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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 the trade-offs involved in state-building. Exploiting the timing and targeting of a weak state's operation designed to assert the state's territory over rebels who taxed the citizens in the "hinterland," we show that when a state asserts its exclusive right to collect taxes without simultaneously securing a monopoly on the use of force, it can inadvertently incentivize rebels to plunder the very citizens they once taxed. Through empirical evidence and testimonies from the villagers and the rebels themselves, we demonstrate that this plunder is not driven by defensive actions or retaliation, but is rather theft operations utilizing informants and designed to extract wealth, targeting rich households in villages where the rebels can no longer tax. To contrast this outcome with the common alternative of negotiating with the rebels, we analyze the targeting and timing of a peace treaty with rebels which integrated them in the state in exchange for retaking their territory. While the state regains its exclusive right to tax over the territory after the treaty, there is no subsequent increase in plundering. However, bargaining is marred by limited commitment, leading to a weakening of the state's legitimacy and to new rebel groups. Our findings underscore that a weak state pursuing taxation today could trade-off safety in the medium-run, and the state's integrity and legitimacy in the long-run.

"*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.

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1. INTRODUCTION

Throughout history, numerous states have struggled to exert *de facto* control over significant portions of the territory over which they have *de jure* sovereignty. These areas, instead, frequently fall under the authority of non-state armed actors (henceforth, *rebels*), who often assume control over these territories and provide stability. How can a state tax those areas?

The state can bargain over the spoils of holding the monopoly of violence with the rebels (Newbury, 2000)—a strategy plagued with commitment problems (e.g., the sack of Rome in 410 (Norwich, 1988)). Alternatively, the state could push back the rebels, as in modern European state formation (Tilly, 1985). However, the rebels' ability to tax the areas they control is sometimes their very incentive to refrain from arbitrary violence and to secure property rights (Olson, 1993). Thus, if the rebels are able to hide and endure the state's military operations, this strategy could create incentives for arbitrary violence. This paper aims to empirically analyze the effect of building a state by ousting the rebels, accounting for the source of the rebels' stability, and contrast it with the alternative of bargaining.

We first situate this trade-off within post-colonial Sub-Saharan Africa. In this region, rebels often exercise control over remote and inaccessible areas, commonly referred to as the "hinterland," where they can hide during state offensives. We present evidence indicating that when the state undertakes military efforts to displace the rebels, it tends to be followed by rebel violence towards civilians, but not when such effort is through treaties.

Our analysis then narrows in on Democratic Republic of the Congo (DRC). 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) in the Basile chiefdom. We compare this approach with the major peace treaty called the 2004 Sun City agreement.

To analyze the effect of Kimia II, we take advantage of three features of the setting. First, the army seamlessly ousted the rebels, making it impossible for the rebels to tax. Second, Basile was surrounded by a hilly forest, offering a hinterland for rebels to endure—a common feature of military interventions in weak states—and the state was too weak to provide security in the newly acquired villages (Stearns et al., 2013). Third, Kimia II was a national concession to Rwanda, making reverse causality unlikely. The setting is thus well-suited to isolate Olson (1993)'s notion of stationary bandits, which conjectures that the ability to regularly tax incentivizes armed actors to refrain from arbitrary expropriation.

We analyze the *effect* of Kimia II in three steps. In a first step, we document that, by 2009, the rebels targeted by Kimia II in Basile Chiefdom controlled many villages (henceforth, *targeted villages*), remote even by eastern DRC standards, where they provided stability: they taxed and provided protection. Despite the FDLR's tendency for violence in the region, these villages had more safety than the rest of villages in the sample. We also document that Kimia II made it impossible for them to tax these villages. In a second step, using an event study comparing these villages to those not targeted by Kimia II, we establish that completion of Kimia II is associated with a 343% larger incidence of attacks by the targeted factions against the targeted villages for *at least* three years after Kimia II. In a third step, we examine spatial spillovers, differential time-trends, aggregate coincidental shocks, intra-Chiefdom correlation of violence, divergence on observable characteristics, migration, classical miss-classification, spatial correlation, specification. We find no evidence of confounds. This is consistent with Kimia II *causing* a rise in *reported* FDLR attacks.

Having provided evidence supporting that Kimia II caused an increase in reported FDLR attacks in targeted villages, we then *characterize* this effect in three steps. In a first step, we examine whether this effect is driven by an increase in reporting or, instead, by an increase in actual attacks. First, we show that our estimates replicate using public data (ACLED, 2022), which is not affected by the same type of reporting bias. Second, we analytically derive sensitivity bounds. We find that, even if 25% of FDLR attacks reported in the FDLR state after Kimia II were *entirely false* and that this bias *only* affected the FDLR state villages and *only* after Kimia II, our estimate would still be causal. Third, we confirm this bound through simulations. Fourth, we conduct qualitative interviews with 22 FDLR perpetrators who were dislodged by Kimia II. The perpetrators report the same effect of Kimia II. In a second step, we conduct a falsification exercise to assess whether the effect of Kimia II on violence is specific to the targeted factions. We find no effect on violence by other rebels in the targeted villages, nor on villages other rebels controlled, thus Kimia II only caused FDLR attacks in FDLR state. In a third step, we determine the effect's duration. After re-surveying a subset of our sample in 2022, we show that the effect on FDLR violent theft operations lasted *four to five* years. This window coincides with the state taking all the villages. 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 far away in search of income.

Our analysis then turns to examining the channels of this effect. Consistent with the villagers' qualitative interviews, we document that this effect is driven by attacks reported to be motivated by theft (henceforth, *pillage*), not retaliation nor conquest. We then address that this purported *motive* is reported by the villagers. First, we document that the rise in attacks is driven by attacks targeting villages in which the Congolese army was *absent*, inconsistent with retaliation or war. Second, using household data, we document that Kimia II had no differential effect on attacks on households of the village elite, former militia or army members or potential targets of retaliation; in contrast, the effect is driven by attacks on the wealthiest households, consistent with theft—even controlling for other household characteristics. Third, qualitative interviews with the perpetrators confirm that these attacks were motivated by theft. We then take a step back and ask *why* Kimia II *induced* theft operations. Our qualitative interviews suggest that, prior to Kimia II, the FDLR factions had no incentive to pillage because they expected to expropriate over a long horizon. We show that choosing to tax instead of pillaging has a break-even point of 3.7 months. Guided by a simple model, analyzing the targeting and timing of the attacks, we provide suggestive evidence that, by making it impossible to tax, Kimia II purportedly reduced the time horizon below this break-even point, for the villages the rebels could no longer tax.

In terms of welfare, Kimia II *permanently* reduced the village yearly tax burden of targeted villages by 41% (by $7,282$ USD, from $17,902$ USD) and increased the state's informal yearly revenue by $10,620$ USD. These economic gains are tainted by violations of dignity: it also increased kidnappings, plundering, and rape by 24 pp., 10 pp., and 18 pp. *per year* respectively as a by-product of pillages, for the duration of the response.

To analyze the effect of the peace treaty, we take advantage of the fact that, while it included most active rebel groups, 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 sub-set of the villages (henceforth, *affected villages*). Consistent with historical evidence (Stearns et al., 2013), we show in an event study 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—the strategy is plagued by limited commitment.

