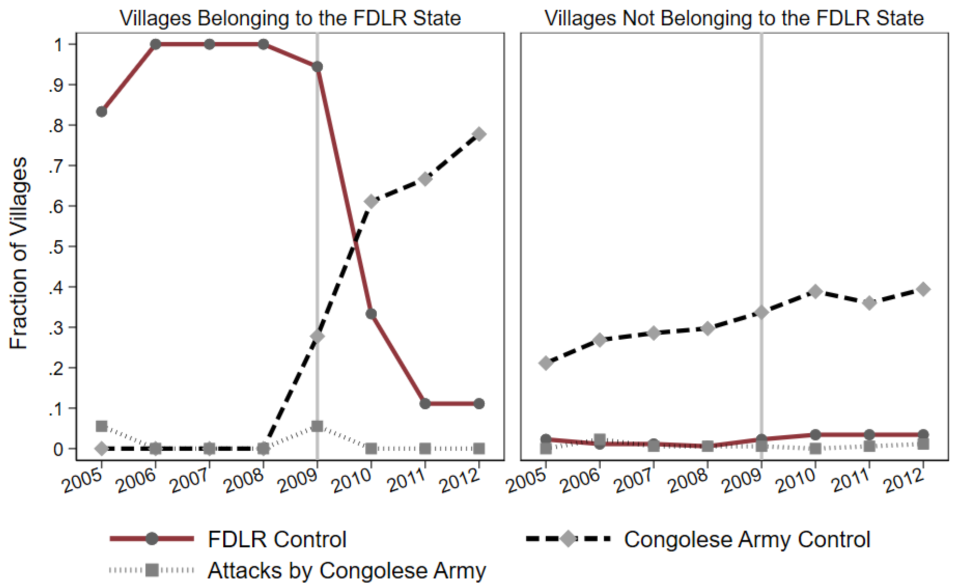
[3] This amounts to 25.5% of 2009 national p.c. income in USD of 2022 (World Bank, 2023). Using alternative estimates of income in these areas, Section 5.4 suggests that a more realistic window is 7% to 11%.

Figure II.: The FDLR State and the Rest, before and after Kimia II



Notes: This figure presents villages controlled by the FDLR in the sample, covering the provinces of North Kivu and South Kivu. The left panel does so for 2008, which is the year before Kimia II. The right panel does 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 and blue striped areas indicate the Chiefdoms where the FDLR, or the Congolese army, holds more than 50% of villages in the sample, respectively.

Figure III.: Kimia II Campaign's Territorial Success



Notes: This figure presents the fraction of villages controlled by the FDLR, by the Congolese army, and in which the Congolese army perpetrates an attack, over the years.

Figure B.4 zooms into Basile. The figure shows that, in response to the Congolese army's advance over the villages they formerly controlled, the FDLR factions resettled in nearby bases in the forest of Itombwe. The figure also paints a picture of the FDLR behavior prior to Kimia II that is consistent with the paper's main argument: it shows that, before Kimia II, the FDLR controlled many remote villages and pillaged the villages it did not control. This is consistent with the qualitative data on life before Kimia II provided in Sections A.1, A.2. While only descriptive, it is consistent with the paper's main argument that the FDLR factions had developed an encompassing interest but only in the villages it could tax.

b) Monopoly of violence. The Congolese army units had limited presence after asserting the territory. Figure III shows that they still failed to occupy 20% of the FDLR state villages. Section A.3 presents additional qualitative evidence about the inability of the army to consolidate power. Even when they were present, they often lived in distant barracks.

> 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. (Qualitative reports of Tubindi and of Mito, 2013)

Figure IV, Panel A, documents that, coinciding with the expansion of the FDLR and settling of the FDLR in the FDLR state, violence decreases in the FDLR state leading up to 2007. After Kimia II, FDLR attacks skyrocket in the FDLR state villages.

To analyze the effect of Kimia II, we estimate the following equation using Borusyak et al.'s (2023) robust and efficient estimator.[4] Let i,t index village and year, respectively:

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

where α_i, β_t^{NK} are respectively village and year-province fixed effects, *FDLR*$_i$ is an indicator for whether village i was controlled by the FDLR in 2008, $1(t = 2009 + k)$ is an indicator for whether $t = 2009 + k$, $Y_{i,t}$ is an indicator for whether the FDLR attacks village i in year t. The sample are all years (1995–2012) for the quasi-experimental villages. We exclude the confounding villages to shield the analysis from coincidental changes in other FDLR's factions behavior induced by policies in other districts. We cluster the standard errors two-ways at the village and Chiefdom-year level.[5] We denote the estimates obtained with this econometric strategy as *baseline estimates*. We seek to test whether $\beta_k > 0$, $k > 0$.

The coefficients, presented in Figure IV, Panel B, are positive each year after 2009 and significant at the 1% level. There are no pre-trends. This implies that after Kimia II is associated with a disproportionate rise of FDLR attacks on the villages of the FDLR state.

Robustness

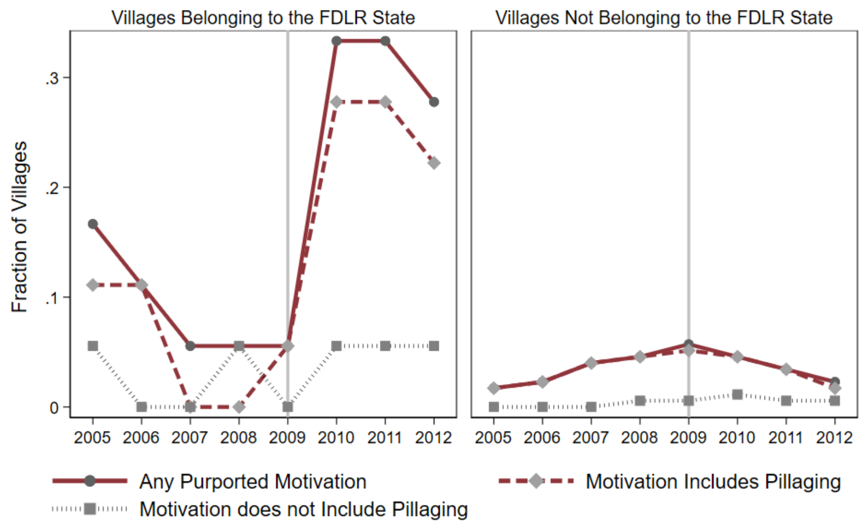
We now examine confounds. First, we verify that the result is also present in *Differences-in-Differences* using simple OLS. Since the sample includes years since 1995, and thus covers the First and Second Congo Wars, it is possible that, in *Differences-in-Differences*, any

[4]With the exception of Borusyak et al. (2023), the estimators developed in the recent papers of staggered treatment adoption, reviewed in Roth et al. (2023), are not designed to improve over OLS when with simultaneous treatment. In contrast, Borusyak et al. (2023) show that their estimator is efficient and robust even with simultaneous treatment, unlike OLS, which is not robust to pre-testing (it conflates estimation and testing). Authors' Personal Supplemental Appendix I, Section 7, confirms that these other estimators and OLS yield the same coefficients and confidence intervals and that, while inefficient, yield identical conclusions to Borusyak et al. (2023).

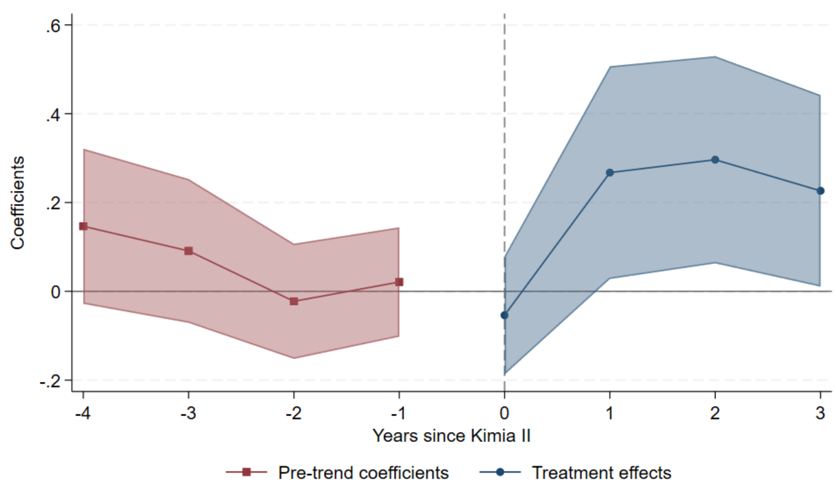
[5]There are 193 and 360 Village and Chiefdom-year clusters, respectively, in the quasi-experimental sample. Monte Carlo simulations, reported in Section 8 of Authors' Personal Supplemental Appendix I, show that the two-way cluster standard errors in Borusyak et al. (2023) estimator perform well in finite-samples.

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

A. Times Series



B. Event Study



Notes: Panel A 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. The sample in this panel includes all quasi-experimental villages. Panel B shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2023) estimator. The dependent variable is an indicator for whether the FDLR attacks village i in year t. The regression includes village and year fixed effects. 3,474 village-year observations are used in the estimation. Standard errors are two-way clustered at the village level and the Chiefdom-year level. The figure presents the treatment effect coefficients as derived by Borusyak et al. (2023) robust and efficient estimator and its associated command. The Borusyak et al. (2023) estimator does not omit the period -1. The reference group for estimation (coefficients on 0,1,2...) is all pre-treatment (and never-treated) observations. The pre-trend is estimated through a separate regression, hence the gap between the coefficient on 0 and -1. The reference group for the pretrend test (coefficients on -1,-2,-3,...) is all periods more than k periods prior to the event date (and all never-treated observations).

earlier shock affecting only the FDLR state villages might induce a spurious correlation. To isolate the effect of Kimia II from the FDLR state levels after the creation of the FDLR state, we include $\gamma^D \; 1(t > 2004) \times FDLR_i$ as a control, where and $\gamma^D \; 1(t > 2004)$ is an indicator taking value one if the year is in the quasi-experimental window. We estimate Equation 2, where α_i^D and $\beta_t^{D:NK}$ are the village and the year-province fixed effects:

$$Y_{it} = \alpha_i^D + \beta_t^{D:NK} + \beta^D \; FDLR_i \times 1(t > 2009) + \gamma^D \; 1(t > 2004) \times FDLR_i + \epsilon_{it}^D \quad (2)$$

We cluster the standard errors at the village and Chiefdom-year level. We denote these estimates as baseline (differences-in-differences) estimates. The estimate of β^D, which we present in Table B.2, Column (1), is positive and significant. Its magnitude implies that Kimia II increases FDLR attack exposure in targeted villages from 7% to 24%, (at least) *for each of the three years after Kimia II*, a 3.4-fold increase over its pre-Kimia II mean.

In Columns (2)–(10), we: examine the robustness of our result to including the confounding villages; include *district*-year fixed effects (in our sample, there are seven districts); include village-year time trends; include, as controls, village constant characteristics interacted with year dummies;[6] include yearly migration levels in and out of the village; combine all of these robustness checks in the quasi-experimental villages, and in the full sample; restrict to villages with gold endowments; include Conley (1999) standard errors (with radius 100km and AR(1) process).[7] Across all columns of Table B.2, the estimate remains large and significant. Its lowest level is .18 (significant at the 5% level) when we include confounding villages. When we include village-year linear trends, the coefficient doubles (.40), and is significant at the 1% level. It is also larger (.43) when we restrict the sample to gold villages—that the magnitude of this coefficient differs in any direction is unsurprising, as this column uses a different sample: the sample falls from 1,422 to 390.[8]

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[6]Table B.4 analyzes the predicted probability that a village belongs to the FDLR state for each covariate.

[7]Figure B.5 presents confidence intervals using Conley (1999) with varying assumptions spatial correlation.

[8]In Table B.5, we estimate Equation 2 using, instead of village and year fixed effects, indicators for $1(t > 2009)$ and for *FDLR*$_i$ as controls (1), excluding the only Chiefdom that produces a negative coefficient if coded as *FDLR*$_i$ (2), clustering the standard errors at the Groupement level or at the Chiefdom-post Kimia II level (respectively 3,4), including controls for the world price of coltan or gold interacted with an indicator for whether village i has coltan or gold, respectively (5,6), controlling for the logged distance to the FDLR state interacted with year indicators (7). Column (8) includes $1(t > 2009)$ interacted with the the logged distance to the FDLR state and shows that the effect is concentrated in the FDLR state. In Table B.6, we replace village fixed effects with the

The baseline estimation is a linear probability model, hence measurement error could create bias (Hausman, 2001). Table B.3 shows that the baseline coefficient is preserved in logit, conditional logit, as well as using count of attacks or inverse hyperbolic sine transformation of attacks' count as dependent variable.[9] Overall, the checks performed in this section suggest that Kimia II caused a rise in reported FDLR attacks.

5.2. *Characterizing the Effect*

We now provide five characterizations that help interpret the coefficient.

Biased Reports?

Even if the coefficient is causal, it is not clear whether Kimia II caused an increase in FDLR attacks, or instead an increase in reporting. We examine this possibility in four steps. First, we re-estimate Equation 2 using publicly available data (ACLED, 2022) in lieu of our potentially biased data (Table B.2, Column 12). The sign, magnitude, and significance, are preserved. Second, we find analytically that, even in the extreme case where up to 24% of FDLR attacks reported to have taken place after Kimia II in the FDLR state were *entirely false* and that this type of overreporting *only* affected the FDLR state villages and *only* after Kimia II, the estimate would still imply an increase in attacks (Section C.1). Third, we use simulations to derive the bound, and find the same result (Figure B.9). Fourth, we conducted qualitative interviews with 22 FDLR perpetrators who were dislodged by Kimia II in 2010. The perpetrators report the same effect of Kimia II (Section A.3.) This suggests that endogenous reporting bias is unlikely to explain the baseline coefficient.

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lagged dependent variable. Figure B.6, Panel A estimates Equation 2 using each year of 2005–2011 as cutoff; Panel B estimates Equation 2 using each Chiefdom in sample as an indicator for FDLR state. We also 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 simulations show that the p-value of the sharp null hypothesis is 0.00. Figure B.7 presents the distribution of the simulated coefficients. Figure B.8 includes the eight pre cofficients.

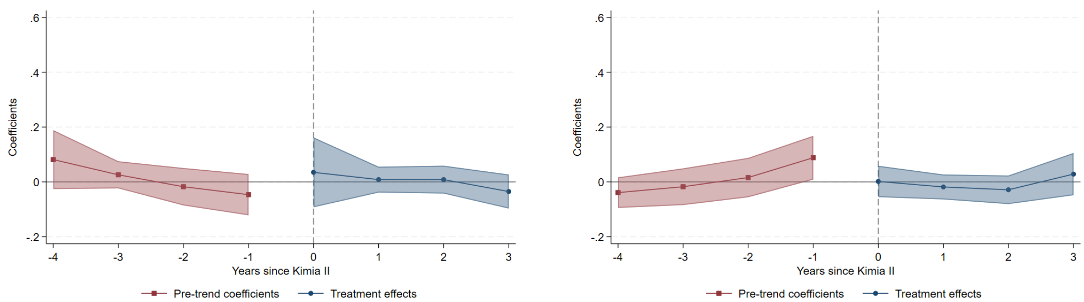
[9]We also simulated different levels of mis-classification of our binary outcome variable drawing on the literature on multiple imputation for missing data to also accommodate measurement error (Blackwell et al., 2017; Cole et al., 2006). For each level, we estimate 1,000 times Equation 2 but each time switch the outcome variable in a random subset of the observations corresponding to the level of measurement error. The 1,000 regressions give us a distribution of coefficients. The point estimate for each level $\bar{q}$ is $\bar{q} = 1/1000 \sum_{j=1}^{1000} q_j$ of the 1,000 estimates. An estimate of the variance of the point estimate is $\bar{s}^2 = 1/1000 \sum_{j=1}^{1000} s_j^2 + S_j^2(1 + 1/1000)$ where s_j is the standard error of the estimate of q_j from the analysis of simulation j and $S_j^2 = \sum_{j=1}^{1000}(q_j - \bar{q})/(1000 - 1)$. The results suggest there would need to be over 20% of such measurement error in our binary outcome variable.

Increase in Violence by all Armed Groups? Falsification Exercise

Another concern with these results is that Kimia II could have changed the dynamics of violence in the region. Panel A of Figure V presents the event study coefficients, with attacks by other actors as dependent variables. The coefficients, both for years before and for years after 2009, are close to zero and not significant. This suggests that Kimia II completion could not have affected FDLR attacks by first affecting the dynamics of violence in the FDLR state. We also examine whether Kimia II completion led to a rise in attacks by other armed actors on the villages they controlled. Panel B of Figure V presents the event study coefficients. Again, the coefficients are indistinguishable from zero. Thus, Kimia II completion is *only* associated with an increase in violence by the targeted FDLR factions.

Figure V.: Falsification Test: Attacks by Other Armed Actors

A. In FDLR State *B. In Villages Controlled by Other Actors*

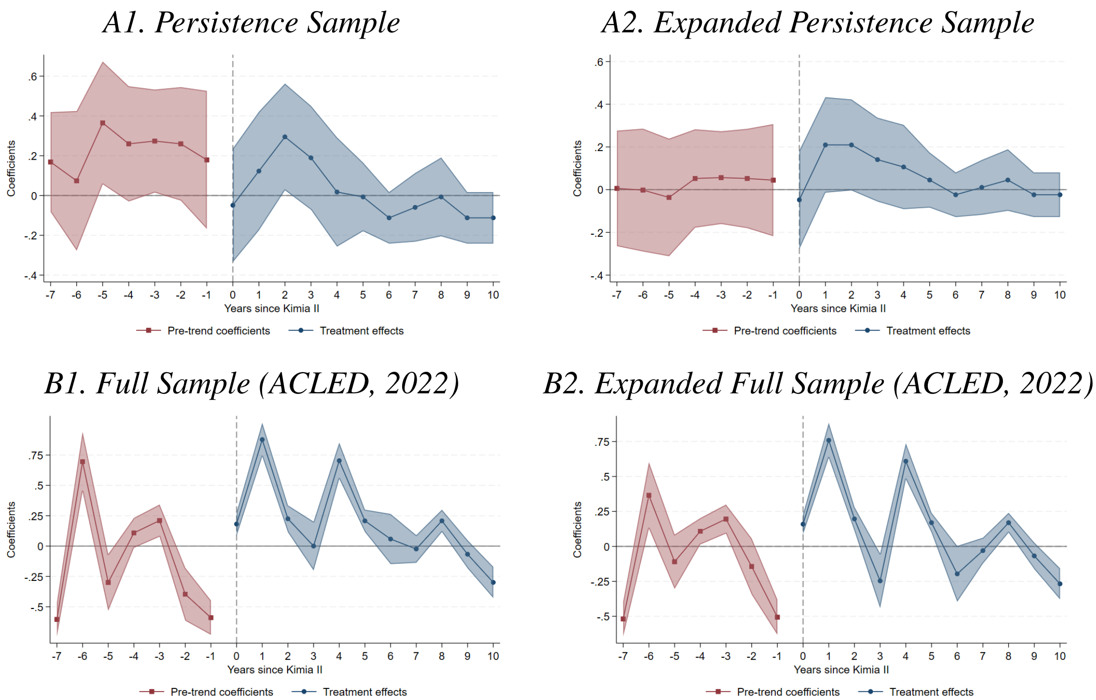


Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2023)'s estimator. The reference group for estimation is all pre-treatment (or never-treated) observations. The reference group for the pretrend test is all periods that are more than k periods prior to the event date (and all never-treated observations). 3,474 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 $FDLR_i$ here takes value 1 if any non-FDLR actor controlled village i in the year 2008.

How Long is the Effect? Duration Analysis

To analyze the duration of the effect, we conducted a second collection exercise in Fall 2022, allowing us to observe attacks until 2022 in a subset of the original sample. Due to resource limitations, we selected all the sample villages in Basile and all the sample villages in the neighboring Chiefdom of Wamuzimu, both in the territory of Mwenga. In each village of this sub-sample, we replicated the data collection exercise, focusing on the years 2000–2022, for which there could be reliable recall in 2022. Panel A1 in Figure VI

Figure VI.: Duration Analysis



Notes: This figure shows Equation 1 estimates, estimated using Borusyak et al. (2023) estimator. Panel A.1 uses data collected in 2022 in the subset of Basile and Wamuzimu villages for which data were collected in 2013 (725 village-year observations). Panel A.2 includes all the data from the expanded persistence sample (1,178 village-year observations). Standard errors are two-way clustered at the village level (34 clusters in Panel A.1 and 55 in A.2) and the Chiefdom-year level (46 clusters in both). While this number of clusters is lower than in other estimations of this study, potentially causing over-rejection when standard errors are clustered, Cameron et al. (2008) suggest that 30 to 40 clusters is sufficiently large. Nonetheless, we also estimated the two-way clustered standard errors at the village and Groupement-year, as well as not clustered. Section 9 of Authors' Personal Supplemental Appendix I shows that these yield identical results. Panel B.1 presents the analysis for the quasi-experimental villages for which data were collected in 2013, using as dependent variable an indicator for whether an attack is recorded in ACLED within 50 kilometers of a village (4,961 village-year observations). Panel B.2 adds the newly surveyed villages of Basile from the expanded persistence sample (5,477 village-year observations). Standard errors are two-way clustered at the village (193 clusters in Panel B.1 and 217 in Panel B.2) and the Chiefdom-year level (520 clusters in both). Despite it being the same sample of villages, the confidence intervals using ACLED (2022) are smaller. This is due to the poor quality of ACLED (2022) data in this area: there are more non-zero observations, but at the same time more geographical clustering because it is poorly measured and thus we had to use 50km bandwidth. Indeed, almost all recorded events in ACLED (2022) in the district of Mwenga were assigned to the town of Mwenga center with no information on the Chiefdom in which the event took place. This town is administratively located in Basile. Section 3.4 of Authors' Personal Supplemental Appendix I provides additional information on corrections we were able to make to the ACLED (2022) data.

presents the baseline estimates from Equation 1 with these data. The confidence intervals are wider (the sample is much smaller), but the pattern is identical and the effect remains significant in year 2. The coefficients are positive until year 4, suggesting that the effect lasts 4 years. Panel A2 in Figure VI shows the results when including additional villages in

Basile (henceforth, *expanded persistence sample)*, which we included to achieve a higher statistical power for the analysis of persistence. This sample includes 21 additional villages, 10 in the FDLR state. The baseline coefficient estimates using the expanded persistence sample are positive until (and including) year 5 after Kimia II. Including Groupement-year clustering produces the same results (see Table B.7, Column (6)). Figure B.10, presents the baseline coefficient estimates in the sample restricted to the 36 villages of Mwenga, using the original data and the data collected in 2022, separately. Furthermore, Panels B1 and B2 use the distance of ACLED (2022) events to the original sample and to the expanded persistence sample, respectively. The baseline coefficients decay over time. Except for year 3, they are positive until year 4 (and in the case of Panel B.1, until year 6) after Kimia II.

Why Does the Effect end after 5 Years?

Providing a causal explanation based on the patterns in the data for the why the effect *stops* would hinge on weak empirical grounds, because the "quasi-experiment" is the completion of Kimia II, rather than the consolidation of state power after Kimia II. However, there is suggestive evidence that this reflects state power consolidation. Indeed, the qualitative interviews with the FDLR combatants suggest that the FDLR factions stopped pillaging because the army consolidated its monopoly of violence in Basile by 2015, making it impossible for the FDLR to even pillage there, and that then, they moved away to another province. For example, in a qualitative interview in Nov. 2022, in the village of Bushaku 2, we noted: "They went to other provinces of the DRC; it was a total disbanding." This interpretation is corroborated in the persistence sample, which shows that the national army's control continuously increased in the FDLR villages (Figure B.16).

Making Sense of the Effect across Samples

One concern with these analyses is that, to assess the robustness of the baseline estimate, we included various sources and datasets. To assess the consistency of our estimates across sources and samples, Table B.7 estimates Equation 2 in various sources and samples. The table presents the baseline estimate; restricts the original sample to Basile and Wamuzimu Chiefdoms; uses the 2022 newly collected data; uses the expanded persistence sample; includes the original sample *and* the new 21 villages from the expanded persistence sample—overall 260 villages for 2000–2012; uses the original sample *and* the expanded persistence sample, two-way clustering the standard errors at the village and at the Groupement-year level; use the universe of villages in Basile, that we obtained at the Groupement capitals from Groupement authorities in 2022. Across sources, the baseline coefficient's sign and statistical significance are preserved (and in some cases they are strengthened).

In sum, in this section, we provided evidence that the effect of Kimia II, which, reported in Section 5.1 and for which we have provided evidence that it is causal, reflects a *4–5 year* increase in *actual* attacks by the FDLR, and does not capture attacks by other actors.

5.3. *Mechanisms*

In this section, we provide evidence suggesting that this effect is driven by the effect of Kimia II on the factions' incentive to steal from the villages they protected.

Violent Theft Operations, Retaliation, or Territorial Conquest Attempts?

To examine the attack motivations induced by Kimia II, we analyze the purported motivation for the attacks. For each attack, we gathered the details of whether, from the perspective of the villagers, the perpetrators' motives included (not mutually exclusive): pillaging, territorial conquest, or retaliation/punishment. These are generally accepted classes of attacks by the population (and the taxonomy comes from the villagers). Table B.8 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. Pillages have more kidnapping of village men (typically for transporting stolen goods) and stealing of cattle. 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. 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. With 268 households per village, the average value of stolen goods in a pillage amounts to 20.39 USD per household. For comparison, Table B.1 showed that taxing a village yields 64.97 USD per year per household. Table I presents the estimates from Equation 2. Column (1) presents the baseline estimates as benchmark. In Columns (2)–(4), the dependent variable is an indicator taking value one if there is an attack motivated by territorial conquest, punishment, or pillaging respectively. Kimia II has no effect on attacks motivated by conquest. Those motivated by punishment explain at most 16% of the increase (the coefficient is 0.043, against 0.24 for any attacks). In contrast, 90% of the increase in attacks is explained by attacks motivated by pillaging, which increase by 22 pp. as a result of Kimia II. Figure B.12 presents the event study estimates for pillages, retaliation, conquest attempts, respectively. This suggests that Kimia II predominantly incentivized violent theft, plunder.

A first concern with this analysis is that attacks can have various motivations, hence the pillages could be a by-product of other motivations. In Columns (5)–(7), we include, as controls, indicators for an attack with any other motivation. The results are unchanged.

Table I: Mechanism—Purported Motivation for the Attacks

	Dependent Variable: FDLR Attack						
	Any	Conquest	Punishment	Pillage	Conquest	Punishment	Pillage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.240***	-0.001	0.043***	0.220***	-0.030	0.037***	0.210***
	(0.068)	(0.018)	(0.006)	(0.063)	(0.020)	(0.006)	(0.060)
Control FDLR Conquest	N	N	N	N	N	Y	Y
Control FDLR Punishment	N	N	N	N	Y	N	Y
Control FDLR Pillage	N	N	N	N	Y	Y	N
Observations	3,474	3,474	3,474	3,474	3,474	3,474	3,474
R^2	0.14	0.08	0.08	0.13	0.29	0.28	0.17
Mean Dep. Var.	0.07	0.02	0.02	0.04	0.02	0.02	0.04
Village Clusters	193	193	193	193	193	193	193
Chiefdom-Year Clusters	360	360	360	360	360	360	360
Conley (1999) p-value	0.00	0.97	0.00	0.00	0.12	0.00	0.00

Notes: This table presents the estimates from Equation 2. Column (1) shows the baseline estimates. In Columns (2)–(4), the dependent variable is an indicator for whether: there is an FDLR attack with the purported intention to conquest, retaliate, pillage, respectively (see Section 3). In Column (5), the dependent variable is an indicator for whether there is an FDLR attack with the purported intention to conquest. It includes controls for whether there is an FDLR attack with the purported intention to pillage, and separately retaliate. In Columns (6) and (7), the dependent variable is an indicator for whether there is an FDLR retaliatory, pillage attack, respectively. The columns include controls for the other two categories. All columns include village and province-year fixed effects. Standard errors, two-way clustered at the village level and the Chiefdom-year level, are in parentheses. *Village Clusters* and *Chiefdom-Year Clusters* indicate the corresponding number of clusters included in standard error estimation. *Mean Dep. Var.:* is the mean of the dependent variable in the FDLR state prior to Kimia II. *Conley (1999) p-value:* Shows the p-value with Conley (1999) standard errors. In Column (3), due to collinearity, we estimated Groupement instead of village fixed effects in the Conley (1999) specification). *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5%, and 1% levels, respectively. The indicator for pillage 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 as described in Section 3), or in which the FDLR confiscated wealth during the attack. Figure B.11 shows that one third of the pillages occur in villages controlled by the army but for which the army was absent during the attack, and one third takes place in villages not controlled by the army; furthermore 84% of attacks that take place when the village security force is present take place at night; in 78%, they used forced labor, to transport stolen goods.

A second concern with this analysis is that the motivation for the attacks is reported by the villagers, and thus could be biased. We address this concern in four ways.

First, we analyze the perpetrator's identity and the *across-village* targeting of the attacks are consistent with *war*. Table II, Panel A, shows that Kimia II has no effect on attacks nor conquest attempts by the Congolese army, and that the effect is driven by villages in which the army was *absent*, which is inconsistent with war against the army.

Second, we analyze the whether the *within-village targeting* is consistent with *retaliation* against households. To do so, decomposing the differential rise in attacks, we examine whether the rise is driven by households which would be more likely candidates to be targeted as retaliation (state officials, militia fighters, or connected to village chiefs) or instead by other households. In Panel B, we use the household-year data to estimate:

$$Y_{ijt} = \beta^D_{HH}\, FDLR_i \times 1(t > 2009) \times 1(target_{jt}) + \beta^D\, FDLR_i \times 1(t > 2009) \\ + \gamma^D\, FDLR_i \times 1(t > 2004) + \alpha^D_{ij} + \beta^{D:NK}_t + \epsilon^D_{ijt}$$

where j indexes the household, $1(target_{jt})$ is an indicator taking value 1 if household j is a possible *target* of punishment at time t, and α^D_{ij} are household fixed effects. Column (1) presents the baseline result in the household-level data. Across Columns (2)-(5), β^D_{HH} is insignificant, β^D is positive and significant, suggesting that the rise in attacks is not driven by households that, a priori, would be targeted by retaliation. Column (6) estimates the fully saturated model, also including $\gamma^{D'}\, FDLR_i \times 1(t > 2004) \times 1(target_{jt}) + \gamma^{D''}\, 1(t > 2004) \times 1(target_{jt}) + \beta^{D'}_{HH} 1(t > 2009) \times 1(target_{jt})$. The coefficients are unchanged, suggesting that the rise in attacks is not caused by increased risk by those households. This pattern is inconsistent with retaliation explaining the effect of Kimia II.

Third, we analyze whether the *within-village* targeting is, instead, consistent with *theft*. The qualitative reports from Basile (Section A.2.1) show that the FDLR regularly acquired information on the wealth distribution by nurturing spies in the villages, called the "*eclaireurs*," those who "provide light." In Panel C, we estimate Equation 3, where $1(target_{jt})$ is an indicator of household wealth. The estimate of β^D_{HH} is positive, large, and significant, suggesting Kimia II led to attacks that disproportionately targeted richer households. This effect holds even in Column (8), where we control for indicators of household education and for the political variables interacted with $FDLR_i \times 1(t > 2009)$. This suggests that other household characteristics do not explain the targeting of richer households. Column (9) shows that the result holds in the fully saturated regression as well.

Table II: Mechanism—War, Retaliation, and Theft

Panel A. War	*Dependent Variable:*				
	FDLR Attack	Congo Army Attack	Congo Army Conquest	FDLR Attack	FDLR Attack
	(1)	(2)	(3)	(4)	(5)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.240***	-0.023	-0.016	0.234***	0.313***
	(0.068)	(0.016)	(0.014)	(0.068)	(0.114)
$\text{FDLR}_\text{i} \times 1(t > 2009) \times$ Congolese Army_{t-1}					-0.158
					(0.141)
Control for Congolese Army_{t-1}	N	N	N	Y	Y
Observations	3,474	3,474	3,474	3,281	3,281
R^2	0.14	0.07	0.08	0.14	0.15
Mean Dep. Var.	0.07	0.00	0.00	0.07	0.07
Conley (1999) p-value	0.00	0.07	0.11	0.00	0.00
Conley (1999) p-value Interaction					0.05

Panel B. Retaliation	*Dependent Variable: FDLR Attack*					
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.016***	0.017**	0.013***	0.013**	0.015***	0.015***
	(0.004)	(0.008)	(0.005)	(0.005)	(0.004)	(0.004)
$\text{FDLR}_\text{i} \times 1(t > 2009) \times 1(target_{jt})$		-0.004	0.149	0.060	0.017	0.019*
		(0.042)	(0.105)	(0.051)	(0.011)	(0.011)
$1(target_{jt})$	-	Official	Fighter	Chief	Index	Index
Controls	N	N	N	N	N	Full
Observations	13,614	6,820	12,834	13,454	12,670	12,670
R^2	0.10	0.09	0.09	0.10	0.09	0.09
Mean Dep. Var.	0.00	0.00	0.00	0.00	0.00	0.00
Conley (1999) p-value	0.00	0.00	0.00	0.00	0.00	0.00
Conley (1999) p-value Interaction		0.94	0.65	0.44	0.47	0.59

Panel C. Theft	*Dependent Variable: FDLR Attack*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.016***	0.003	0.006*	0.002	-0.011**	-0.004	0.006**	0.021**	0.007*
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.010)	(0.003)
$\text{FDLR}_\text{i} \times 1(t > 2009) \times 1(target_{jt})$		0.037***	0.010*	0.015**	0.038***	0.023***	0.019***	0.013***	0.013***
		(0.007)	(0.006)	(0.006)	(0.003)	(0.002)	(0.003)	(0.005)	(0.004)
$1(target_{jt})$	-	Rich	Wives	Lands	Assets	Married	Index	Index	Index
Controls	N	N	N	N	N	N	N	Educ+Pol	Full
Observations	13,614	13,442	13,382	13,345	6,462	12,243	11,855	11,326	11,855
R^2	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10
Mean Dep. Var.	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Conley (1999) p-value	0.00	0.42	0.01	0.43	0.01	0.57	0.05	0.01	0.05
Conley (1999) p-value Interaction		0.00	0.07	0.00	0.00	0.00	0.00	0.00	0.00

Notes: Panel A presents the estimates from Equation 2. Column (1) shows the baseline estimates. In Columns (2)–(4), the dependent variable is an indicator for whether: the Congolese army attacks, Congolese army attempts a conquest, the FDLR attacks. Column (5) is as Column (4) but controls for whether the Congolese army occupied positions in the village in year $t-1$ (coefficient not reported), and interacting $\text{FDLR}_\text{i} \times 1(t > 2009)$ with whether the Congolese army was present in year $t-1$ (in that case p-values with Conley spatially clustered s.e. are reported for both the main coefficient and the interaction term), respectively. Panel B uses household-year data. Column (1) first presents the estimates from Equation 2 at the level of the household, including household fixed effects. The dependent variable is an indicator for whether the respondent's household is victim of an attack by the FDLR (*Mean. Dep. Var.* exact value is 0.41%, rounded to 0.00). In Columns (2)–(4), we include, as control, $\text{FDLR}_\text{i} \times 1(t > 2009) \times 1(target_{jt})$, where $1(target_{jt})$ stands for: if the respondent works in the government during 2006–08, if the respondent participates in any armed group during 2006–08, if the respondent is related to chief. In Column (5) $1(target_{jt})$ is a normalized index of all 3 characteristics from Columns (2)–(4) constructed by principal component analysis. Column (6) replicates Column (5) but includes additional sub-terms of the interactions to estimate the saturated model. Panel C uses the same interaction where $1(target_{jt})$ stands for household characteristics that indicate wealth. Column (1) first presents the estimates from Equation 2 at the level of the household. The dependent variable is an indicator for whether the respondent's household is victim of an attack by the FDLR (mean 0.41%). In Columns (2)–(6), we include, as control, $\text{FDLR}_\text{i} \times 1(t > 2009) \times 1(target_{jt})$, where $1(target_{jt})$ stands for: if the respondent's father comes from a rich family, if the respondent's father has at least 1 wife, if the respondent's father owns at least 1 plot, a normalized proxy for the assets owned by the respondent's household in year $t-1$, if the household respondent is married in year $t-1$, respectively. In Columns (7)–(9), $1(target_{jt})$ is a normalized index of all 5 characteristics constructed by principal component analysis. In addition, Column (8) controls for the interaction of FDLR_i with whether the household head completed primary school and with whether they finished secondary school as well as with the interaction of FDLR_i with the index of political characteristics used in Panel B Column (7). Column (9) replicates Column (7), but also includes additional sub-terms of the interactions to estimate the fully saturated model. *Controls:* indicates whether additional household characteristics are interacted with FDLR_i ("Educ+Pol" indicates that it is interacted with whether the household head completed primary school and with whether they finished secondary school and, in addition it is also interacted with the index of the households political role as defined for Column 5 of Panel B.). "Full" indicates that the specification is fully saturated. All columns include village and province-year fixed effects. Standard errors, clustered two-way at the village level (193 clusters) and the Chiefdom-year level (360 clusters), are in parentheses. *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.