Lastly, we explore the implications of our findings regarding the benefits and costs of each approach and supplementary measures that could complement either approach, the comparative statics of each approach for social welfare, and and the incentives faced by a rational government, highlighting potential deviations from the socially optimal path.

A priori, it would seem hardly surprising that, when rebels are pushed out, they fight back (e.g., Dell et al., 2018; Lessing, 2017). In war, it is common for the losing side to target the location where the enemy seeks refuge in order to weaken them. An illustrative quote by journalist Peter Arnett, attributed to a US major during the Vietnam War, supposedly highlights this approach: "*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 regular expropriation of citizens dis-incentivizes the rebels to arbitrary steal from them. This relationship is often not "legible" to the state (Scott, 1998), who, in our context, did not anticipate disrupting 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.

Extensive research in economics has investigated the factors that give rise to state functions (e.g., Allen et al., 2020; Mayshar et al., 2021). Building upon 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). Second, we introduce the notion that the emergence of the state can have adverse effects on the behavior of bandits. Specifically, in our context, when the state establishes territorial control, bandits become nomadic, relocating to *hard-to-control* areas where they sustain themselves through plundering (Scott, 2009). The introduction of hierarchy disrupts their encompassing interest, leading to excessive expropriation in line with à-la Shleifer and Vishny (1993), which reduces the returns to investment. These dynamics may intensify the challenges associated with escaping the "civilizational paradox" (Dal Bó et al., 2022), and complement Mayshar et al. (2021), who highlight the incentivizing 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 abundant 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 Carneiro (1970) argument regarding state formation. Furthermore, the challenging accessibility of the area, combined with its resource endowments, resembles the conditions postulated by Dal Bó et al. (2022) that facilitated the rise of Ancient Egypt on a larger scale.

Our study relies on Olson (1993)'s notion of "stationary" and "roving" bandits to rationalize the response of the rebels. Olson (1993)'s argument is that a longer time horizon in which a bandit can expropriate from a location may induce the bandit to develop an encompassing interest in that location, lowering his/her expropriation rate. Sánchez de la Sierra (2020) shows 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 for the location, tending to *decrease* the time horizon; that is, it is not a test of Olson (1993)'s *raison d'être* of stationary bandits. Our study isolates Olson (1993)'s mechanism by leveraging features of our context which are common to weak states: the state has military superiority, hence can shrink the bandit's horizon of taxation; there is a hinterland, which allows stationary bandits and their violence to endure, as roving bandits.

These findings also contribute to the state-building literature and have implications for state-building policy. First, they show that bargaining can be successful in the short-run to regain territory. Yet, we also provide novel suggestive evidence of the problems of commitment inherent in peace agreements, pervasive in weak states: if the state cannot commit to future negotiations, and if rebels cannot commit not to use the state for their interests, bargaining with the rebels can disintegrate the state in the long-run. Second, our findings challenge applications of Weber (1946)'s conception of the state as a territorial monopoly of (legitimate) violence. Judged from the territorial lens, Kimia II was a success: the territory was reclaimed, and the state now taxed. 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 Herbst (2011)'s argument about the pitfalls of this notion in post-colonial Africa, this finding underscores a limitation of the notion of territorial sovereignty acquired by violence—popularized in Tilly (1985) and predominating current legal doctrine towards state-building in post-colonial Africa.

2. TERRITORIAL CONTROL IN POST-COLONIAL AFRICAN STATES

By the second half of the 20th century, many post-colonial African states were on the brink of collapse.[1] 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).[2]

In response, governments developed two strategies to assert their states' territorial authority. On the one hand, when they had the means, they have 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 Colom-

[1] See, for example, Zartman (1995). Current African states artificially emerged from the 1885 Berlin's conference. After independence, such states therefore did not rule over consolidated nations, and faced the problem of expanding and consolidating power over their territories with the limited administration they had inherited from the Europeans (Herbst, 2011, Ch. 4). During the Cold War, superpowers infused African states with economic and military support when their rule was challenged. For example, Zaire won notable aid from the U.S. against the Shaba rebellions; Chad relied on France against Libyan aggression, and Ethiopia was given critical military support by the Soviet Union to resist Somalia. However, in the 1970s, the weak internal control of the states began to become apparent. Many states began to disintegrate, with dwindling resources, and a resulting decline in the capacity of states to control their de jure territory (Bierschenk, 2010; Bates, 2008; Van de Walle, 2001; Blundo, 2006). The Soviet Union collapse made cheap weapons available to rebels.

[2] In Niger, for example, large parts of the territory were controlled by various armed groups affiliated to Touaregs until a peace agreement in 1995; today, large swaths of the territory are contested by armed groups including by Boko Haram. In Sierra Leone, where the state collapsed into a long civil war in 1991 (Sierra Leone Civil War 1991–2002), the state lost control of its territory to various armed groups including the RUF, the AFRC, and the NPFL for more than a decade. In Mozambique, after an anti-communist group signed a peace agreement with the government in 1992, the state regained its territory, only to lose it again to the al-Shabab militia in 2017. In Rwanda, following the collapse into massive violence during the Rwandan genocide in 1994, a rebel group, the Rwandan Patriotic Front, took territory and ultimately the country, ending the genocide. In Zaire (today DRC), after a first internal rebellion called Magrivi, the AFDL rebellion took the whole country in less than a year, only to lose half of the country to various factions of the RCD and Mai-Mai groups until the Sun City peace agreement of 2004. In Nigeria, Boko Haram, which emerged in northeast Nigeria in 2002, became a violent insurgency in 2009 controlling large parts of the territory. In Cameroon, for example, Ambazonian forces consisting of eleven armed groups took control of a significant share of the territory in 2017. In Benin, since 2020, the state has lost its territory to jihadist groups in large areas of its territory on its border with Burkina Faso. In Burkina Faso, Islamist rebels began to encroach the state's territory in 2015, and Burkina Faso only controls 60% of its de jure territory.

bia. 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.