Fourth, we interviewed 22 FDLR members who were in Basile around Kimia II (some quotes are in Section A.4). We extracted 51 attacks. Based on the descriptions, we recovered that pillaging motivated 42 of the 51 (83%); 16 of the 42 targeted our sample; although years are given with uncertainty, 12 of the 16 coincide with an attack in our data within 2 years. In 11 of these 12, both villagers and perpetrators said it was to pullage. Overall, 98% of the attacks they reported in the FDLR state villages of our sample were to pillage.

Why Did Kimia II Incentivize Violent Theft?

Section C.2 presents a model of a bandit choosing the level of expropriation each period in a village, building on the qualitative data presented in Sections A.2 and A.4. If expropriation decreases village growth, settled factions should have an incentive to refrain from pillaging as an investment in future expropriation. If the expected frequency of expropriation shrinks, expropriating all assets (pillaging) can be done without loss to future income.

We first verify whether the pillaging/taxation trade-off exists, by computing the break-even point from the decision not to pillage. Table B.8 estimates imply that "no pillaging" implies foregoing $5,464.82$ USD today on average. As we estimated in Section 5.4, taxing a village yields $17,902.4$ per year on average, amounting to $1,490$ USD per month. Hence, *refraining* from pillaging and instead taxing (an investment) has a break-even point of $5,464.82/1,490 = 3.7$ months. Disrupting the ability to expropriate daily for more than 3.7 months should incentivize pillaging. Figure B.13 shows that one pillage permanently reduces the assets of pillaged households. We then verify whether Kimia II reduced expropriation *frequency*. The frequency of FDLR expropriation events (tax payments and pillages) in the FDLR state goes from 350 to 50 events per year (Figure B.14, Panel A).

Another possibility is that the rise in theft is due to a reduction in the FDLR factions' income, an *income effect* (see Figure B.14, Panel B). If so, then FDLR attacks should increase in *any* village, not just those that the FDLR can no longer tax. We test this alternative interpretation by examining whether Kimia II increased attacks in any village (as should be the case in the case of an income effect), or instead disproportionately affected villages that the FDLR can no longer tax (as should be the case if the village-specific time horizon of expropriation was at least partly accountable for the rise in pillaging). Figure B.15 shows that FDLR attacks increased in villages in which the FDLR expropriates with low frequency that year (Panel A), but not the rest (Panel B), inconsistent with an income effect.

Overall, this suggests Kimia II led to FDLR pillaging because it broke the encompassing interest the FDLR factions previously had for the villages they could tax.

5.4. *Implications for Household Welfare*

On the one hand, FDLR pillages come with attacks on human dignity and degrading violence. Table III estimates Equation 2 using indicators of costs faced by the households as dependent variables (Figure B.17 presents the baseline coefficients of the event study). Columns (1)–(4) show that Kimia II doubled sexual violence and plundering incidence, and quintuples abduction incidence, typically for forced labor for transporting stolen goods.

On the other hand, Kimia II caused a decrease in the households' tax burden and an increase in the state's informal tax revenue. Columns (5)–(10) show that the value stolen from the average household increases sevenfold, from $.50$ USD in 3.52 USD, total taxation payments per year per household decrease 41%, from 66.80 USD, in 27.17 USD (the average yearly household tax paid to nonstate actors decreases in 70%, from 63.84 to 17.04 USD). Including taxation and pillage, household payments decrease from 67.34 USD in 24.41 USD yearly. With 268 households per village, village tax decreases by $7,281.56$ USD down from $17,902.4$ USD; the state's revenue increases by $10,620.84$ USD.

We can benchmark the corresponding decrease in the tax burden using available data sources for the area. This corresponds to a decrease in the tax burden as a share of assets held in cattle from 7% to 2% and as a share of income from $[7\%, 11\%]$ to $[4\%, 6\%]$.[10]

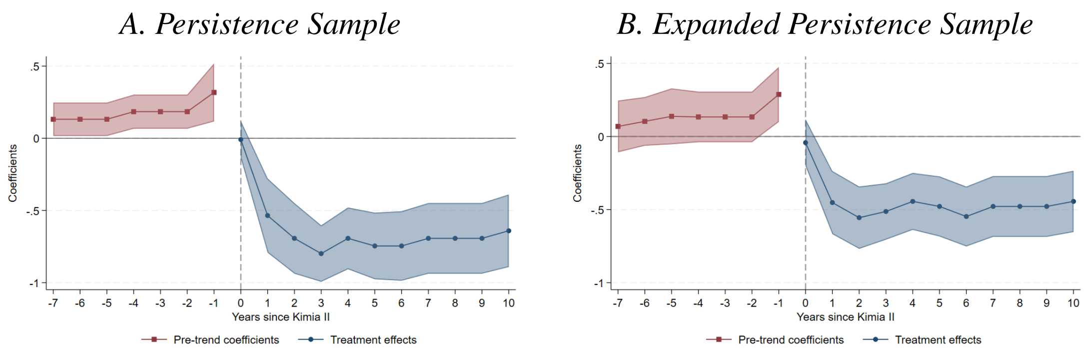
[10] We use the data in Sánchez de la Sierra (2014). For assets: We use the perpetual inventory method to produce a plausible stock of cows, goats, and pigs from Sánchez de la Sierra (2014) data on flows. On average, a household held 921 USD (in USD of 2015) of assets, hence the tax burden before Kimia II is 7% as a share of assets held in cattle. For income: we reconstructed income estimates arising from gold production to provide a lower bound of the area's per capita income (the most lucrative sector), and therefore an upper bound of the tax burden: a. daily production of a gold miner: approximately $1g$ (Geenen 2013); b. total number of work days per year of a gold miner: 300 (assumed, based on our own qualitative work conducted in 2013 in Mwenga); c. local sale price of gold in 2008: 25 USD per gramme (average in the province, using our data from Sánchez de la Sierra 2014, and also reported in Marchais et al. 2022, Figure F.13); d. share of taxes on the gold mining sector (licences) over value sold by miners: 45% (average in the province, using our data from Sánchez de la Sierra 2014, and also reported in Marchais et al. 2022, Table E.12); e. fraction of adult males in a village of the FDLR state who worked in the mine in 2008: 13% (Table B.1, Column 2). These numbers imply that every adult male that works in mining full time would earn 4,875 USD yearly. Assume the extreme case that the rest of villagers make zero income, this implies a 633.8 USD yearly average income per capita, and a tax burden of 10.5% as a share of income. Assume instead that the rest earns 1 USD a day (a lower bound, as these areas may be richer than this benchmark), this implies a 951.3 USD yearly per capita income, and a tax burden of 7.02% as a share of income.

Table III: 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*
	Rape	*Death*	*Plunder*	*Kidnapping*	*Theft*	*Market*	*Toll*	*Poll*	Total	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.120***	0.029	0.198***	0.233***	3.521**	-2.196	-8.520***	-16.741**	-27.030***	-24.265***
	(0.043)	(0.052)	(0.070)	(0.056)	(1.444)	(1.555)	(2.791)	(7.250)	(9.149)	(8.144)
Observations	3,474	3,474	3,474	3,474	3,132	3,124	3,124	3,124	3,124	3,124
R^2	0.11	0.11	0.12	0.11	0.12	0.49	0.54	0.35	0.37	0.30
Mean Dep. Var.	0.04	0.06	0.07	0.06	0.50	4.12	18.82	44.37	67.32	67.85
Village Clusters	193	193	193	193	185	185	185	185	185	185
Chiefdom-Year Clusters	360	360	360	360	360	360	360	360	360	360
Conley (1999) p-value	0.00	0.65	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

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)–(10) are continuous variables in USD listed in the headers. Standard errors, two-way clustered at the village level and the Chiefdom-year level, are in parentheses. All regressions include village fixed effects and year-province fixed effects. 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 the standard error estimation. *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 B.9 replicates the analysis in Columns (5)-(10) using indicators as dependent variables. The estimate in Column (10) implies that state protection is *less* expensive than protection by the FDLR. A negative coefficient means that Kimia II caused a reduction in the household transfers to armed actors. Overall, the average yearly household tax decreases from 66.80 USD to 39.63 USD after Kimia II was completed.

Figure VII.: Persistent Reduction of Expropriation



Notes: This figure shows the coefficients β_k, $k = -4, ..., 10$ and their corresponding 95% confidence intervals from Equation 1. The dependent variable is an indicator for whether the household paid taxes to any actor in year t. Standard errors are two-way clustered at the village level (34 clusters in Panel A and 55 in Panel B) and the Chiefdom-year level (46 in both Panels). The estimates without standard error clustering or two-way clustering at the village and Groupement-year level yield identical results. Those are reported in Section 9 of Authors' Personal Supplemental Appendix I.

In sum, Kimia II would only be welfare improving if villagers were willing to accept 24.41 USD p.c. per year in exchange for more rape exposure in the village from 4% to 14%, plunder from 8% to 26%, and kidnapping from 6% to 30% during five years.

6. BETTER TO BUILD STATES THROUGH BARGAIN? THE SUN CITY PEACE AGREEMENT

6.1. *The Effect of Sun City on Violence*

By 2002, many urban areas of half of the country were controlled by a coalition of armed groups under the banner of Rassemblement Congolais pour la Democratie (henceforth, RCD). Many rural areas were controlled by the RCD, Congolese militias under the banner of Mai-Mai groups, and the FDLR. In 2003, the Congolese state signed the Sun City peace agreement with the RCD and Mai-Mai rebels, agreeing to *integrate* the RCD and the Mai-Mai armed groups as part of the state apparatus through a process named *brassage* (see Autesserre 2006; Verweijen 2013; Stearns et al. 2013).

Figure VIII, Panel A, left segment, shows that, while the state controlled less than 10% of the locations that were controlled by a faction from an armed group that was part of the agreement (henceforth, "*affected villages*") prior to the agreement, this rises to 40% within three years, and 50% within six years. While 90% of the locations were controlled by rebel factions affected by the treaty, the share drops to less than 10% within three years. No such change in territory by the rebels is in the right segment, showing the fraction in non-affected villages, while the state's tendency to regain territory is weaker. Yet, in contrast to Kimia II, this territorial success was not met with a disruption of peace. The fraction of affected villages with violence falls from 30% to less than 10% (see a map in Figure B.18).

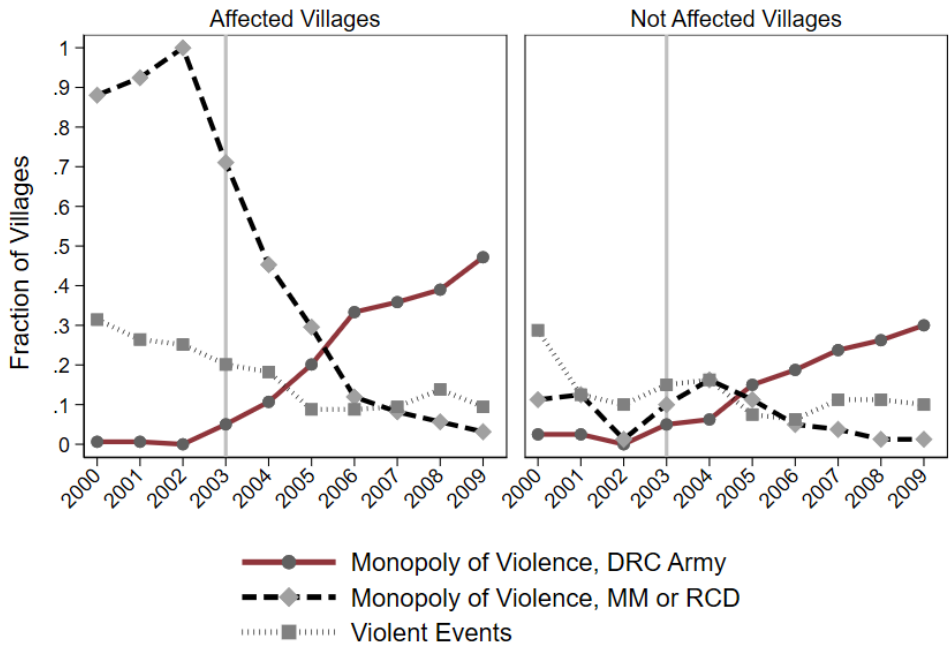
To formally analyze this relationship, we estimate Equation 1, replacing $\text{FDLR}_i \times 1(t = 2009 + k)$ with $\text{SC}_i \times 1(t = 2002 + k)$, where SC_i takes value one for affected villages and zero otherwise, and $1(t = 2002 + k)$ takes value one if the observation is k years after 2002, zero otherwise. Figure VIII, Panel B presents the coefficients. There are no distinguishable pre-trends. None of the post coefficients is positive—they are even all negative. Table B.10 presents the differences-in-differences coefficient: it is robust to the usual confounds. In sum, the agreement achieved comparable territorial success but, unlike for Kimia II, it did not cause an increase in violence, let alone of plundering by the rebels.

6.2. *The Pitfalls of Building States by Bargain*

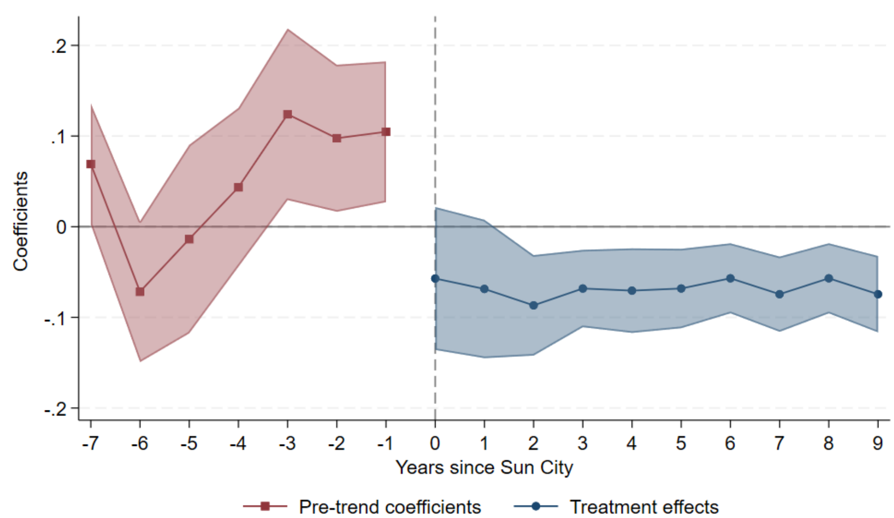
However, bargaining with the rebels over the spoils of the state was also costly. First, qualitative researchers have suggested that the treaty incentivized armed groups' proliferation (e.g., Stearns et al., 2013). This literature further argues that the peace agreement incentivized the creation of armed groups in part by sending a signal that if one threatened the state by force, one could obtain spoils from it. To make progress on this question, we gathered the universe of armed groups known to have existed in eastern DRC since 1990. To do so, we collected the description of each armed group in Kivu Security Tracker (2021) and Vogel (2021), and complemented this information with extensive searches. When such dates were missing, we conducted online searches, and hired informants to obtain this information in Bukavu and Goma in Fall 2022. See Authors' Personal Supplemental Appendix II. Figure IX presents the life-span of armed groups. Their number is small prior to the Sun City peace treaty, but new armed groups began to burgeon after Sun City. The timing of this rise is consistent with the view that Sun City caused a rise of new rebel groups and suggests that it sent a signal that becoming a rebel may translate into spoils. Supporting the interpretation that the proliferation of armed groups is in part due to the expectation of obtaining spoils, many armed groups' stated objectives included obtaining positions of the high-ranked officers inside the Congolese national army. For example, the demands of the Coalition des Congolais pour la Libération, include: "the sons and daughters of Uvira are also incorporated in the national government and are represented in Congolese enterprises"

Figure VIII.: Building States by Bargaining with Rebels: The Benefits

A. Times Series



B. Event Study: Effect on Violence



Notes: Panel A presents the fraction of villages that are controlled by the Congolese army, nonstate armed groups, as well as the fraction of villages where a conquest attempt takes place, by nonstate armed actors. Panel B presents the estimated coefficients and confidence intervals from estimating Equation 1 using Borusyak et al. (2023) estimator, replacing $\text{FDLR}_i \times 1(t = 2009 + k)$ with $\text{SC}_i \times 1(t = 2002 + k)$, where SC_i takes value one for affected villages and zero otherwise, and $1(t = 2002 + k)$ is takes value one if the observation is k years after 2002, zero otherwise. 4,302 village-year observations were used in the estimation. Standard errors are two-way clustered at the village level and the Chiefdom-year level.

and "in the provinces, that the sons and daughters of Uvira are also recruited in the public administration, in the capacity of division head." (Stearns et al., 2013).

Second, it has been suggested that this led to parallel networks within the Congolese army (Verweijen, 2015; Baaz and Verweijen, 2013), and in officers arming rebel groups or splitting with military equipment, often accusing the government of failing to commit to its original agreement (henceforth, *mutinies*). For example Verweijen (2015, p. 83) concludes:

> The "open door policy" that was pursued towards military dissidents reinforced, rather than diminished, incentives for desertion and rebellion [. . .] Under the leadership of Laurent Nkunda, this group, which was supported by several other ex-RCD hardliners who were hostile to the transition, gradually built up a parallel military structure comprised of around 10,000 troops... In this manner, Nkunda managed to establish an autonomous sphere of control within North Kivu [. . .] the CNDP rapidly became one of the strongest and most cohesive armed groups in the Congo [. . .] Many combatants in the Kivus [. . .] were subsequently deployed within the Kivu provinces [. . .] facilitated the establishment of parallel chains of command.

Figure IX, Panel B, shows that mutinies emerge, and skyrocket, in the years after Sun City. While other factors may explain this trend, it is consistent with the qualitative literature, which suggests that integrating the rebels in the army resulted in subsequent mutinies.

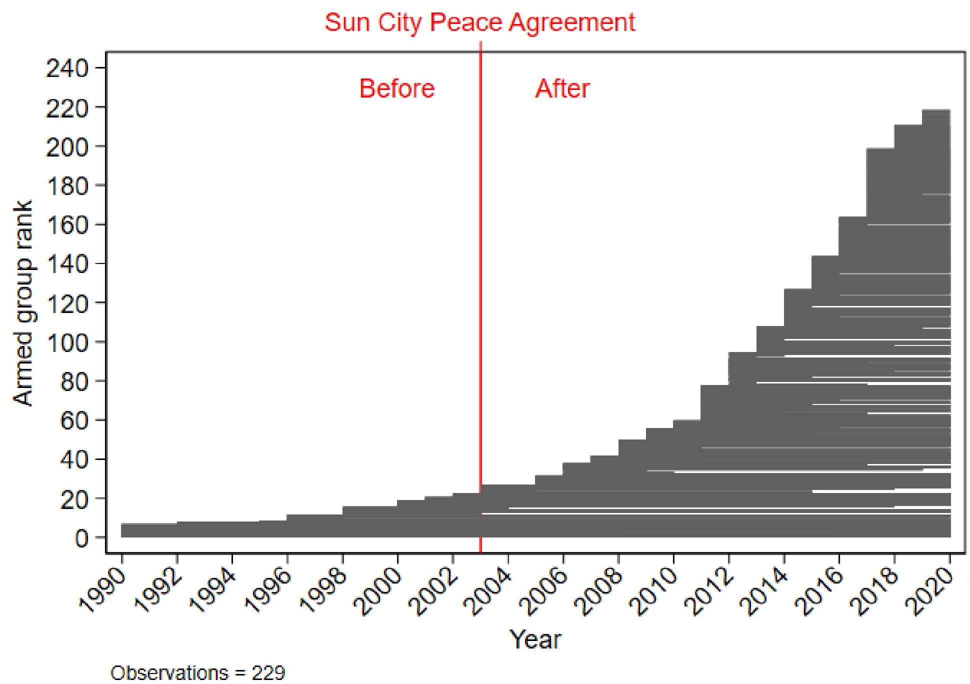
Third, integrating Rwandan-backed rebels, often Rwandophone, into the army created the perception among part of the population that the army was a vehicle of Rwanda, severely straining the legitimacy of the army. For example, Verweijen (2015, pp. 113–114) notes:

> The integration [. . .] of factions perceived to be mobilized along antagonistic identity [. . .] has contributed to fostering the impression that the new military is not impartial. [. . .] a prominent idea [. . .] was that there is not one, but two militaries in the Congo: one pertaining to the Congolese government and a Rwandophone/Rwandan/Tutsi military. This fits into a teleological narrative about the increasing "Rwandophonization" of the armed forces [. . .] Since the ex-RCD, considered a "Rwandan puppet", managed to obtain most of the important command positions in the east, Rwandophone/Rwandan domination became entrenched. The 2009 integration of the CNDP (and to a lesser extent PARECO) was but the logical next step in the Rwandan "colonization" of the Congolese military.

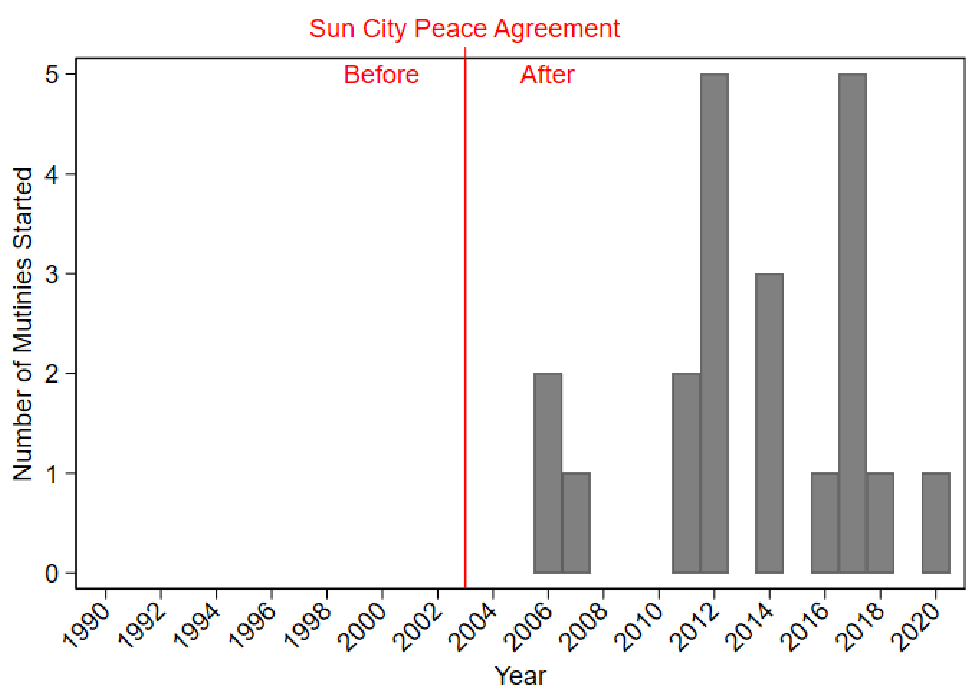
Thus, while bargaining with the rebels in exchange for regaining their territory did not lead to a rise in plunder, the empirical evidence is consistent with the view that it led to the proliferation of rebel groups and undermined the state's integrity in the long-run.

Figure IX.: Building States by Bargaining with Rebels: The Pitfalls

A. Armed Groups' Lifespan



B. New Mutinies and New Groups Equipped by Former Armed Groups inside the Army



Notes: Panel A presents the armed groups that formed by year of start and their year of end. Panel B shows the total number of mutinies started from the Congolese army. To determine whether the group is a mutiny from the Congolese army, we analyzed each armed group in Kivu Security Tracker (2021) and Vogel (2021) and complemented it with our own search. *Sources*: Kivu Security Tracker (2021), Vogel (2021), and authors' data entry and calculations based on the data from these sources. See Authors' Personal Supplemental Appendix II for a description of how we constructed these data.

7. CONCLUSION

A growing literature is concerned with how can states increase their capacity to tax. The questions have centered around technical improvements on the margin within territories that the states control. However, *de facto*, a large share of states do not control areas over which they have *de jure* sovereignty. These areas often represent significant revenue opportunities. Facing this challenge, governments have tended to use two strategies to tax those areas: ousting or bargaining with the rebels. This study provides an economic perspective on the challenges induced by these approaches when the state aims to tax the hinterland.

We show that while asserting territory in a location surrounded by *hard-to-access* hinterland succeeded in *permanently* reducing the households' unofficial taxation burden and shifting taxation away from rebels and towards state officials, it can create incentives for rebels to relocate further into the hinterland, and plunder the very citizens they taxed and protected. Contrasting this response with the effect of a major peace treaty, we have also shown that, by bargaining with the rebels rather than ousting them, the state can avoid this increase in plunder. However, bargaining with rebels is plagued with the difficulty to commit. Indeed, the peace treaty *weakened* the state's monopoly of violence and its legitimacy in the long-run, and made rebels stronger. Some of these costs, such as a rise in mutinies and a reduction of the army's legitimacy are specific to the ways in which the state bargained in this particular case—offering positions in the army. Yet a central aspect of these costs—the incentives it provided for armed groups to proliferate—does not uniquely hinge on the type of transfers: negotiating can signal that the state can concede to extortion, thus creating incentives to form rebel organizations in order to bargain for future spoils.

What strategy is socially optimal depends on how society values the violations of human dignity that arise through plundering. However, our results yield comparative statics for normative analysis. First, asserting the state by force will be *more* desirable when the state has *more* capacity to prevent crime. This will be *less* likely in the hinterland, where rebels can find refuge. Second, bargaining will be *more* desirable when the state has mechanisms to credibly commit to its promises and to not negotiate in the future with new rebels. States could also provide incentives for rebels to integrate into society (de-mobilization). While the success of this approach has been limited in the DRC (Stearns et al., 2013), it can complement military efforts by reducing the risk of plunder when it is highest. Finally, the state can also opt to not interfere. Our results suggest that this would be *more* socially desirable when rebels' territorial control is unchallenged, and when the state is *weaker*.

However, what is socially optimal will often not be rational for politicians (Bates, 2014): even when status quo may be socially desirable, it can be irrational as it signals weak leadership; the effects of de-mobilization are dissipated, thus states may underinvest in de-mobilization; while bargaining may yield short-term political gains against costs in a distant future, military operations have short-term political costs, hence bargaining might be chosen even when it is not socially desirable; if rebels are foreigners, as in Kimia II, even were bargaining to be socially desirable, it may not be rational; even the level of state capacity investments itself is a choice, and might differ from what is socially optimal (e.g., Besley and Persson, 2009)—hence, while a government may bargain with the rebels to avoid short-term political costs, it may at the same time deplete state capacity if yields low political gains, even when its depletion undermines the social gains of bargaining. Indeed:

> Even units with limited fighting capabilities that are not fully controlled by the central hierarchy ultimately contribute to shoring up the incumbents' power. This explains why the rulers have little incentives to transform [it] into a well controlled, well resourced and well organized force. (Verweijen, 2015, p. 347)

Our results do not provide a panacea for increasing the ability of weak states to collect revenue. They provide rather cynical conclusions about the costs of attempting to increase state capacity. While, alone, they do not imply that such goal is not desirable, they suggest that the constraints imposed by architecture of the international state system, which influences what is perceived as legitimate, and the political incentives it creates, can induce undesirable outcomes, given the "real governance" in weak states. Future research could examine how the interaction between such governance and the sanctity of the international state system can be leveraged to better serve the interests of the citizens of such states.

The data underlying this article are available in Zenodo, at https://dx.doi.org/[doi] Sanchez de la Sierra, Raul, Henn, Soeren, Wu, David, Ortiz, Miguel, & Mugaruka, Mastaki. (2023). Replication package for: Monopoly of Taxation Without a Monopoly of Violence: The Weak State's Trade-Offs From Taxation. https://doi.org/10.5281/zenodo.8370065

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Online Appendix

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A. QUALITATIVE QUOTES

A.1. *Life in the FDLR State Prior to Kimia II*

"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." Source: qualitative report in Basile (villages of Pohe, Mito, Wimbi) with civilians, South Kivu, 2013

"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)." Source: qualitative report in Basile (villages of Pohe, Mito, Wimbi) with civilians, South Kivu, 2013

A.2. *Tax vs. Pillage*

"The pillage is taking everything you own, without your consent, and leaving you nothing and you having to transport it to where they want; it is brutal racket. The tax: they don't take everything but you have to contribute; if you don't you can be put in jail or brutalized." Source: qualitative report in Basile (village of Mito) with a civilian, South Kivu, November 2022

"Pillage is racket, we take everything and you can be killed if you try to intervene. A tax is like an established right; you give a part of what you have; if you refuse, you can be pillaged and you lose everything." Source: qualitative report in Basile (village of Lugumbo) with a civilian, South Kivu, November 2022

"The tax is ordered over an economic activity... it is with a paper that proves the payment. A pillage is with a weapon that you are pillaged, if you refuse, you are killed, it is either on the road, or at your house." Source: qualitative report in Basile (village of Itabi) with a civilian, South Kivu, November 2022

"Pillage is a theft by surprise where they take everything from you; whereas tax has been first sensibilized/informed by the state or at the church, it's like a state commandment; the pillage is worse than the tax." Source: qualitative report in Basile (village of Kapanga) with a civilian, South Kivu, November 2022

"*Theft? When you say pillage, it encompasses it all. There is theft, killing, massacre in pillage. Because in a pillage, when they arrived in a household where there is nothing to steal, by default they just killed the people in the household. They find you have nothing, they kill.*" *Source*: qualitative report in Masisi with an FDLR ex-combatant, North Kivu, October 2022.

"*In a pillage, they steal, their objective is to take everything you have. And sometimes, they arrive in a household, and you as responsible member of the household may be tempted to resist their theft to protect your family, they simply kill you. By bullet or by machete. They had everything: weapons, machete, and even spears, and axes. They used axes to destroy households' doors to take everything inside.*" *Source*: qualitative report in Masisi with an FDLR ex-combatant passing as a civilian, North Kivu, October 2022.

"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)." Source: interviews with anonymous civilians in the Chiefdom of Basile (2013).

"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." Source: interviews with anonymous civilians in the Chiefdom of Basile (2013).

"Our logistic was simple. We did not have vehicles nor heavy weapons. The pillages were done late at night. Weapons used were AK47, MAG, and machetes and knives to protect ourselves against dangers during our activities. In the kiosks, we gathered the yield, the clothes, the money, and the booty was transported by the unlucky men and women of the pillage who were taken hostage in their village." Source: interviews with anonymous ex-combatants in the Chiefdom of Basile (2013).

"The importance of the attack to the monthly revenue of my unit is that we ... found food in abundance. We found it easy to save a little money also. We had the power to do small commerce with the villagers with whom we had good relations. My chief trafficked minerals. He paid the checks to villagers. We received nice clothes.." *Source*: interviews with anonymous ex-combatants in the Chiefdom of Basile (2013).

"The attacks had as goal our enrichment and libidinal satisfaction. They gave us money each time we organized them, it was also an occasion for us to satisfy our sexual desire with Congolese women. " *Source*: interviews with anonymous ex-combatants in the Chiefdom of Basile (2013).

"When we lacked money, the high ranks ordered to go organize pillage or ambushes. I was always informed about the orders given to my husband, where it was traps on the population in the road, or pillages in villages. Three times, the attacks led to pillage and rape of women coming back from the market of Mutwanga in Balobola. For me, the attacks were beneficial because I benefited too from the booty." *Source*: interviews with wife of anonymous ex-Lieutenant in the Chiefdom of Basile (2013).

A.2.1. *Spies*

"The villages and households were chosen at random or thanks to intelligence about their economic situation. The information was given by the 'éclaireurs.' The éclaireurs were either our former members who stayed loyal, or members of our families in law because we married some women in the community. They did it by interest because they received dividends in those operations. " *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

"Some villagers let themselves manipulate by the FDLR and worked for them as spies inside this village and other villages." *Source*: authors' qualitative reports from the village of Kyunga, 2013.

A.3. *Effect of Kimia II*

"The security of the Congolese army we can say it was successful at 50%. On the one hand, they were there, and the FDLR was gone. But there is also the other 50%. Even if they were they were not really there. So the FDLR could conduct operations no problem because the army did not stay numerous and was not in every village." *Source*: qualitative report in Masisi with an FDLR ex-combatant, North Kivu, October 2022.

"The Kimia II operations started in Kakole. We were informed of their existence before they arrived. They did not cooperate with us to negotiate our departure, but they informed the population to leave the place and, based on these messages, we learnt what was planned against us. [...] The first villages to lose were those close to Basile: Isopo, Bujinda, Ilombwe, Itabi, Tabaku, Kigogo, Kyanda, Kitamba." *Source*: qualitative report in Masisi with an FDLR ex-combatant, North Kivu, October 2022.

"We lost the control of various villages to find refuge in the forests and the population stayed with the army." *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

"After Kimia II, we acquired resources through pillages we organized. To survive, we had to do incursions at night to pillage and steal. Because of the high number of those, the dates are hard to remember for me. " *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

"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, 2013.

A.4. *Mechanisms*

"Before the change of this relationship, the food and money were easily collected without hassle. After, it was no longer the case. Before, there were villages that, I would say, were not really pillaged. With the change of this relationship, no village was spared from pillaging." *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

"To remove us by military operation, is to make it impossible for us to tax freely these villages. We thus had to use pillage since it was the only mode of survival we had left, we had no other option." *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

"Our relationship with the population worsened because there was no more link between us and them. We were the main losers by losing our source of financing. After Kimia II, we did not have enough resources. We were obliged to change strategies. All the means were good to help us gain our income." *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

"During this time, the pillages were for our survival. If Kimia II had not impeded us from freely taxing these villages, we would not have chosen this modus operandi of pillage. We would have stayed in the same system taking taxes and levies openly known by everyone, it was less annoying and less dangerous for civilians. " *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

The relevance of the villages changed for various reasons: no more fees, tax payments, levies. The taxes before Kimia II were, on the one hand, for our survival day by day, and on the other, to prepare the logistics to fight Rwanda in a distant future. With Kimia II, we struggled to survive because we no longer received taxes, nor levies. Kimia II impacted us negatively by depriving us of our source of finance." *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

We had to do incursions in these villages to find means of subsistence." *Source*: interviews with FDLR members who were present in the Chiefdom of Basile in 2013 (2022).

B. TABLES AND FIGURES

B.1. *Tables*

Table B.1: Quasi-Experimental Sample and Confounding Villages, Before Kimia II

	All	Mean outcomes			P-value		
		FDLR State	Rest	Confounder	1 vs. 2	1 vs. 3	2 vs. 3
Observations		36	350	92			
Panel A: Village Characteristics							
Village Accessible on Foot, by Motorcycle, and by Car	0.44	0.00	0.56	0.17	0.00	0.01	0.00
Village Only Accessible by Motorcycle or on Foot	0.33	0.22	0.30	0.50	0.34	0.00	0.00
Village Only Accessible on Foot	0.23	0.78	0.14	0.33	0.00	0.00	0.00
Access to Phone Network	0.36	0.17	0.46	0.10	0.00	0.31	0.00
Distance to Rwanda (km)	103.23	75.59	93.64	150.52	0.08	0.00	0.00
Distance to River (km)	4.34	5.81	4.46	3.33	0.06	0.00	0.02
Distance to Road (km)	1.44	2.49	1.20	1.94	0.01	0.38	0.02
Distance to Airport (km)	18.41	13.28	20.92	10.85	0.00	0.05	0.00
Endowed with Coltan Mine	0.15	0.50	0.06	0.35	0.00	0.11	0.00
Endowed with Gold	0.30	0.44	0.23	0.48	0.01	0.73	0.00
% of Household Survey Respondents in Farming	0.48	0.39	0.48	0.51	0.30	0.06	0.50
% of Household Survey Respondents in Mining	0.15	0.13	0.18	0.11	0.50	0.56	0.10
Panel B: FDLR Expropriation Strategy							
FDLR Expropriation Frequency (# Days per Year)	26.44	347.67	0.34	0.03	0.00	0.00	0.45
Monopoly of Violence	0.08	1.00	0.01	0.00	0.00	.	0.37
Poll Tax	0.07	0.94	0.00	0.00	0.00	0.00	.
Toll Fees for Transit	0.07	0.83	0.01	0.00	0.00	0.00	0.47
Market Tax	0.02	0.28	0.00	0.00	0.00	0.00	.
Total Value Taxed per Household, Yearly (USD)	4.99	64.97	0.13	0.00	0.00	0.00	0.47
Panel C: Security Outcomes							
Attack by any Actor	0.12	0.06	0.13	0.08	0.19	0.69	0.15

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, Rest, and Confounder show the means for the FDLR State, the rest of the quasi-experimental villages, and for the confounding villages, respectively. All variables, unless otherwise noted, are binary indicators. P-value columns report the p-value of the t-test for whether the mean across columns is different. When the respondent has multiple occupations, we report the occupations that are their main occupations, according to the respondent. We omit the mill tax because there are no mill taxes collected in the years used for this table. The data are aggregated at the level of village-year observations. We use two years of pre-Kimia II, hence there are 386 observations in the quasi-experimental villages (two years for the averages of 193 villages), and 92 in the confounding villages (46 villages for two years).