Details of the end of these wars allows to provide some numbers of these strategies.[3] Figure I uses data we consolidated covering all civil wars in Africa since 1961 to document the prevalence of either strategy.[4] Most countries experienced a civil war, but the strategy in 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 ended in peace agreements, such as Mozambique, Sudan, Sierra Leone, and the Second Liberia 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; of the Ugandan Civil War (1980–1986), which ended when the rebels took the capital, establishing a government, yet various rebel factions remained active, and continued to exert violence for years. Second, 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; of the Second Sudanese Civil War of 1983–2005, which ended with the Comprehensive Peace Agreement of Nairobi in 2005; of 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); of 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.[5]

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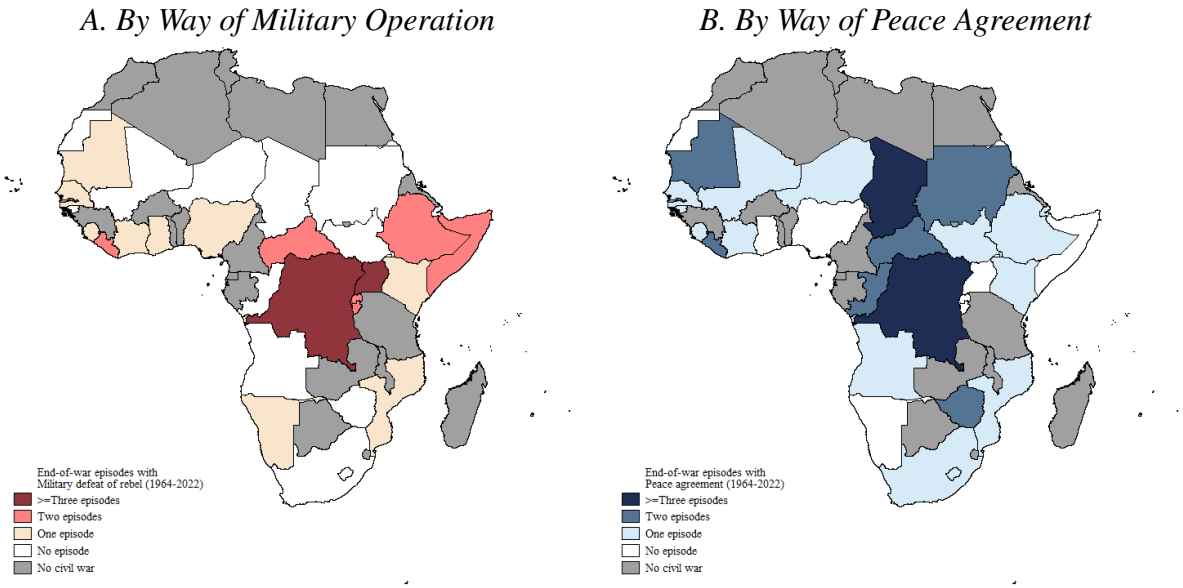
[3] Since independence, civil wars ended with the state regaining its territory, except in rare cases (e.g., Sudan).

[4] We list all these wars in Section 1 of Authors' Personal Supplemental Appendix I.

[5] These correlations are in Figure B.1.

Figure I.: Post-colonial African States' Endings of Civil Wars



Notes: This figure presents the incidence of post-independence 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 post-colonial 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.[6] While most African countries are affected by these events, the DRC stands out.[7] 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).[8] We document a moderate increase

[6]The data were introduced in Bell and Badanjak (2019). At the start of 2023, PA-X (2022) covers the world from 1990 to April 2022, amounting to over 1,959 agreements in 140 peace processes.

[7]Figure B.2 shows the incidence of intra-state peace agreements and events of government regaining territory.

[8]The analysis is presented as country-year event studies in Figure B.3.

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 in the army; b) military operations (Stearns et al., 2013).

The largest of such peace treaties is 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. The most militarily successful example 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"). 22,000 and 8,000 Congolese and UN soldiers, respectively, faced at most 4,000 FDLR soldiers (Florquin and Debelle, 2015).[9] Three features of Kimia II make it suitable to isolate the effect of the ability to tax on the incentives for violence:

[9]Militarily, Kimia II 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.

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 targeted villages of Basile without a fight. Kimia II ended in December 2009, having ousted the FDLR, making it impossible for them to tax the targeted villages.[10]

b) No consolidation of the state's monopoly of violence. Basile is surrounded by the hilly forest of Itombwe, where the factions re-settled, maintaining semi-autonomous command structures (Florquin and Debelle, 2015) in nearby camps.[11] 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.[12]

> *"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."*
> **Source**: 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.[13] In what follows, we take advantage of Kimia II's timing and spatial targeting to estimate its causal effect on the behavior of the targeted FDLR factions.[14]

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[10] See Vlassenroot and Verweijen (2017, p.113): *"The Kimia II operations were indicative of a shift in government policy toward armed groups, which now became more strongly focused on military operations."*

[11] Maps in Vogel (2021) show that the FDLR no longer exerted territorial control thereafter.

[12] See Sawyer and Van Woudenberg (2009) and Levine (2014, Ch. 8). Perpetrators were the same individuals: *"A number of the victims of abuses had clearly been able to identify their attackers as FDLR since they knew them by name and had lived side-by-side with them for many years."* **Source**: ICC (2012), p. 80.

[13] UNSC (2009) documented that 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). This response, which was nationwide and short-lived, affected villages in North Kivu, far from Basile.

[14] 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.[15]

The sample comprises interviews conducted in 1,537 randomly selected households, in 239 villages of South and North Kivu.[16] 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.[17]

We refer to the years 2005-2012 as the *Quasi-Experimental Window*, which zooms into 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*.[18] We refer to the rest of villages as the *Quasi-Experimental Villages*.[19] We refer to the quasi-experimental villages in the quasi-experimental window years as the *Quasi-Experimental Sample*.

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[15]The historical village data for this paper was introduced and described in Sánchez de la Sierra (2014, 2020).

[16]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.

[17]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.

[18]Kimia II was followed with another military policy, the *Regimentation*, which, in 2011, called Congolese army troops from Shabunda to the urban barracks for streamlining the command structure 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).

[19]Table B.1 compares the confounding villages to the quasi-experimental villages.

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:"[20]

> *"[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."* **Source**: qualitative report for the village of Pohe, Mito and Wimbi, 2013.

Indeed, the FDLR expropriated through poll taxes, toll fees, taxes on the market in 94%, 83%, 28% of their villages, respectively.[21] Each household paid, on average, 64.97 USD yearly to the FDLR in taxes.[22] 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.

Second, our data suggest that the villages' remoteness enabled the FDLR state:

> *"These FDLR came to settle in the village because it is a village very remote in the forest and far from the city [. . .] they became absolute masters in this village."* ***Source****: qualitative report of the village of Musingi*

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.[23]

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 Congolese army military superiority over territory was undisputed. Figure II, right panel, shows that the army ousted FDLR from most villages by 2011. The FDLR factions re-settled in nearby bases in the forest of Itombwe.[24] Figure III shows that, by 2011, the FDLR had lost 90% of the Basile villages they controlled.

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[20] Section A.1 presents qualitative evidence on the behavior of the FDLR factions prior to Kimia II in Basile.