Table B.2: Robustness Analysis in Differences-in-Differences

	Dependent Variable: *Attack by FDLR*											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\text{FDLR}_{\text{i}} \times 1(t > 2009)$	0.240***	0.181**	0.238***	0.272***	0.216**	0.233***	0.268***	0.276***	0.425***	0.240***	0.184***	0.406*
	(0.068)	(0.071)	(0.067)	(0.065)	(0.099)	(0.068)	(0.072)	(0.072)	(0.125)	(0.048)	(0.056)	(0.215)
Village FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Confounding Villages	N	Y	N	N	N	N	N	Y	N	N	N	N
District-Year FE	N	N	Y	N	N	N	Y	Y	N	N	N	N
Village Year Trends trends	N	N	N	Y	N	N	Y	Y	N	N	N	N
Pre-treatment Controls $\times$ Year	N	N	N	N	Y	N	Y	Y	N	N	N	N
Control for Migration	N	N	N	N	N	Y	Y	Y	N	N	N	N
Conditional on Gold Village	N	N	N	N	N	N	N	N	Y	N	N	N
Conley (1999) SE	N	N	N	N	N	N	N	N	N	Y	N	N
Borusyak et al. (2023) Estimator	N	N	N	N	N	N	N	N	N	N	Y	N
ACLED	N	N	N	N	N	N	N	N	N	N	N	Y
Observations	3,474	4,302	3,456	3,474	3,096	2,554	2,297	3,002	870	3,474	3,474	3,088
R^2	0.14	0.13	0.17	0.14	0.14	0.17	0.20	0.19	0.20	0.19		0.49
Mean Dep. Var.	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.00	0.07	0.07	0.39
Village Clusters	193	239	193	193	193	193	193	239	49	193	193	193
Chiefdom-Year Clusters	360	378	360	360	360	360	360	378	180	360	360	320

Notes: This table presents the 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, two-way clustered 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. *Confounding Villages*: includes the confounding villages. *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 separately for each village. *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 estimate the predicted probability that a village belongs to the FDLR state, we estimated a probit model for an indicator for whether the village is in the FDLR state on indicators for the distance to Rwanda, the closest river, the closest road, the closest airport and whether the village has coltan or gold. 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. *Conley (1999) SE:* adjusts standard errors for spatial correlation (within a radius on 100km) and serial correlation (with an AR(1) process), following Hsiang (2010). Estimates with different assumptions on the radius of the spatially correlated errors are in Panel A of Figure B.5, and estimates with different assumptions on the number of lags in Panel B. *Borusyak et al. (2023) Estimator:* estimates the average treatment effect using Borusyak et al. (2023)'s estimator. *ACLED:* uses whether there was an FDLR attack reported within 50km of the village i in year t in ACLED as the outcome variable. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in the standard error estimation. *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 B.3: Robustness to Econometric Specification and Outcome Modelling

	Dependent Variable: *Attack by FDLR*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{FDLR}_i \times 1(t > 2009)$	0.240***	1.993***	1.760***	1.766***	1.067***	1.315**	0.208**	0.194***
	(0.068)	(0.537)	(0.592)	(0.599)	(0.281)	(0.525)	(0.096)	(0.069)
Village FE	Y	N	N	Y	N	N	Y	Y
Year-Province FE	Y	N	Y	Y	N	N	Y	Y
Cluster IDV	Y	N	N	N	N	N	Y	Y
Cluster Chiefdom-Year	Y	N	N	N	N	N	Y	Y
Outcome Coding	Binary	Binary	Binary	Binary	Binary	Count	Count	H-Sine
Specification	OLS	Logit	Logit	Logit	Hausman et al. (1998)	Neg. Binomial	OLS	OLS
Observations	3,474	3,474	2,944	1,782	3,474	3,474	3,474	3,474
R^2	0.14						0.11	0.13
Mean Dep. Var.	0.07	0.07	0.07	0.07	0.07	0.11	0.11	0.08
Village Clusters	193	193	193	193	193	193	193	193
Chiefdom-Year Clusters	360	360	360	360	360	360	360	360

Notes: This table presents the estimates from Equation 2, with variations in the specification. *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. *Cluster Village*: clusters standard errors at the village level. *Cluster Chiefdom-Year*: clusters standard error at the Chiefdom-year level (two-way clustering if there is also clustering at the village level). *Outcome Coding*: Indicates whether the outcome variable is a binary indicator taking value 1 in village i and year t if village i is attacked by the FDLR in year t, and zero otherwise, or the inverse hyperbolic sine trans-formation of the count variable. *Specification*: indicates whether OLS, Conditional Logit, the method proposed by Hausman et al. (1998), or Negative Binomial is implemented. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in the standard error estimation. *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. In Column (4), the number of observations drops because for 94 villages (1,692 obs) there are no FDLR attacks in 1995–2012, so their village fixed effects perfectly predict success/failure.

Table B.4: Testing for Selection on Observable Characteristics

	Dependent Variable: *FDLR Attack*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.241***	0.258***	0.256***	0.243***	0.240***	0.240***	0.242***	0.260***	0.233***
	(0.070)	(0.071)	(0.071)	(0.067)	(0.068)	(0.070)	(0.067)	(0.070)	(0.063)
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	3,041	3,041	3,025	3,474	3,474	3,041	3,474	3,474	3,474
R^2	0.16	0.15	0.16	0.14	0.15	0.16	0.15	0.14	0.14
Mean Dep. Var.	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07
Village Clusters	193	193	193	193	193	193	193	193	193
Chiefdom-Year Clusters	360	360	360	360	360	360	360	360	360

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 the standard error estimation. *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 B.5: Alternative Econometric Specifications

	Dependent Variable: *FDLR Attack*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.245***	0.235***	0.240***	0.240***	0.239***	0.236***	0.270**	
	(0.064)	(0.067)	(0.018)	(0.049)	(0.067)	(0.066)	(0.122)	
$\mathbf{1}(t > 2009)$								0.148**
								(0.063)
Log(Distance to FDLR State+1)$_\text{i}$ $\times 1(t > 2009)$								-0.033***
								(0.013)
Village FE	N	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	N	Y	Y	Y	Y	Y	Y	N
Village Clustering	Y	Y	N	Y	Y	Y	Y	Y
Chiefdom-Year Clustering	Y	Y	Y	N	Y	Y	Y	Y
Chiefdom-Post Clustering	N	N	N	Y	N	N	N	N
Groupement Clustering	N	N	Y	N	N	N	N	N
Controls	FDLR_i & $1(t > 2009)$	N	N	N	Coltan Price	Gold Price	Dist. Basile	N
Observations	3,474	3,456	3,474	3,474	3,368	3,368	3,456	3,456
R^2	0.02	0.14	0.14	0.14	0.14	0.14	0.14	0.11
Mean Dep. Var.	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07
Village or Groupement Clusters	193	192	61	192	193	193	193	193
Chiefdom-Year Clusters	360	360	360	40	360	360	360	360

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 Groupement level (the unique administrative level below the Chiefdom and above the village), (4) clusters standard errors at the Chiefdom-Post level instead of Chiefdom-Year, (5) includes the yearly world coltan price and (6) the gold price interacted with whether the village has coltan/gold, (7) controls for Log(Distance to FDLR State+1)$_\text{i}$ where Distance to FDLR State is the distance between village i and the FDLR state (in km) multiplied with year indicators. In this regression, all villages in the FDLR state have zero distance. In Column (8), $\text{FDLR}_\text{i} \times 1(t > 2009)$, is replaced with Log(Distance to FDLR State+1)$_\text{i}$ $\times 1(t > 2009)$. For transparency, in that column, we omit the Province-Year fixed effects. This allows interpreting the coefficient on $1(t > 2009)$ 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 the standard error estimation. *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 B.6: Specification with Lagged Dependent Variable

	Dependent Variable: *Attack by FDLR*										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.202***	0.205***	0.225***	0.202***	0.193**	0.204***	0.197***	0.206***	0.367***	0.202***	0.170
	(0.055)	(0.056)	(0.050)	(0.055)	(0.089)	(0.054)	(0.054)	(0.058)	(0.106)	(0.043)	(0.182)
Village FE	N	N	N	N	N	N	N	N	N	N	N
Year-Province FE	N	N	N	N	N	N	N	N	N	N	N
Confounding Villages	N	Y	N	N	N	N	N	Y	N	N	N
District-Year FE	N	N	Y	N	N	N	Y	Y	N	N	N
Village Year Trends trends	N	N	N	Y	N	N	Y	Y	N	N	N
Pre-treatment Controls × Year	N	N	N	N	Y	N	Y	Y	N	N	N
Control for Migration	N	N	N	N	N	Y	Y	Y	N	N	N
Conditional on Gold Village	N	N	N	N	N	N	N	N	Y	N	N
Conley (1999) SE	N	N	N	N	N	N	N	N	N	Y	N
ACLED	N	N	N	N	N	N	N	N	N	N	Y
Observations	3,281	4,063	3,247	3,281	2,924	2,440	2,179	2,852	824	3,281	2,895
R^2	0.04	0.03	0.11	0.05	0.06	0.07	0.14	0.12	0.08	0.10	0.06
Mean Dep. Var.	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.00	0.07	0.39
Village Clusters	193	239	193	193	193	193	193	239	49	193	193
Chiefdom-Year Clusters	360	378	360	360	360	360	360	378	180	360	300

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, two-way clustered 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. *Confounding Villages*: includes the confounding villages. *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 separately for each village. *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 the standard error estimation. *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 B.7: Validating Data

	Dependent Variable: *FDLR Attack*											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$FDLR_i \times 1(t > 2009)$	0.240***	0.238***	0.179**	0.172**	0.183***	0.183***	1.187***	1.187***	1.188***	1.188***	1.184***	1.184***
	(0.068)	(0.067)	(0.070)	(0.068)	(0.050)	(0.064)	(0.116)	(0.254)	(0.119)	(0.185)	(0.165)	(0.254)
$FDLR_i$							-0.760***	-0.760***	-1.044***	-1.044***		
							(0.075)	(0.116)	(0.095)	(0.142)		
$\mathbf{1}(t > 2009)$							-0.812***	-0.812***	-0.813***	-0.813***	-0.812***	-0.812***
							(0.099)	(0.146)	(0.101)	(0.093)	(0.140)	(0.129)
Village FE	Y	Y	Y	Y	Y	Y	N	N	N	N	Y	Y
Year-Province FE	Y	N	N	N	Y	Y	N	N	N	N	N	N
Village Clustering	Y	Y	Y	Y	Y	Y	Y	N	Y	N	Y	N
Chiefdom-Year Clustering	Y	Y	Y	Y	Y	N	N	N	N	N	N	N
Groupement-Year Clustering	N	N	N	N	N	Y	N	Y	N	Y	N	Y
Year of Data Collection	2013–2015	2013–2015	2022	2022	2013–2015 & 2022	2013–2015 & 2022	2022	2022	2022	2022	2022	2022
Sample	Full Sample	Mwenga Only	Mwenga Only	Mwenga Only +	Full Sample +	Full Sample +	Basile	Basile	Basile	Basile	Basile	Basile
Observations	3,474	612	442	715	3,747	3,747	189	189	189	189	189	189
R^2	0.14	0.14	0.22	0.19	0.16	0.16	0.26	0.26	0.58	0.58	0.73	0.73
Mean Dep. Var.	0.07	0.07	0.24	0.17	0.06	0.06	0.18	0.18	0.18	0.18	0.18	0.18
Village Clusters	193	34	34	55	214	214	95	95	95	95	95	95
Chiefdom-Year Clusters	360	36	26	26	360							
Groupement-Year Clusters						1137	20	20	20	20	20	20
Baseline estimation p-value	0.00	0.01	0.07	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Bootstrapped p-value	0.00	0.01	0.04	0.02	0.01	0.00	0.00	0.04	0.00	0.09	0.00	0.00
Post + Post × Observable = 0							0.37***	0.37*	0.37***	0.37**	0.37***	0.37
							(0.06)	(0.21)	(0.06)	(0.16)	(0.08)	(0.22)

Notes: This table reports the coefficient estimates from Equation 2, but changes the sample used. 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. Columns 1–6 validate the persistence sample collected in 2022. Column 1 is our baseline specification using the full sample. Column 2 restricts our sample to only villages in the district of Mwenga. Column 3 also restricts to the district of Mwenga but uses data collected in 2022. Column 4 uses the Mwenga data collected in 2022 as well as 21 additional villages for which data were also collected 2022. Columns 5–6 use the original sample as well as 21 additional villages for which data were collected 2022. All columns only use observations up until 2012. Standard errors are in parentheses. For Columns 1–5 standard errors are two-way clustered at the village level and the Chiefdom-year level. In Columns 2, 3, and 4 there are only 26 to 36 Chiefdom-Year clusters so we bootstrap the standard errors and report the p-values we obtain in the last row. Column 6 standard errors are two-way clustered at the village level and the Groupement-year level since in this extended sample treatment is clustered at the Groupement-year level. Columns 7–12 use the universe of villages from Basile Chiefdom. The dependent variable is an indicator variable if a village was attacked by the FDLR in period t. There are two periods per village, before and after Kimia II. *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. *Years of Data Collection:* lists the years of data collection for the sample used. *Sample:* describes which villages are included. *Years:* gives a range of years for which data are included. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in the standard error estimation. *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. The new data does not have 1995 to 1999, hence in columns (3),(4) the sample starts in 2000. Columns (1),(2) use our main sample, hence the sample in those columns starts in 1995. Columns (5) and (6) have both main sample and newly collected data. The sample in those columns thus starts in 1995 but then is missing for the new villages in 1995–1999. Interestingly, the coefficient on $FDLR_i$ in Columns (7)-(12), which can exploit within-Basile variation in the location of the FDLR before Kimia II due to the specific (and lower quality) source of data collected in 2022, also shows that the villages of Basile controlled by the FDLR were hugely less likely to be pillaged by the FDLR before Kimia II. This finding, also visible in Figure B.4, is consistent with the main idea of the paper that the FDLR had developed an encompassing interest in the villages it taxed, dis-incentivizing it from arbitrarily pillaging them. In Figure B.4, we see clearly how the FDLR controlled many remote villages before Kimia II and pillaged the villages it did not control, many of which were along the road. The qualitative interviews shown in Section A.2.1 also support that, while the FDLR protected the villages it taxed before Kimia II, it pillaged villages it did not control, sometimes sharing the spoils with the population in the villages it controlled.

Table B.8: Mechanisms—Descriptive Statistics of Attacks, by Attack Type

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

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

Table B.9: 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)
$\text{FDLR}_\text{i} \times \mathbf{1}(t > 2009)$	0.205**	-0.289***	-0.110	0.042*	-0.322***	-0.443***	-0.071
	(0.086)	(0.110)	(0.097)	(0.024)	(0.110)	(0.116)	(0.061)
Village FE	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y	Y
Observations	3,330	3,326	3,330	3,330	3,330	3,330	3,330
R^2	0.13	0.49	0.48	0.34	0.52	0.47	0.63
Mean Dep. Var.	0.04	1.00	0.28	0.00	0.83	0.94	0.67
Village Clusters	185	185	185	185	185	185	185
Chiefdom-Year Clusters	360	360	360	360	360	360	360

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, two-way clustered 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 the standard error estimation. *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 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 B.10: Sun City: Differences-in-Differences Estimate

	Dependent Variable: *Attack by MM or RCD*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{SC}_\text{i} \times 1(t > 2002)$	-0.069***	-0.052**	-0.124***	-0.118***	-0.060**	-0.178***	-0.069***
	(0.023)	(0.021)	(0.041)	(0.026)	(0.028)	(0.048)	(0.022)
Village FE	Y	Y	Y	Y	Y	Y	Y
Year-Province FE	Y	Y	Y	Y	Y	Y	Y
District-Year FE	N	Y	N	N	N	Y	N
Village Year Trends trends	N	N	Y	N	N	Y	N
Pre-treatment Controls × Year	N	N	N	Y	N	Y	N
Control for Migration	N	N	N	N	Y	Y	N
Conley (1999) SE	N	N	N	N	N	N	Y
Observations	4,302	4,284	4,302	3,924	3,259	3,002	4,302
R^2	0.161	0.250	0.162	0.130	0.213	0.332	0.214
Mean Dep. Var.	0.19	0.19	0.19	0.19	0.19	0.19	0.19
Village Clusters	239	239	239	239	239	239	239
Chiefdom-Year Clusters	378	378	378	378	378	378	378

Notes: This table presents the 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 affected factions (Mayi-Mayi or RCD) in year t, and zero otherwise, and $FDLR_i \times 1(t > 2009)$ is replaced by $SC_i \times 1(t > 2002)$, which is an indicator taking value 1 if the village was controlled by an affected faction in 2002 prior to the peace agreement. Standard errors, two-way clustered 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. *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 separately for each village. *Pre-treatment Controls × Year:* include controls for all characteristics in 2003 that predict that the location is affected by the peace agreement, interacted with indicators for years. To estimate the predicted probability that a village belongs to the affected faction in 2002, we estimated a probit model for an indicator for whether the village belongs to the affected faction in 2002 on indicators for the distance to Rwanda, the closest river, the closest road, the closest airport and whether the village has coltan or gold. Then, for each village, we estimate the predicted probability that the village affected based on the vector of characteristics. We then estimate Equation 2, but include, as controls, this 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. *Conley (1999) SE:* includes Conley (1999) standard errors, assuming an AR(1) process with radii of 100km. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in the standard error estimation. *Mean Dep. Var.:* is the mean of the dependent variable in the villages controlled by the affected factions prior to the agreement.

Table B.11: Sun City: Implications for Household Welfare

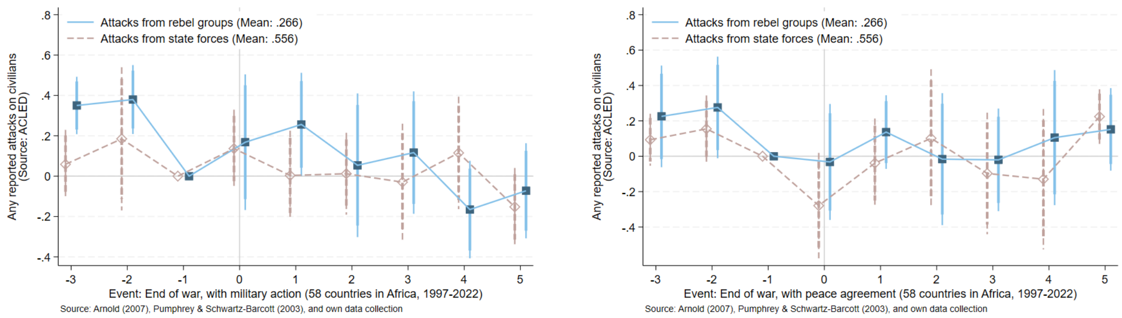
	Dep. Var.: *Disutility of Violence*				Dep. Var.: *Household Transfers to Armed Actors (USD)*					
	Village experiences:				*Pillage*	*Taxation*				*Total*
VARIABLES	*Rape* (1)	*Death* (2)	*Looting* (3)	*Kidnapping* (4)	*Theft* (5)	*Market* (6)	*Toll* (7)	*Poll* (8)	Total (9)	Total (10)
$SC_i \times 1(t > 2002)$	-0.032	-0.024	-0.039	-0.007	-0.823	-1.216**	-2.958**	-6.504	-10.259*	-6.305
	(0.020)	(0.021)	(0.026)	(0.018)	(0.822)	(0.481)	(1.181)	(5.514)	(6.199)	(7.123)
Village FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	4,302	4,302	4,302	4,302	3,942	3,934	3,934	3,934	3,934	3,934
R^2	0.117	0.116	0.113	0.100	0.101	0.477	0.470	0.325	0.344	0.292
Mean Dep. Var.	0.06	0.09	0.09	0.06	0.7	1.988	9.972	31.64	45.84	46.50
Village Clusters	239	239	239	239	239	239	239	239	239	239
Chiefdom-Year Clusters	378	378	378	378	378	378	378	378	378	378
Conley (1999) p-value	0.06	0.30	0.08	0.68	0.30	0.00	0.00	0.16	0.06	0.27

Notes: This table reports the coefficient estimates from Equation 2, where $FDLR_i \times 1(t > 2009)$ is replaced by $SC_i \times 1(t > 2002)$, which is an indicator taking value 1 if the village was controlled by an affected faction (Mayi-Mayi or RCD) in 2002 prior to the peace agreement. The dependent variables in columns (1)–(4) are indicator variable listed in the headers. The dependent variables in columns (5)–(10) are continuous variables in USD listed in the headers. Standard errors, two-way clustered 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. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in the standard error estimation. *Mean Dep. Var.:* is the mean of the dependent variable in the targeted villages prior to the agreement. *, **, *** indicate that the corresponding coefficient is statistically significant at the 10%, 5%, and 1% levels, respectively.

B.2. *Figures*

Figure B.1.: postcolonial African States' Endings of Civil Wars, and Rebels Violence

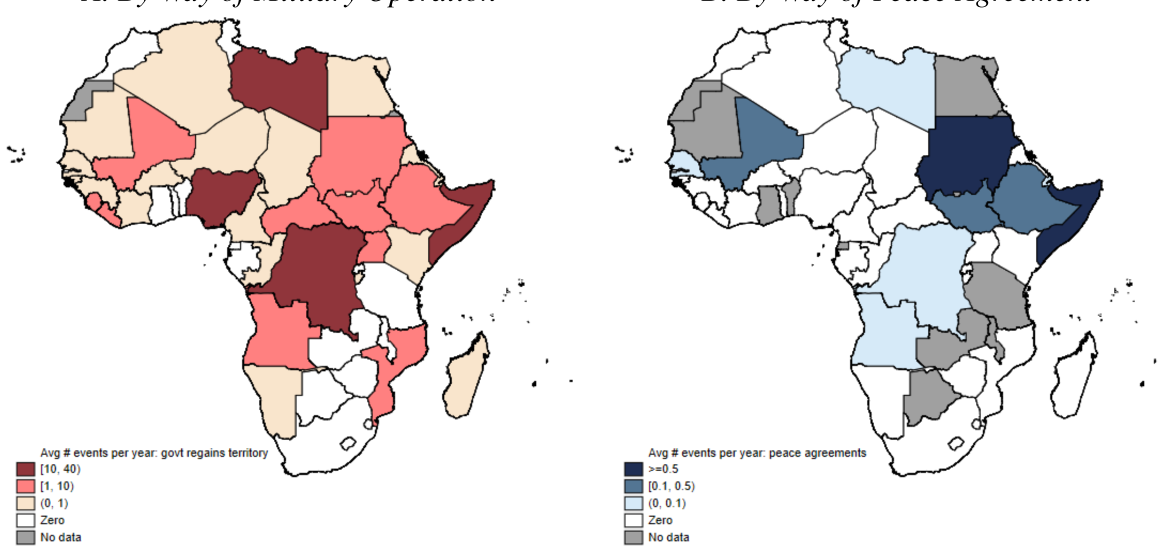
A. By Way of Military Operation *B. By Way of Peace Agreement*



Notes: This figure presents the event study estimates for rebel violence around the events of civil wars ending with the postcolonial state regaining territory by military operation (Panel A) or regaining territory by signing of a peace agreement (Panel B). The event study estimates are constructed as follows. The dependent variable is an indicator variable taking value one if there is at least one reported attack in ACLED in country j in year t. There are no control variables, but the estimating equation includes country and year fixed effects (58 countries, 1997–2002, only missing data in disputed districts like Western Sahara). Standard errors clustered at the level of the country. *Source*: authors' reconstruction using historical publications and ACLED (2022). The pattern is robust to adding controls of mutiny and armed clash events and to controlling end-of-war episodes ending with peace agreement. It is not robust to using continuous count dependent variable. The analysis is unchanged if we drop from the sample the countries that have multiple episodes.

Figure B.2.: postcolonial African States' Regaining Territory from Rebels

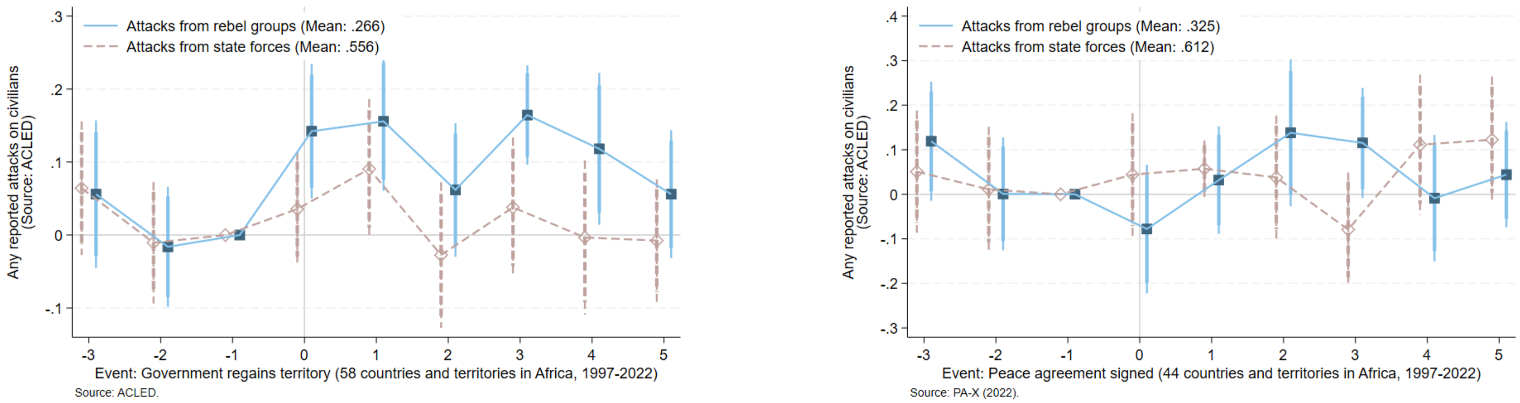
A. By Way of Military Operation *B. By Way of Peace Agreement*



Notes: This figure presents the incidence across countries of events in which the postcolonial state regained territory by military operation (Panel A) and the prevalence of peace agreements signed by postcolonial states to regain territory from rebels (Panel B). The former uses data from ACLED (2022), covering the years 1997 through today; the latter uses data from Bell and Badanjak (2019), where we restrict the data to cover the years 1997 through today for comparability with Panel A.

Figure B.3.: Effect of States' Regaining Territory on Rebel Violence Against Civilians

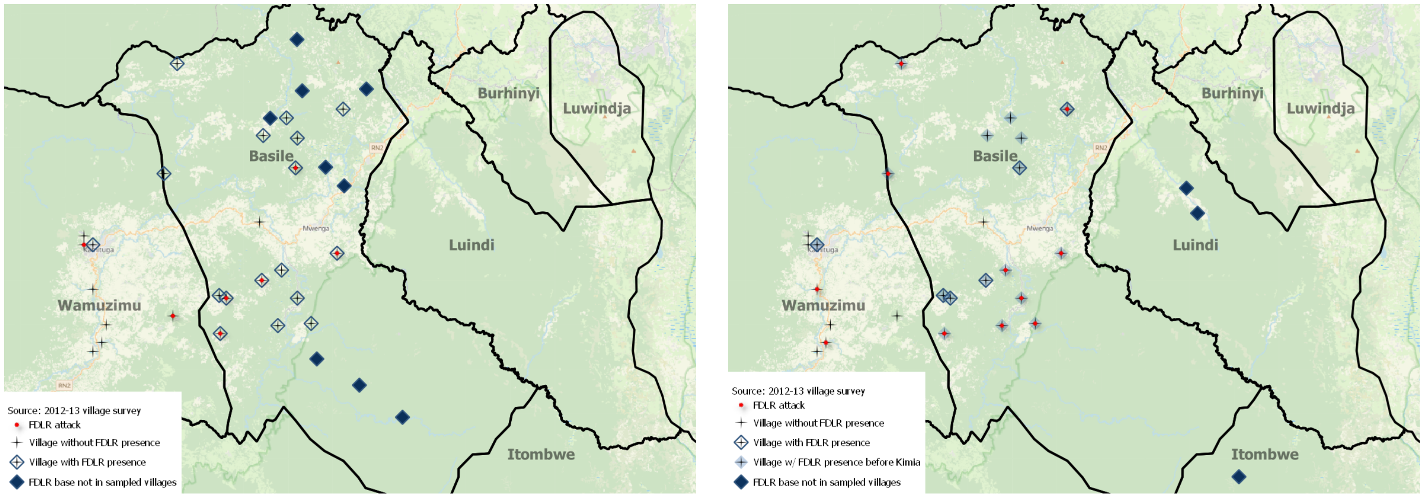
A. When the States Regained Territory by Force *B. When the States Regained Territory by Bargain (Peace Agreements)*



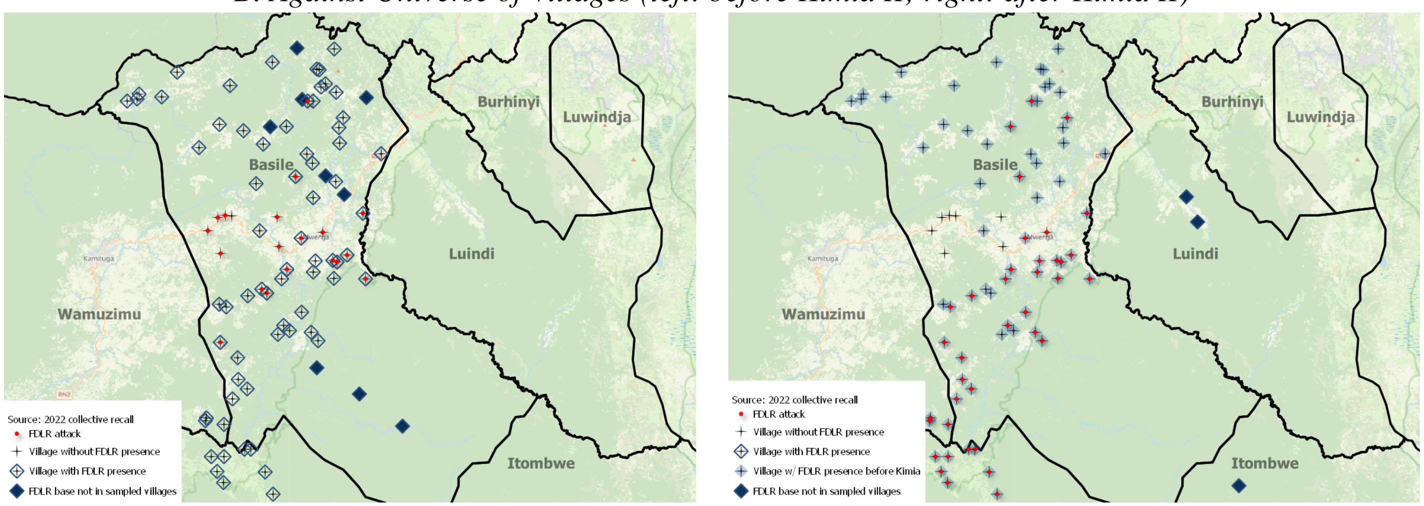
Notes: This figure shows estimates for the coefficients β_k estimated with OLS from the following equation: $Y_{it} = \alpha_i + \alpha_t + \sum_{k=-3}^{k=5} \beta_k \, 1(t = r_{it} + k) + \epsilon_{it}$ where α_i and α_t are country and year fixed effects, respectively. The dependent variable Y_{it} is an indicator for whether the rebels attack civilians in country i in year t. Standard errors are clustered at the country level (58 countries). We include dummies for mutiny and clashes. In Panel A, the indicator $1(t = r + k)$ takes value 1 if the year is $t = r + k$, where r indicates the year of the event in which the state regains country i from the rebels. In that panel, for military events in which the state regains the territory, we use ACLED records of "government regains territory" as a proxy. The event is defined as whether the country has at least one reported event "government regains territory" in a given year. The dependent variable is an indicator variable taking value 1 if there is at least one reported attack on civilians in ACLED (2022) in the country-year. There are no other control variables, except country and year fixed effects. There are 58 countries, years range from 1997 to 2022. Data are missing only for disputed districts, such as Western Sahara. We cluster the standard errors at the level of the country. The pattern is robust to adding controls of mutiny and armed clash events, and to using continuous count dependent variable (although the estimates are slightly less precise). In Panel B, the indicator $1(t = r + k)$ takes value 1 if the year is $t = r + k$, where r indicates the year of an intra-state peace agreement in country i associated leading the state to regain territory from the rebels. In that panel, for peace agreements, the dependent variable is an indicator variable taking value 1 if there is at least one reported attack on civilians in ACLED (2022). There are no other control variables, except country and year fixed effects. There are 44 countries spanning the years 1997–2022. Overall, there are 14 countries with no data in this analysis, that is, they are never observed through the entire dataset even outside this time span. We cluster the standard errors at the level of the country. The pattern is robust to adding controls for mutiny and armed clash events. It is not robust to using continuous count dependent variable. The dip in $t = 0$ becomes insignificant and the rebel violence slightly increases as t increases, however, the increase is not statistically significant either, underscoring the main descriptive conclusion.

Figure B.4.: FDLR camps and Villages of Basile, Using all Sources of Information we Collected from the Field

A. Against survey villages (left: before Kimia II; right: after Kimia II)

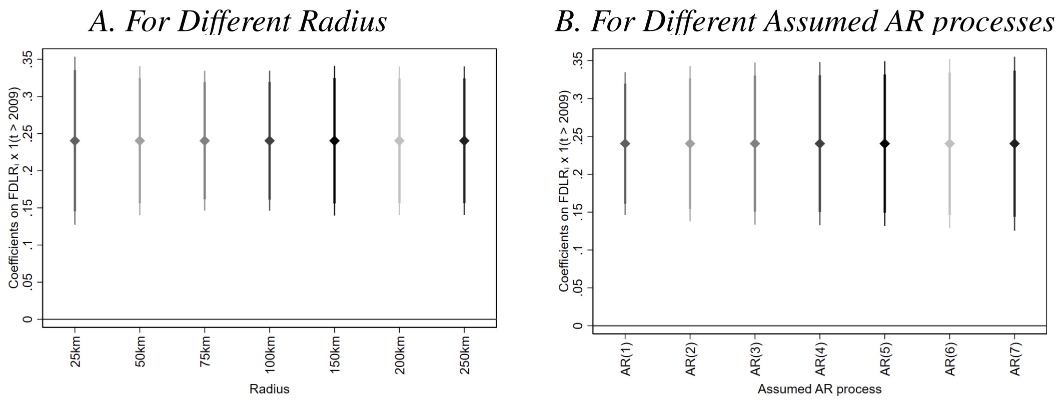


B. Against Universe of Villages (left: before Kimia II; right: after Kimia II)



Notes: This figure zooms into the Chiefdom of Basile and shows the FDLR camps that formed the bases of the FDLR after the Kimia II operation was complete and from where they conducted the attacks. **Source** for the FDLR camps: direct interviews with former FDLR members, conducted in November 2022 in the district of Masisi; the FDLR members we interviewed, through an intermediary that is close to the FDLR, had previously themselves occupied Basile prior to Kimia II and moved to those camps. Source for the survey villages: we use the main data source of this paper collected in 2013. **Source** for the extra villages: in October–December 2022, we implemented a scoping exercise whereby teams of data collectors went through Groupement capitals, reconstructing the universe of villages in the area, and obtaining approximate information about the FDLR location before and after Kimia II. The latter source is imprecise, collected 14 years after Kimia II, and does not allow to distinguish between years (only before vs. after Kimia II). For this reason, it cannot be used for primary analysis. However, we show in Table B.7 that the baseline Difference-in-Differences estimates for the effect of Kimia II on the likelihood that a village is attacked by the FDLR are much larger and are more precisely estimated than using the survey. This figure shows clearly how the FDLR controlled many remote villages before Kimia II and pillaged the villages it did not control, many of which were along the road, consistent with qualitative interviews shown in Section A.2.1 and with the paper's argument that the FDLR had developed an encompassing interest in the villages it could tax in Basile.