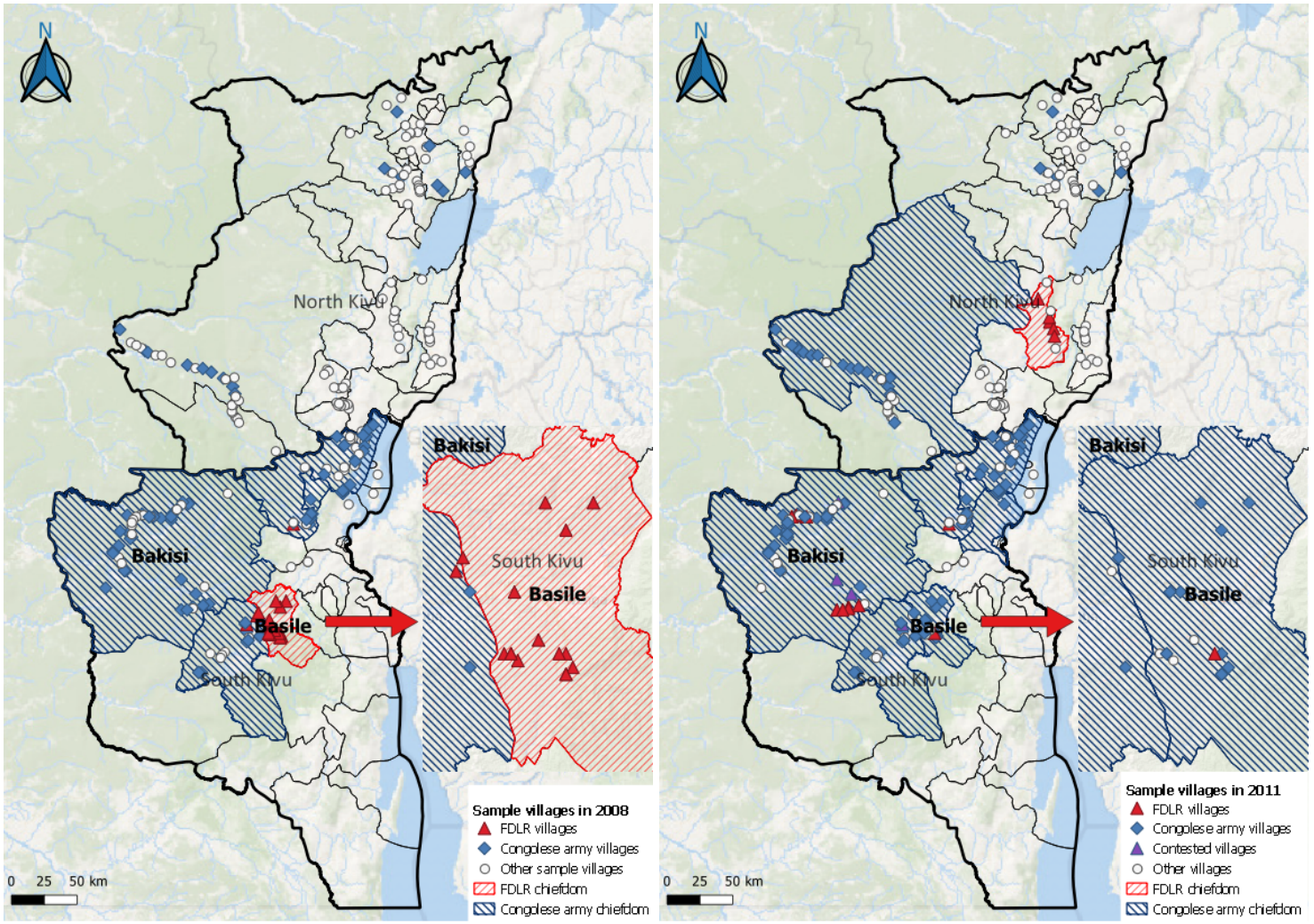
[21] The quantitative description draws from the statistics presented in Table B.1.

[22] This amounts to 25.5% of 2009 national p.c. income in USD of 2022 (**Source**: World Bank (2021)). Using alternative estimates of income in these areas, Section 5.4 suggests that a more realistic share is [7%-11%].

[23] We account for those differences in robustness analysis, Table B.2, Column (5).

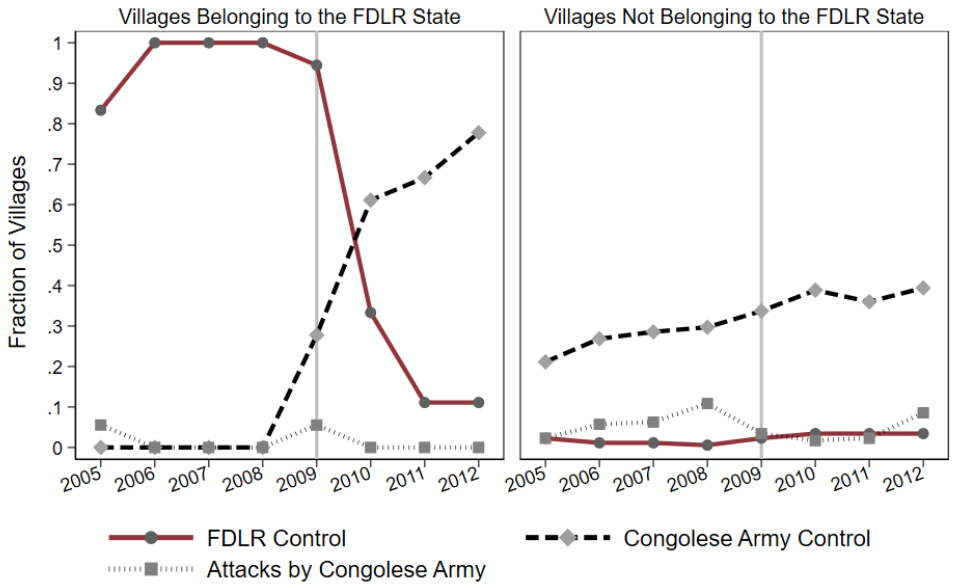
[24] Figure B.4 zooms into Basile to show the locations of the FDLR camps after Kimia II in the forest. Panel A uses the sample villages, Panel B uses the universe of villages. The figure also shows that the FDLR controlled

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



Notes: This figure shows villages controlled by the FDLR in the sample, covering the provinces of North Kivu and South Kivu. The left 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.

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. Even in those they 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."* **Source**: Qualitative reports of Tubindi and of Mito (2013).[25]

Baseline Estimates

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.

---

many remote villages before Kimia II and pillaged the villages it did not control, many of which were along the road. This is consistent with the qualitative interviews shown in Sections A.1 and A.2, which show that, while the FDLR protected the villages it taxed before Kimia II, it pillaged villages it did not control. This supports that the FDLR had developed an encompassing interest in the villages it could tax in Basile.

[25] Section A.3 presents additional qualitative evidence about the inability of the army to consolidate power.

To analyze the effect of Kimia II, we estimate the following equation using Borusyak et al. (2020) robust and efficient estimator.[26] 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.[27] 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.[28] 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 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

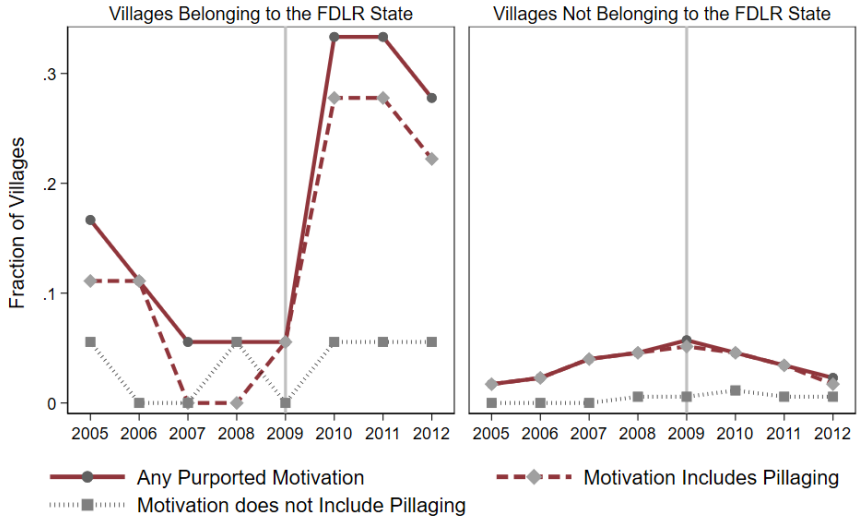
[26] With the exception of Borusyak et al. (2020), the estimators developed in the recent papers of staggered treatment adoption, reviewed in Roth et al. (2023), are not designed to improve over OLS when with simultaneous treatment. In contrast, Borusyak et al. (2020) show that their estimator is efficient 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. (2020).