Figure B.5.: Accounting for Spatially Correlated Shocks with Conley (1999) Standard Errors



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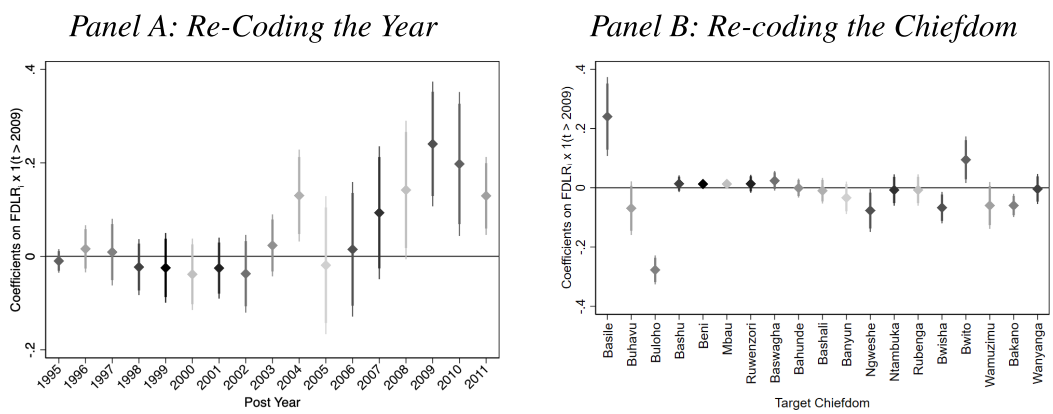
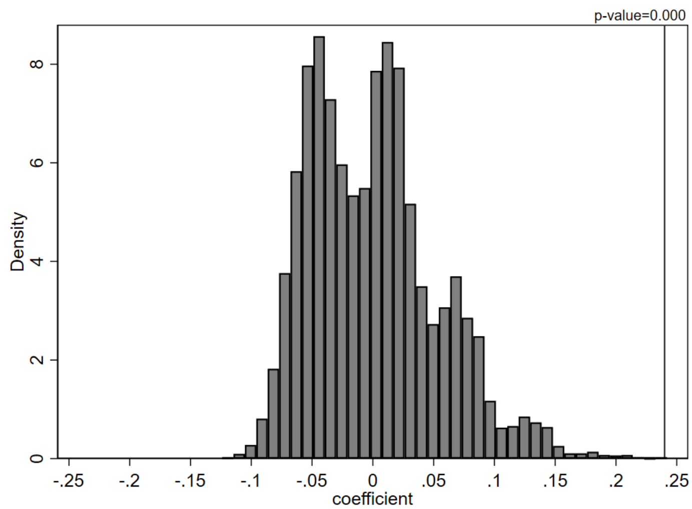
Notes: This figure shows the coefficients and standard errors estimated from Equation 2 but including Conley (1999) Standard Errors. Panel A shows standard errors for AR process 1, varying the radius (from 25km to 250km). Panel B shows standard errors with 100km radius, varying the AR process (from 1 to 7 lags).

Figure B.6.: Alternative Treatment Definitions



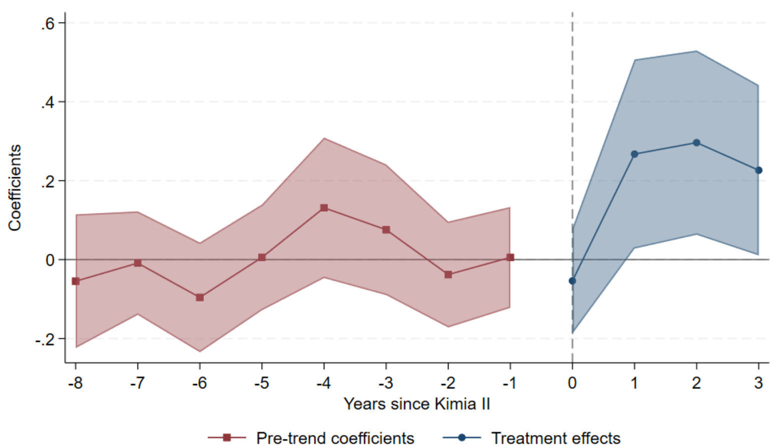
Notes: Panel A estimates Equation 2 for each possible cutoff year in defining the variable Post. The cutoff years for $1(t > 2009)$ are reported in the x-axis, while the y-axis are the magnitude of each coefficient and standard errors. Panel B does the same for each administrative division called Chiefdom. Since the FDLR state controlled an entire Chiefdom, we re-estimate Equation 2 for each Chiefdom in our sample as the targeted area, FDLR_i. In all panels, thick lines represent 90% confidence intervals and thin lines represent 95% confidence intervals. Standard errors are two-way clustered at the village level and the Chiefdom-Year level.

Figure B.7.: 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.

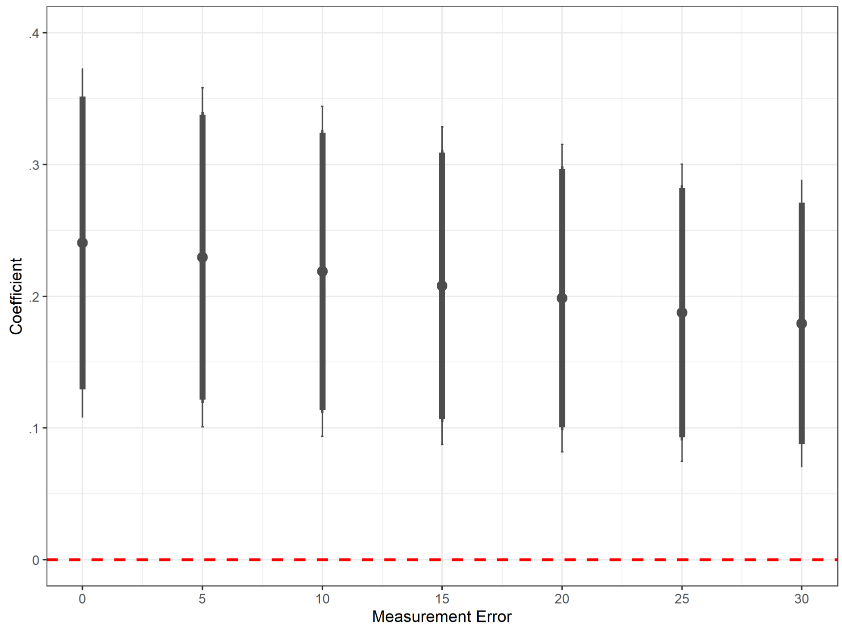
Figure B.8.: Pre-trends



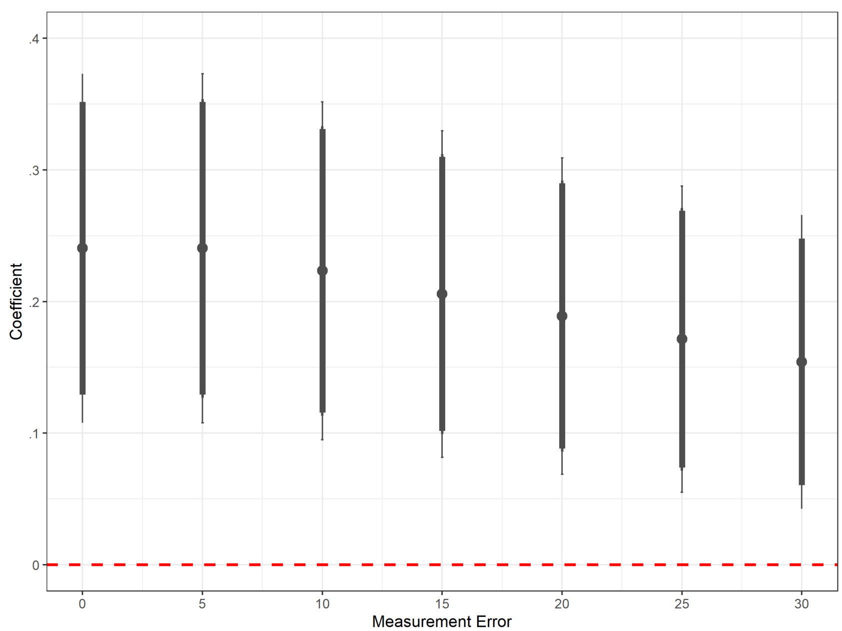
Notes: This figure shows the coefficients $\beta_k, k = -8, \ldots, 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2023) estimator. The regression includes village and year fixed effects. The dependent variable is an indicator for whether the FDLR attacks village i in year t. The reference group for estimation is all pre-treatment (or never-treated) observations. The reference group for the pretrend test is all periods more than k periods prior to the event date (and all never-treated observations). 3,474 village-year observations were used in the estimation. Standard errors are two-way clustered at the village level and the Chiefdom-Year level.

Figure B.9.: Simulated Non-Classical Measurement Error

A. Overreporting of Attacks in FDLR State



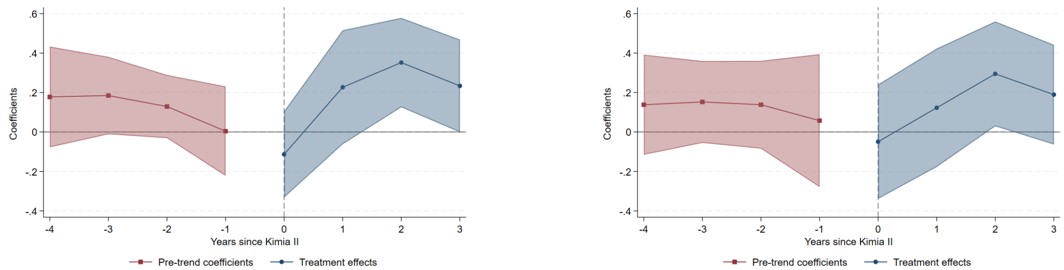
B. Overreporting of Attacks in FDLR State in Post



Notes: This figure shows how the coefficients on $FDLR_i \times \mathbf{1}(t > 2009)$ change when increasing the rate of non-classical measurement error. The first coefficient at 0% measurement error is our baseline result from Equation 2. Each coefficient is taken from the distribution of 1,000 coefficients resulting from the 1,000 regressions following specification 2. Before each regression 5–30% of random measurement is induced. Panel A re-codes a random subset of attacks in the FDLR state as non-attacks. Panel B re-codes a random subset of attacks in the FDLR state after Kimia II as non-attacks.

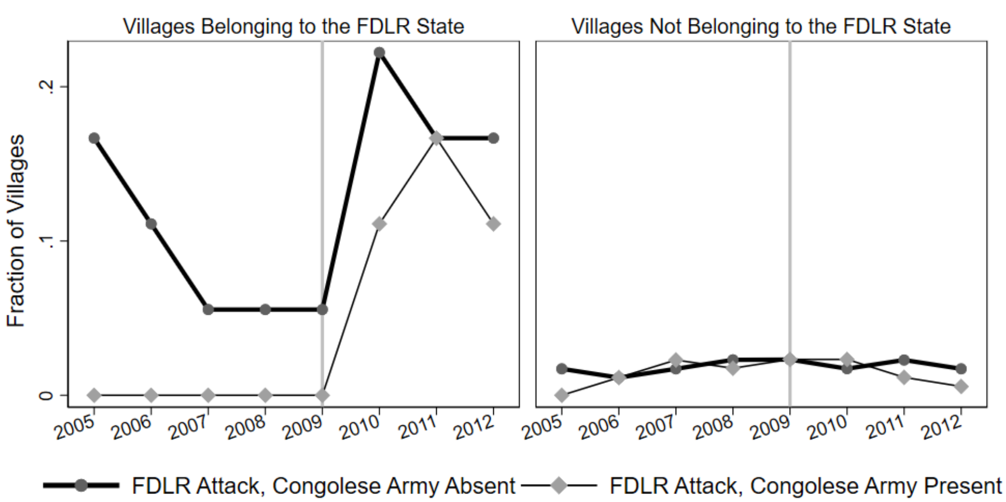
Figure B.10.: Validating the 2022 data collection with the 2013 data collection

A. Original Sample, Basile and Wamuzimu *B. Persistence Sample, Basile and Wamuzimu*



Notes: This figure shows the coefficients on year indicators estimated from Equation 1 using Borusyak et al. (2023) estimator. Panel A uses the data for our main specification collected in 2013–2015 but restricts to the Chiefdoms Basile and Wamuzima in Mwenga district. Panel B uses data collected in 2022 in the same villages as Panel A.

Figure B.11.: Trends of FDLR Taxation and Attacks, by Congolese Army Control



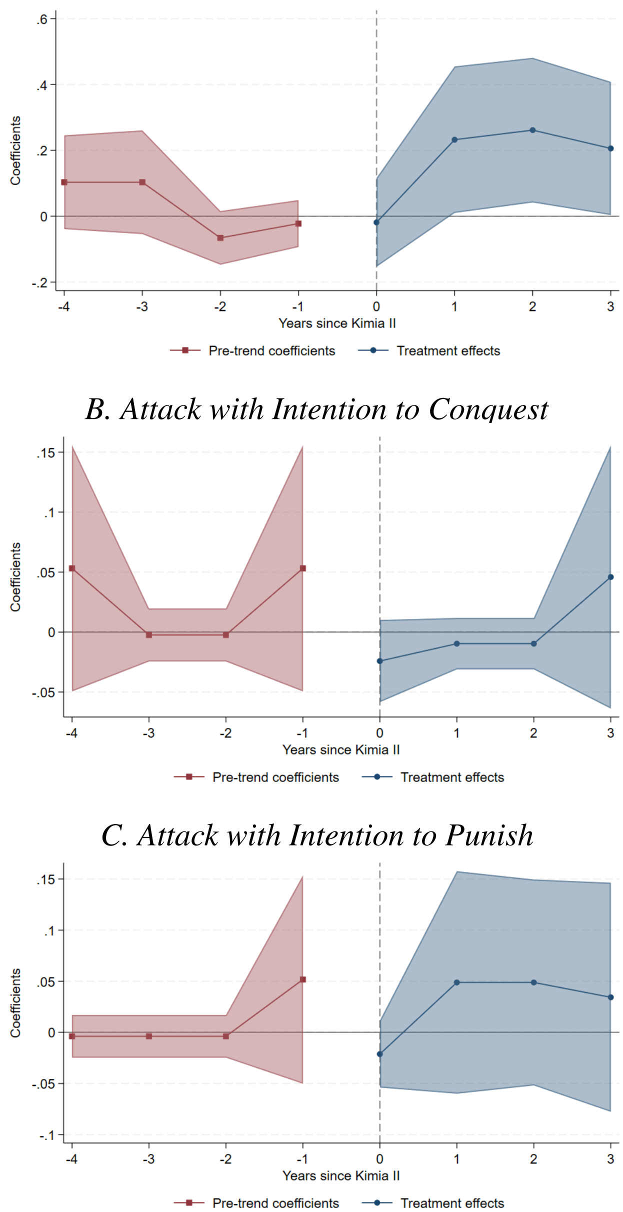
Notes: This figure shows the incidence of FDLR attacks separately for whether the Congolese army controlled the village and was present in the village at the time of the attack and whether the reported attack took place in a village either not controlled by the Congolese army or controlled by the Congolese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

Figure B.12.: Event Study Estimates, by Type of Attack

A. Attack with Intention to Pillage

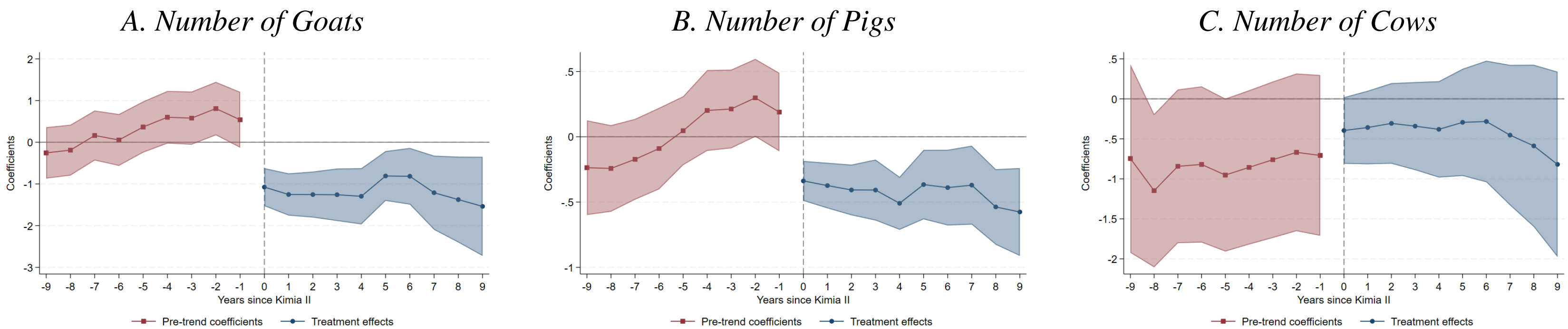
B. Attack with Intention to Conquest

C. Attack with Intention to Punish



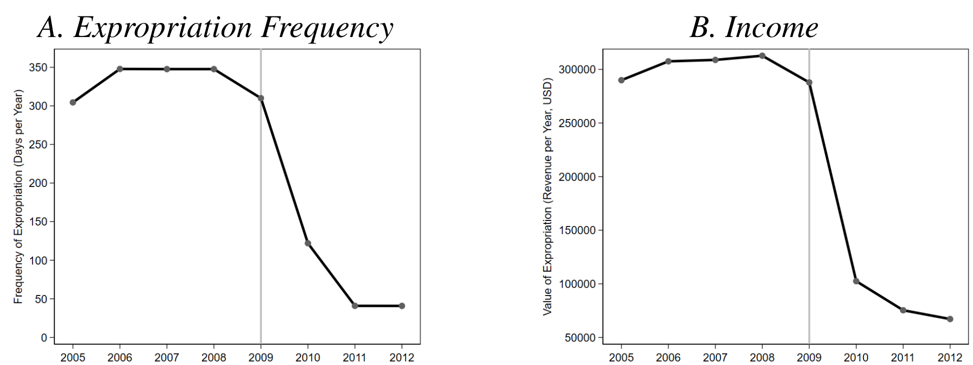
Notes: This figure shows the coefficients β_k, $k = -4, \ldots, 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2023) estimator. The dependent variable is an indicator for: whether the FDLR attacks village i in year t with the intention to pillage (Panel A), whether the FDLR attacks village i in year t with the intention to conquest (Panel B), whether the FDLR attacks village i in year t with the intention to punish (Panel C). The reference group for estimation is all pre-treatment (or never-treated) observations. The reference group for the pretrend test is all periods more than k periods prior to the event date (and all never-treated observations). 3,474 village-year observations were used in the estimation. There are 193 and 360 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.

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



Notes: This figure shows estimates from Borusyak et al. (2023)'s efficient and robust estimator for the effect of a pillage on the household's assets.