[27] 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. (2020) estimator perform well in finite-samples.

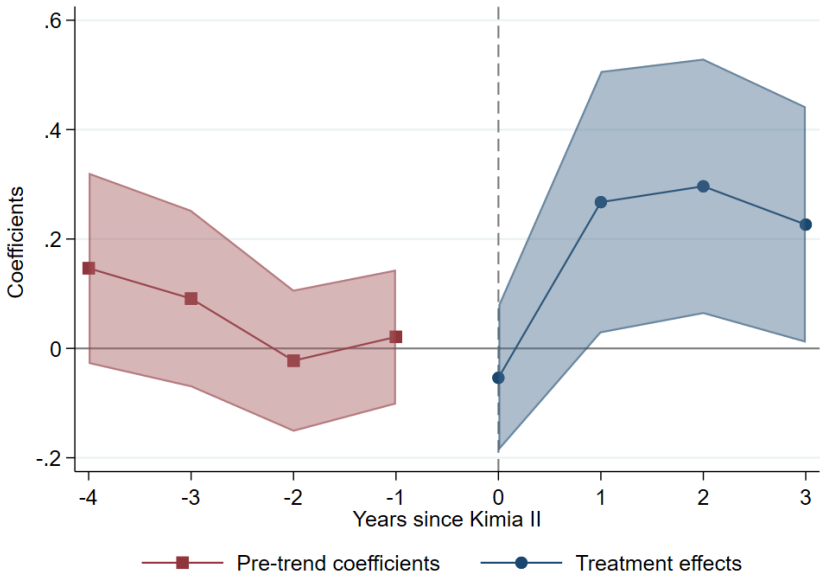
[28] The coefficients include a decrease in non-FDLR villages, hence they converge weakly after 2011.

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. (2020) 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 clustered two-ways at the village level and the chiefdom-year level. The figure presents the treatment effect coefficients as derived by Borusyak et al. (2020) robust and efficient estimator and its associated command. The Borusyak et al. (2020) 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).

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, presented 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;[29] 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).[30] 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.[31]

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

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

[31]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 of the sample 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 lagged dependent variable. Figure B.6 conducts two falsification exercises for the specification choice. Panel A estimates Equation 2 using each year of 2005–2011 as cutoff. Panel B separately estimates Equation 2 using each Chiefdom in sample as an indicator for FDLR state. We also simulated 10,000 random

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.[32] Overall, the checks performed in this section suggest that Kimia II caused a rise in reported FDLR attacks.

5.2. *Characterizing the Effect*

Having provided evidence in support that the baseline coefficient has a causal interpretation, 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 show 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 over-reporting *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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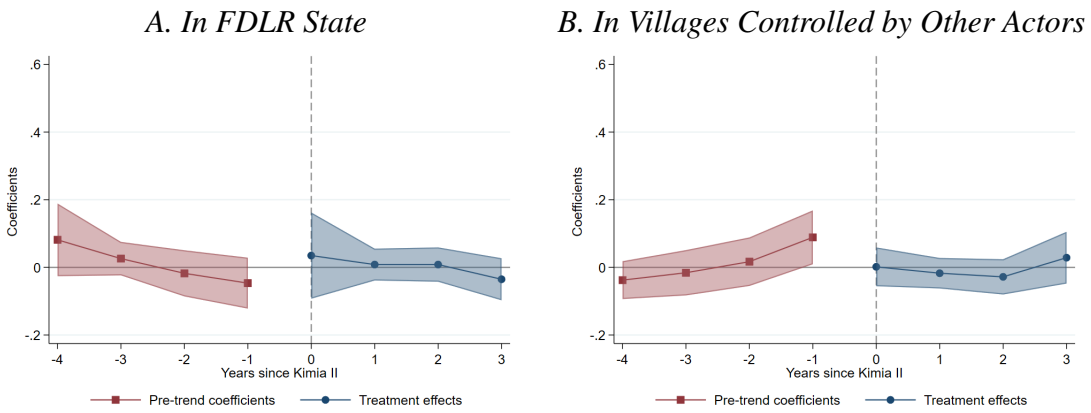
assignments of FDLR state to villages, holding the fraction of targeted villages constant. For each simulation, we estimated Equation 2. The simulations show that the p-value of the sharp null hypothesis is 0.00. Figure B.7 presents the distribution of the simulated coefficients. Figure B.8 includes the eight pre-Kimia II cofficients.

[32]We also simulated different levels of mis-classification of our binary outcome variable. 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 resulting coefficients and confidence intervals for 5, 10, 15, 20, 25, and 30 % of measurement error suggest there would need to be over 20% of measurement error in our binary outcome variable to make the effect of Kimia II statistically insignificant.

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

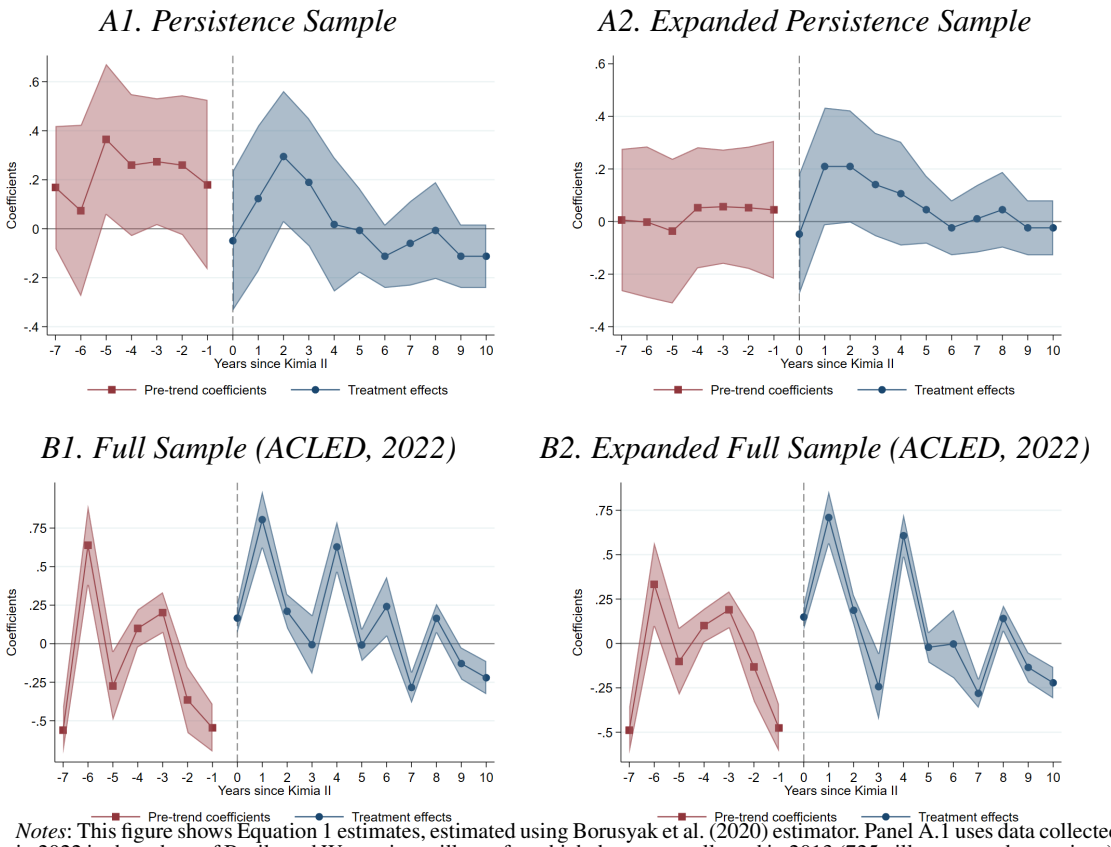


Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020)'s estimator. The reference group for estimation is all pre-treatment (or never-treated) observations. The reference group for the pretrend 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. 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 conduct two extensions to the baseline coefficient estimation. First, we conducted a second collection exercise in Fall 2022, allowing us to observe attacks until 2022 in a sub-set 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

Figure VI.: Duration Analysis



Notes: This figure shows Equation 1 estimates, estimated using Borusyak et al. (2020) 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 clustered two-ways 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 standard errors clustered two-way at the village and groupement-year, and 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 clustered two-ways 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.

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 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.[33] The baseline coefficient estimates using the expanded persistence sample are positive until (and including) year 5 after Kimia II. While statistical significance drops below 5% after year 3, the pattern of the coefficients is consistent with the effect persisting, while decaying, until year 4–5 after Kimia II.[34]

Second, Panels B1 and B2 use the calculated distance of ACLED (2022) events to the full original sample, and then adding the newly collected villages of 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?

It would be unreliable to provide a causal explanation for the why the effect ends, because the "quasi-experiment" is the completion of Kimia II, rather than the consolidation of state power after Kimia II, and hence our interpretation cannot be conclusive. 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.[35] 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

[33]We expanded the set of villages in Basile. This sample includes 21 additional villages, 10 in the FDLR state.

[34]This sample also allows to exploit variation of Kimia II within Basile. Including groupment-year clustering produces the same results (see Table B.7, Column (6)). To provide additional confidence in the validity of the new sample, Figure B.10, Panel A, presents the baseline coefficient estimates using the original data with the sample restricted to the 36 villages of Mwenga. Panel B does that using the data collected in 2022.

[35]E.g.: *"They went to other provinces of the DRC; it was a total disbanding."* **Source**: Qualitative interview, Nov. 2022, Bushaku 2.

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, clustering the standard errors two-way 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). This provides reassurance that the baseline coefficient is not driven by sample choice.

In sum, this section has shown that the effect of Kimia II, which we 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.[36] Table I presents the estimates from Equation 2. Column (1) presents the baseline estimates as benchmark. In Columns (2)–(4), the

[36]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.

dependent variable is an indicator taking value one if there is an attack motivated by territorial conquest, punishment, or pillaging respectively.[37]

Kimia II has no effect on attacks motivated by conquest, and attacks motivated by punishment could explain at most 16% of the increase in attacks (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. This suggests that Kimia II predominantly incentivized violent theft operations, plunder.[38]

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.

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. In Panel B, we use the household-year data to estimate:

$$\begin{aligned} Y_{ijt} = \beta^D \, FDLR_i \times 1(t > 2009) + \beta^D_{HH} \, FDLR_i \times 1(t > 2009) \times 1(target_{jt}) \\ + \gamma^D \, 1(t > 2004) \times FDLR_i + \alpha^D_{ij} + \beta^{D:NK}_t + \epsilon^D_{ijt} \end{aligned} \tag{3}$$

where j indexes household, $1(target_{jt})$ is an indicator taking value 1 if household j is a probable *target* of punishment at time t, and α^D_{ij} are household fixed effects. Column (1) shows that the baseline result holds in the household-level data. Across columns, β^D_{HH} is insignificant, β^D is positive and significant. Columns (8) and (9) show that this result

[37] 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 motive 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, which typically used to transport stolen goods.

[38] Figure B.12 presents the event study estimates for pillages, retaliation, conquest attempts, respectively.

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)
FDLR$_i$ $\times 1(t > 2009)$	0.240***	-0.001	0.043***	0.220***	-0.030	0.037***	0.210***
	(0.068)	(0.018)	(0.013)	(0.063)	(0.020)	(0.006)	(0.060)
Control FDLR Punishment	N	N	N	N	Y	N	Y
Control FDLR Pillage	N	N	N	N	Y	Y	N
Control FDLR Conquest	N	N	N	N	N	Y	Y
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, clustered two-way 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.

holds controlling for of $FDLR_i \times 1(t > 2009)$ interacted with whether the household head completed primary or secondary education and with a wealth index.

Third, we analyze whether the *within-village* targeting of the attacks is consistent with their motivation being *theft*.[39] 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 triggered attacks that disproportionately targeted the rich households. This effect holds even in Columns (8) and (9), in which we control for indicators of household education and for the political variables interacted with $FDLR_i \times 1(t > 2009)$, suggesting that other household characteristics do not explain the targeting of richer households.[40]

[39]The qualitative reports from Basile show that the FDLR regularly acquired information on the wealth distribution by nurturing spies in the villages, called the "*eclaireurs*," those who "provide light." See Section A.2.1.