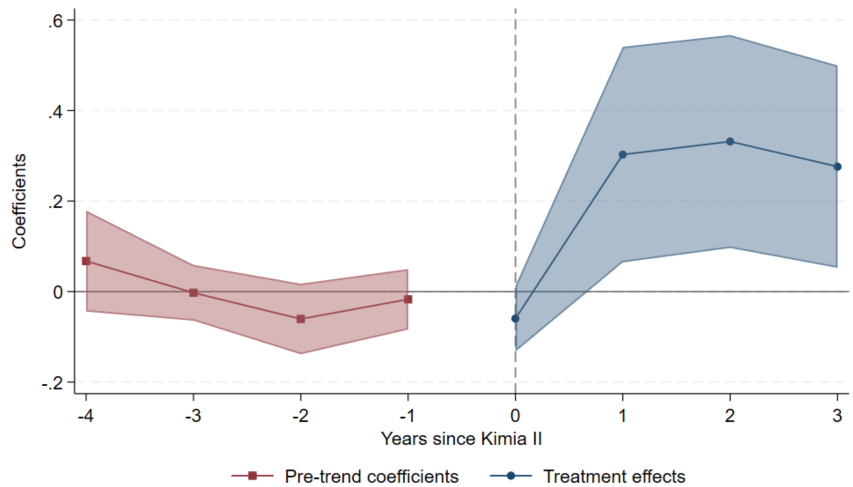
Figure B.14.: Mechanism: Expropriation, Value of Future Expropriations—Validation



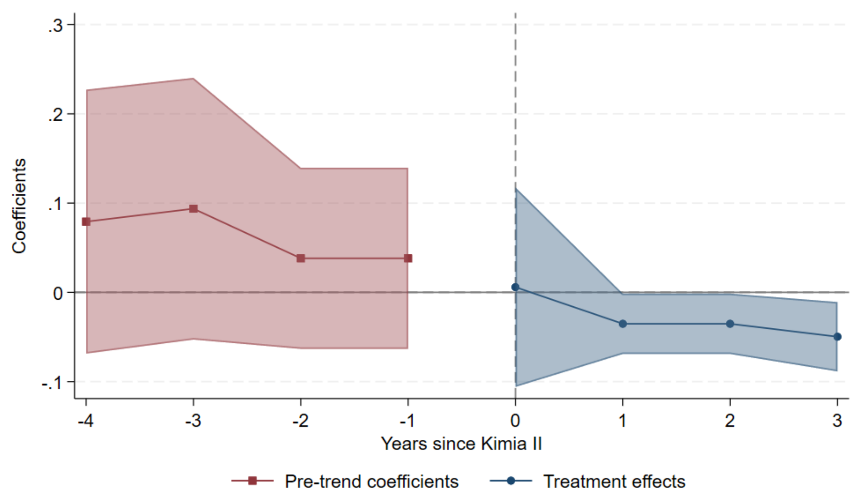
Notes: Panel A shows the trend of frequency of FDLR expropriation in the FDLR state. The frequency of expropriation is the sum of pillaging and tax payment events per year. These include toll fees (daily), poll taxes (weekly and monthly), mill taxes (daily), market taxes (weekly), and the number of pillages per year. Panel B shows the total income in USD generated by the FDLR in the FDLR state, through taxation and through pillage confounded.

Figure B.15.: Unpacking The Expropriation Channel

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



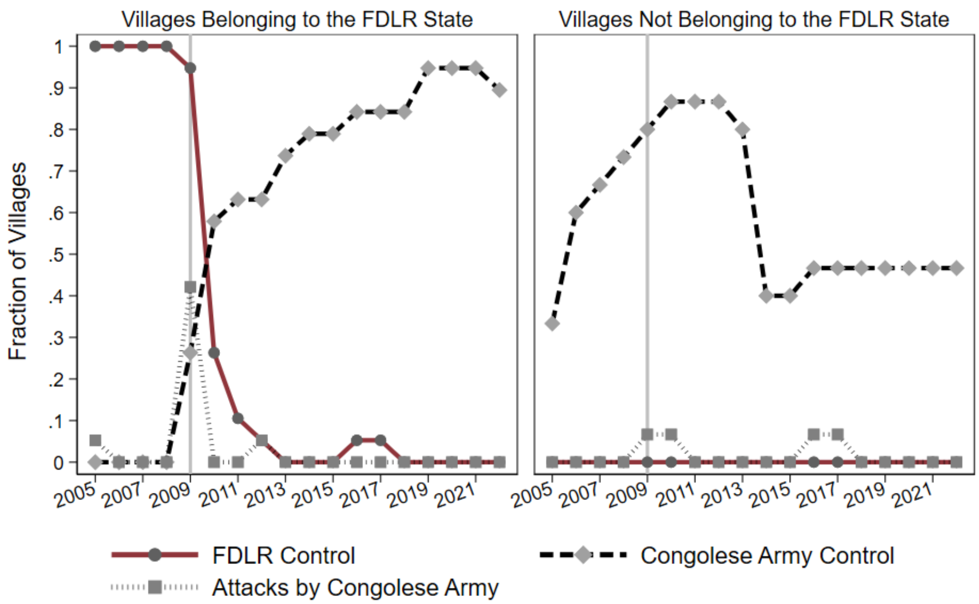
B. Dependent Variable: FDLR Violent Attack And Expropriation Frequency is High



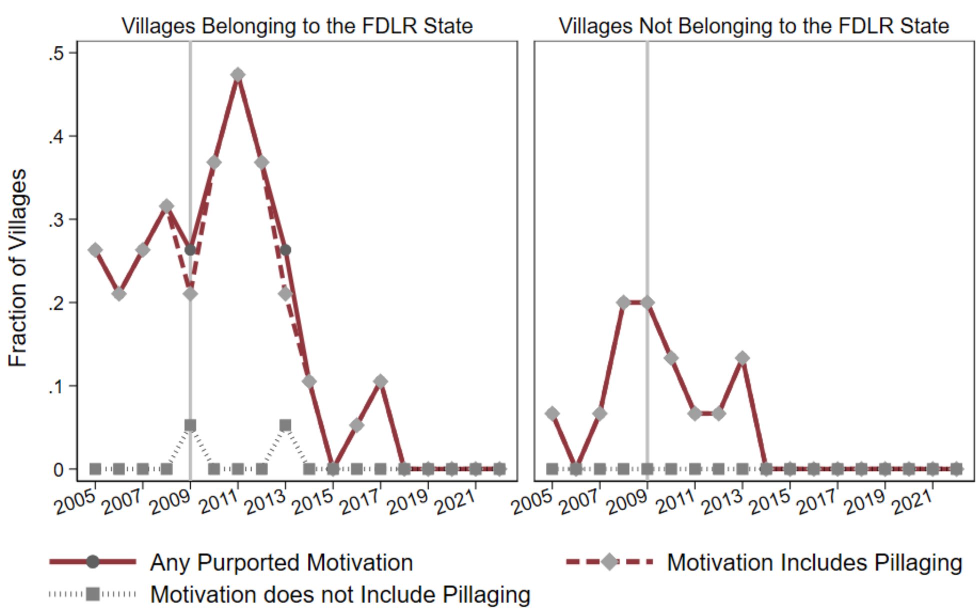
Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2023)'s estimator. In the top panel, the dependent variable is an indicator for whether the FDLR attacks village i in year t and the FDLR 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 reference group for estimation is all pre-treatment (or never-treated) observations. The reference group for the pretrend test is all periods more than k periods prior to the event date (and all never-treated observations). 3,474 village-year observations were used in the estimation. High frequency is more than 1 event per year (the 90$^{\text{th}}$ percentile). Results are insensitive to the cutoff.

Figure B.16.: Time-series for data collected in 2022

A. 2022 data, timeline of control

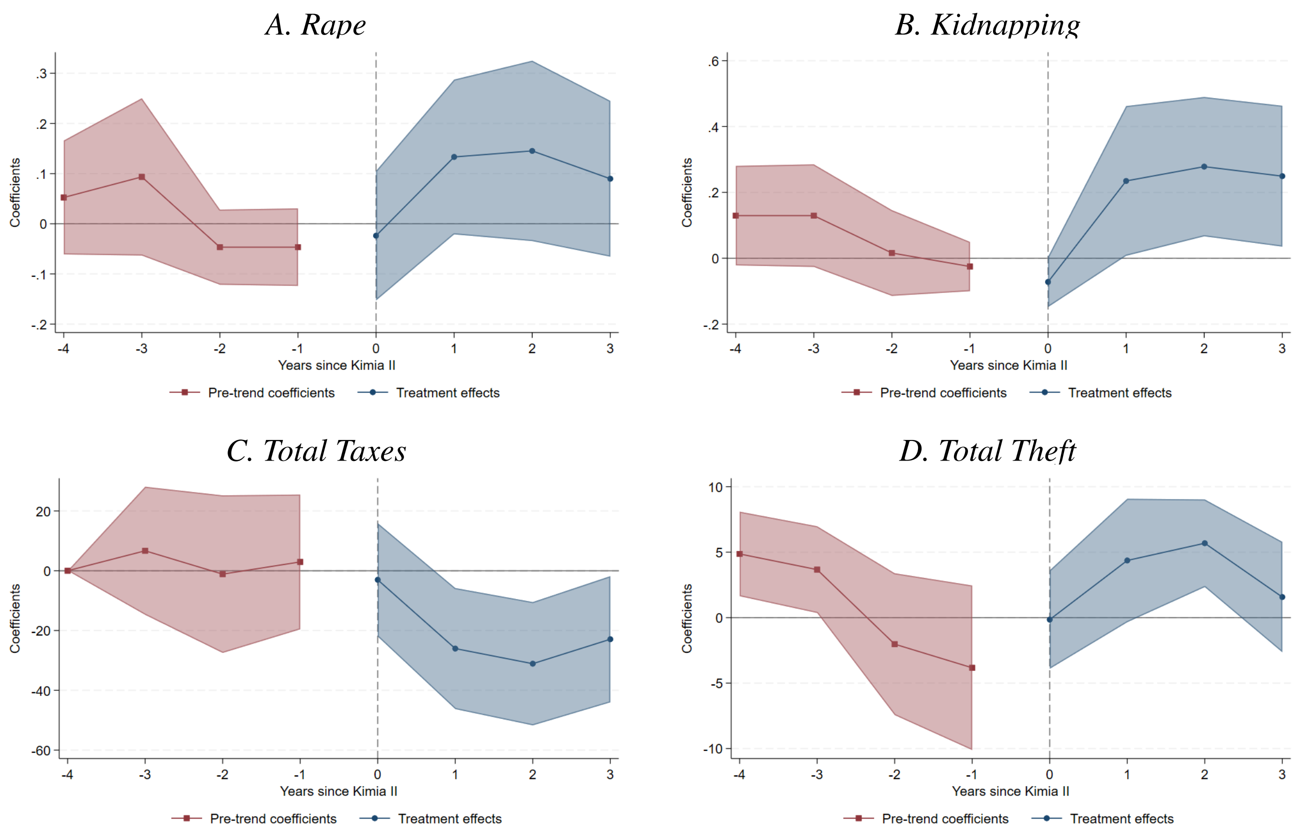


B. 2022 data, timeline of attacks



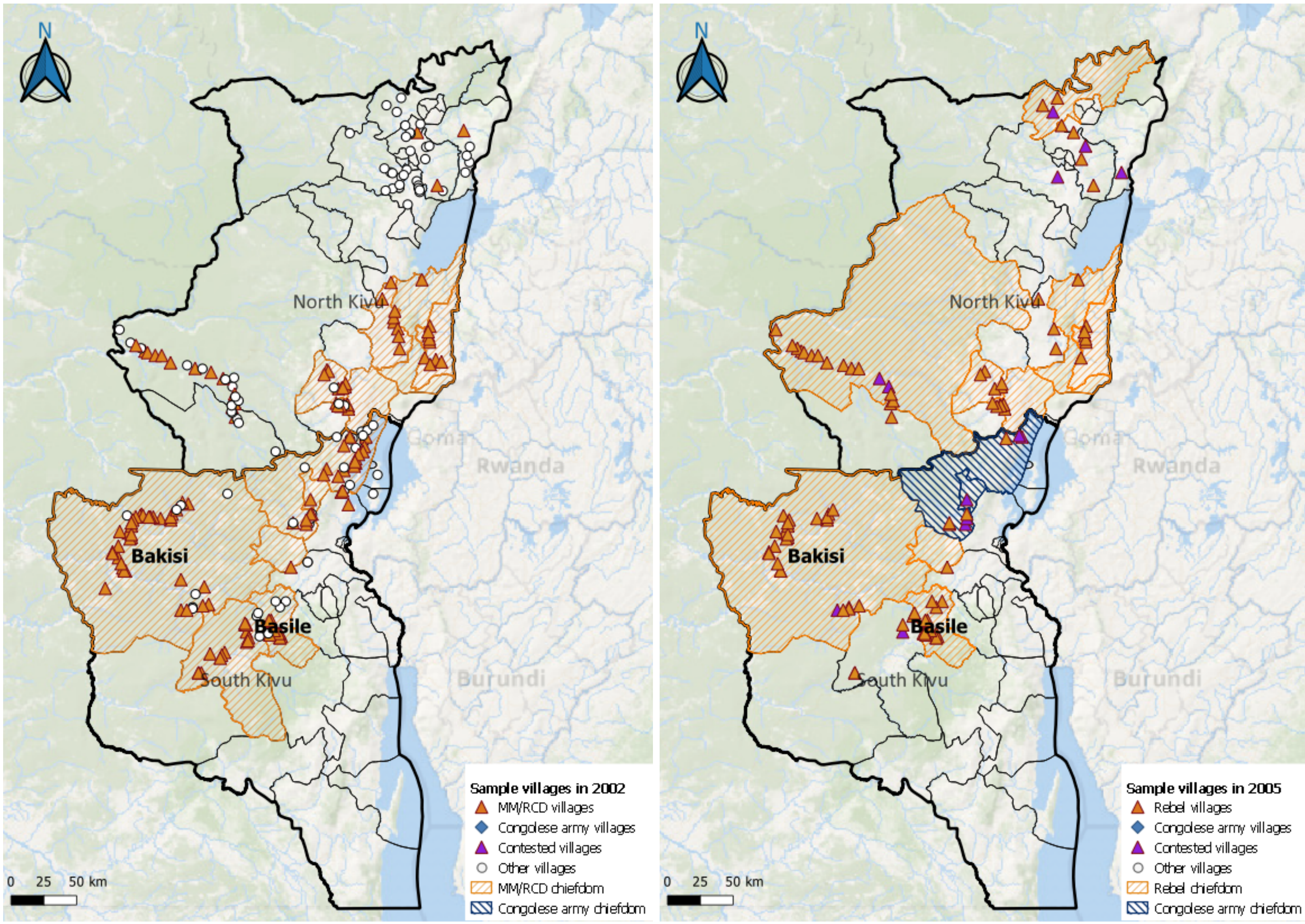
Notes: This figure is a replication of Figure III and Panel A of Figure IV. Panels A and B only use the data collected in 2022 for the villages that were also included in the 2013–2015 data collection.

Figure B.17.: Event Study Estimates, Welfare



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. (2023) estimator. It takes key variables from Table B.9 as dependent variable: rape (Panel A), kidnapping (Panel B), total taxes (Panel C), and total theft (Panel D). The reference group for estimation is all pre-treatment (or never-treated) observations. The reference group for the pretrend test is all periods more than k periods prior to the event date (and all never-treated observations). 3,474 village-year observations were used in the estimation of Panels A and B, 3,321 for Panel C, and 3,132 for Panel D. There are 193 and 168 village and Chiefdom-Year Clusters used for estimating standard errors, respectively.

Figure B.18.: Rebel and State Control, Before and After the Sun City Peace Agreement



Notes: This figure shows villages controlled by the Mayi-Mayi or the RCD in the sample, which are the part-takers of the Sun City Peace agreement of 2004, covering the provinces of North Kivu and South Kivu. The left panel does so for 2002, which is the year before the Sun City Peace Agreement negotiations began. The right panel does so for 2005, which is the year after the signing of the Sun City Peace Agreement. The orange triangles are the villages where Mayi-Mayi or the RCD had control, blue squares are those where the Congolese army had control. Orange striped areas indicate the Chiefdoms where the union of Mayi-Mayi and RCD hold more than 50% of villages in the sample villages in the Chiefdom. Blue striped areas indicate Chiefdoms in which more than 50% of the villages in the sample for that Chiefdom were held by the Congolese army.

C. MATHEMATICAL APPENDIX

C.1. *Bounds for Household Mis-reporting*

Let e be the treatment effect of being a village controlled by the FDLR in 2008 on the probability of being attacked by the FDLR after Kimia II. e is the estimand of interest. Let $A_t = 1$ if a village was attacked in period t, and $F = 1$ if the village was controlled by the FDLR in 2008. Consider only two periods, pre and post, such that $t = 1, 2$. The difference in difference estimator is:

$$\hat{e} = \big(E[A_2|F{=}1] - E[A_1|F{=}1]\big) - \big(E[A_2|F{=}0] - E[A_1|F{=}0]\big)$$

Let $\tilde{A}_t$ be an indicator for whether village was *reported* to be attacked in period t. Let $\tilde{A}_t{\geq}A_t$ if $F{=}1$ and $t{=}2$, and $\tilde{A}_t{=}A_t$ otherwise. Suppose instead that we estimate e using $\tilde{A}_t$ in differences-in-differences, yielding $\tilde{e}$. Let s be the proportion of FDLR state villages that overreport being attacked in the post period.

$$s = \mathrm{P}(\tilde{A}_2{=}1|F{=}1) - \mathrm{P}(A_2{=}1|F{=}1) = E[\tilde{A}_2|F{=}1] - E[A_2|F{=}1]$$

The difference in difference estimator of $\tilde{e}$ is then

$$\begin{aligned}
\tilde{e} &= \big(E[\tilde{A}_2|F{=}1] - E[\tilde{A}_1|F{=}1]\big) - \big(E[\tilde{A}_2|F{=}0] - E[\tilde{A}_1|F{=}0]\big) \\
&= \underbrace{\big(E[A_2|F{=}1] - E[A_1|F{=}1]\big) - \big(E[A_2|F{=}0] - E[A_1|F{=}0]\big)}_{\hat{e}} + \underbrace{\big(E[\tilde{A}_2|F{=}1] - E[A_2|F{=}1]\big)}_{s} \\
&= \hat{e} + s
\end{aligned}$$

Hence, $\tilde{e} = e + s$. In order for the true effect to be zero, $e = 0$, it would have to be that $s = \tilde{e}$. In our context, $\tilde{e} = .24$, thus our estimated coefficient accommodates a causal interpretation even with up to 24% of FDLR villages *always* reporting to have been attacked by FDLR after Kimia II while in fact they have not been attacked (ie, attacks are made-up), *and* this over-repoting being *only* in FDLR state villages *and* afte Kimia II.

C.2. *Simple Model of Expected Frequency of Expropriation*

Time, indexed by t, is discrete and runs forever. 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 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 reproduction, τ_t is the bandit's expropriation in period t, $\theta(s_t)$ is state functions, with $\theta'(s_t) > 0$, $\theta''(s_t) < 0$.

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

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

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

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

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

Proof: envelope theorem and F.O.C. 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 $p = 0$, $\tau_t^* = a_t$.

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.