[40]The household-level regression is consistent with the village-level estimate. The likelihood of an FDLR state village experiencing an FDLR attack after Kimia II is 31% (baseline 0.07 + effect 0.24). Column (1) shows that

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					0.07

Panel B. Retaliation	*Dependent Variable: FDLR Attack*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.016***	0.017**	0.013***	0.013**	0.013**	0.012	0.019***	0.024**	0.022*
	(0.004)	(0.008)	(0.005)	(0.005)	(0.006)	(0.008)	(0.004)	(0.011)	(0.011)
$\text{FDLR}_\text{i} \times 1(t > 2009) \times 1(target_{jt})$		-0.004	0.149	0.060	0.081	0.024	0.028*	0.035***	0.032**
		(0.042)	(0.105)	(0.051)	(0.074)	(0.045)	(0.015)	(0.012)	(0.013)
$1(target_{jt})$	-	Official	Fighter	Chief	Land	Cattle	Index	Index	Index
Controls	N	N	N	N	N	N	N	Educ	Educ+Wealth
Observations	13,614	6,820	12,834	13,454	12,228	6,757	12,013	10,754	10,754
R^2	0.10	0.09	0.09	0.10	0.09	0.09	0.09	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	0.00	0.00	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	0.01	0.45	0.41	0.41

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**	0.012	0.022*
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.011)	(0.011)
$\text{FDLR}_\text{i} \times 1(t > 2009) \times 1(target_{jt})$		0.037***	0.010*	0.015**	0.038***	0.023***	0.019***	0.019***	0.009***
		(0.007)	(0.006)	(0.006)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)
$1(target_{jt})$	-	Rich	Wives	Lands	Assets	Married	Index	Index	Index
Controls	N	N	N	N	N	N	N	Educ	Educ+Pol
Observations	13,614	13,442	13,382	13,345	6,462	12,243	11,855	11,855	10,754
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	0.00	0.14	0.01	0.03	0.00	0.00	0.00	0.00
Conley (1999) p-value Interaction		0.01	0.78	0.10	0.00	0.34	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)–(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 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, if the respondent purchases any land during 2006–08, if the respondent purchases any cattle during 2006–08, respectively. In Columns (7)–(8), $1(target_{jt})$ is a normalized index of all 5 characteristics constructed by principal component analysis. Columns (8) controls in addition the interaction of FDLR_i and whether the household head completed primary school and with whether they finished secondary school. 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. Columns (8) controls in addition the interaction of FDLR_i and whether the household head completed primary school and with whether they finished secondary school. Columns (9) controls in addition the interaction of FDLR_i and education levels and the interaction of FDLR_i and the index of political characteristics used in Panel B Column (7). *Controls:* indicates whether additional household characteristics are interacted with FDLR_i ("Educ" indicates that it is interacted with whether the household head completed primary school and with whether they finished secondary school. "Educ+Pol" indicates that in addition it is also interacted with the index of the households political role as used in Column 7 of Panel B.). 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 before and after Kimia II. Section A.4 presents excerpts from such interviews: 98% of the attacks they reported in the FDLR state villages of our sample were *for* the extraction of resources.[41] Interviews indicated the overall response to Kimia II was to steal from villages they did not control.

Why did Kimia II Incentivize Violent Theft?

One possibility is that, by making it harder for the FDLR factions to internalize the effects of their theft operations in future village growth, Kimia II broke the pre-existing FDLR incentive to refrain from pillaging.[42] 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 show 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.[43] We then verify whether Kimia II reduced expropriation frequency, using our data for the frequency of the collection of each tax payment.[44] The frequency of FDLR expropriation events 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.

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the likelihood of a household in FDLR state experiencing an FDLR attack after Kimia II is 1.6% (baseline 0.00 + effect 0.016). A simple back-of-the-envelope calculation shows that this proportion corresponds to around 23 households being targeted in an FDLR attack in FDLR villages due to Kimia II ($n^* = 23$ is the value of n that satisfies $(1 - (1 - 1.6\%)^n = 31\%)$).

[41] From these interviews, we extracted 51 attacks. Based on the descriptions, we found pillaging to motivate 42 of the 51, (83%); 16 of these targeted a village of our sample; although the years are very uncertain, 12 of those coincide with an attack in our data within 2 years. In 11 of the 12, both villagers and perpetrators reported the attack to be motivated by pillaging the village.

[42] 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.

[43] Figure B.13 shows that one pillage permanently reduces the assets of pillaged households.

[44] These include toll fees (daily), poll taxes (weekly, monthly, depending on the episode), mill taxes (daily), market taxes (weekly), and the number of pillages per year.

We empirically test whether either the first or the second interpretation holds 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.[45]

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*

We now separately analyze the human consequences of these attacks for the households, and contrast those to the economic implications of Kimia II.

On the one hand, Kimia II caused a range of attacks on human dignity. Table III estimates Equation 2 using indicators of costs faced by the households as dependent variables.[46] 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 3.521 USD, total taxation payments per year per household decrease 41%, from 66.80 USD, in 27.171 USD.[47] Overall, household payments decrease from 67.34 USD in 24.41 USD yearly.[48]

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.[49]

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[45]High frequency is more than 1 event per year (the 90^{th} percentile). Results are insensitive to the cutoff.

[46]Figure B.17 presents the event study baseline coefficients.

[47]The average yearly household tax paid to non-state actors decreases in 70%, from 63.84 to 17.04 USD.

[48]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.

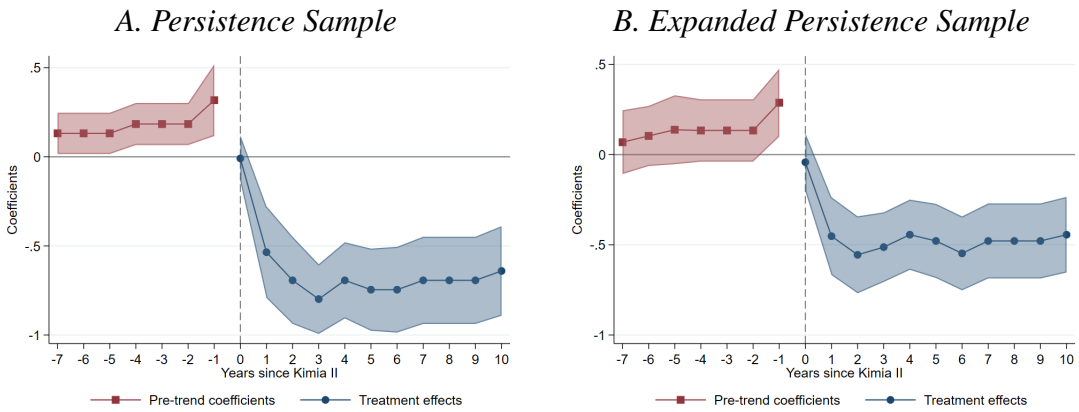
[49]National income per capita may be unrealistically low, since the FDLR state villages have gold artisanal mining. If this were true, this could give the false impression that the tax rate is higher than it is. To obtain

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)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	0.104**	0.056	0.179***	0.237***	3.521**	-2.200	-8.671***	-16.741**	-27.171***	-24.406***
	(0.040)	(0.051)	(0.055)	(0.053)	(1.451)	(1.559)	(2.874)	(7.250)	(9.192)	(8.199)
Observations	3,372	3,372	3,372	3,372	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	0.57	0.35	0.37	0.30
Mean Dep. Var.	0.04	0.06	0.08	0.06	0.50	4.13	18.30	44.37	66.80	67.34
Village Clusters	190	190	190	190	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	0.36	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, clustered two-way 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 clustered two-ways 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 clustering two-way at the village and groupement-year level yield identical results. Those are reported in Section 9 of Authors' Personal Supplemental Appendix I.

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*.[50]

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a locally-adjusted more realistic tax burden rate, we found no regional dis-aggregated income data other than the one presented in Sánchez de la Sierra (2014). We used these data to conduct the following exercises. First, using our survey data on household histories of assets acquisitions, we constructed an estimate for the value of household assets (predominated by cows, goats, and pigs). We have the yearly acquisition and loss of assets, but do not have the actual stock, hence we used the perpetual inventory method to produce a plausible stock. On average, a household held 921 USD (in USD of 2015) of assets in any given year. In that case, the tax burden equals 7% of assets held in cattle in the corresponding households. Arguably, this estimate is sensitive to the assumed rate of depreciation. Second, using our survey data and existing studies in the district of Mwenga, we also reconstructed the average per capita estimates of income arising from gold production to provide a lower bound of the area's per capita income (using arguably the most lucrative sector), and hence an upper bound of the tax burden: a. daily production of a gold miner: approximately $1g$ (**Source**: Geenen (2013)); b. total number of work days per year of a gold miner: 300 (**Source**: assumed, based on own qualitative work); c. local sale price of gold in 2008: 25 USD per gram (**Source**: average in the province using our data from Sánchez de la Sierra (2014), and also reported

Figure VIII, Panel A, left quadrant, 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 quadrant, 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%.[51]

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 $1(t = 2002 + k)$ is 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.[52] 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,

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in Marchais et al. (2022), Figure F.13); d. share of taxes on the gold mining sector (licences) over value sold by miners: 45% (**Source**: 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% (**Source**: 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% of yearly average per capita 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%.

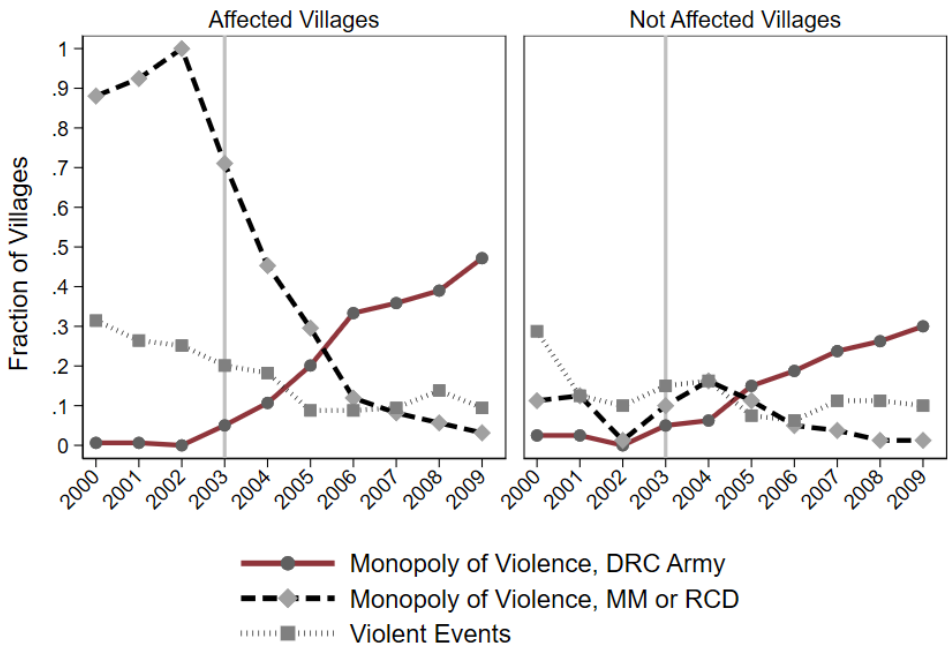
[50] See Autesserre (2006); Verweijen (2013); Stearns et al. (2013).

[51] Figure B.18 shows state and rebel control in a map around the agreement.

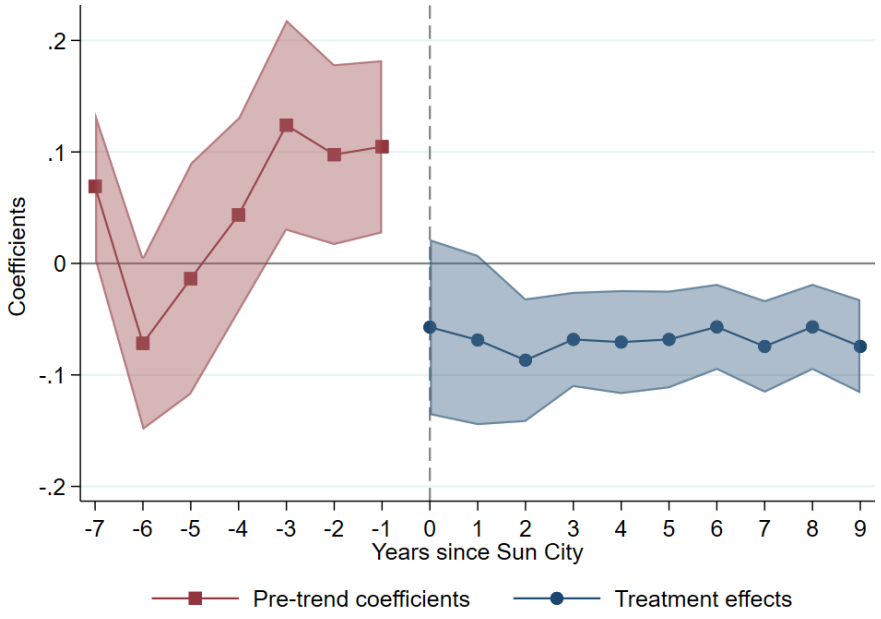
[52] Table B.10 presents the differences-in-differences shows that it is robust to the usual confounds.

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, non-state armed groups, as well as the fraction of villages where a conquest attempt takes place, by non-state armed actors. Panel B presents the estimated coefficients and confidence intervals from estimating Equation 1 using Borusyak et al. (2020)'s 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 clustered two-ways at the village level and the chiefdom-year level.

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 KST (2021) and Vogel (2021), and complemented this information with extensive searches.[53] 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 the decision to bargain with rebels sent a signal that becoming a rebel may translate into spoils.[54]

Second, qualitative scholars have suggested that this led to parallel networks within the Congolese army (Verweijen, 2015; Baaz and Verweijen, 2013). This resulted in officers arming other rebel groups or splitting with military equipment, often accusing the government of failing to commit to its original agreement in Sun City (henceforth, *mutinies*).[55] Figure IX, Panel B, shows that there is a large number of mutinies, yet only in the years following Sun City.[56] This is consistent with the qualitative literature, which suggests that integrating the rebels in the army gave them the cover and resources of the army.[57]

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[53]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.

[54]Many armed groups' stated objectives included obtaining positions of the high-ranked officers inside the Congolese national army. Documents of official demands of recently created armed groups demand direct access to the benefits of the state, often explicitly demanding ranks in the national army, state salaries and other benefits. For example, the Mai-Mai Kapopo public political demands includes: "*To recognize the ranks and positions of the military and police officers issued from the Mai-Mai Kapopo*;" The Coalition des Congolais pour la Libération (CCL, Coalition of Congolese for Liberation), demands include: "the sons and daughters of Uvira are also incorporated in the national government and are represented in Congolese diplomacy," "*representativeness in the higher management functions of public enterprises" and "in the provinces, that the sons and daughters of Uvira are also recruited in the public administration, in the capacity of division head.*" **Source**: Stearns et al. (2013).

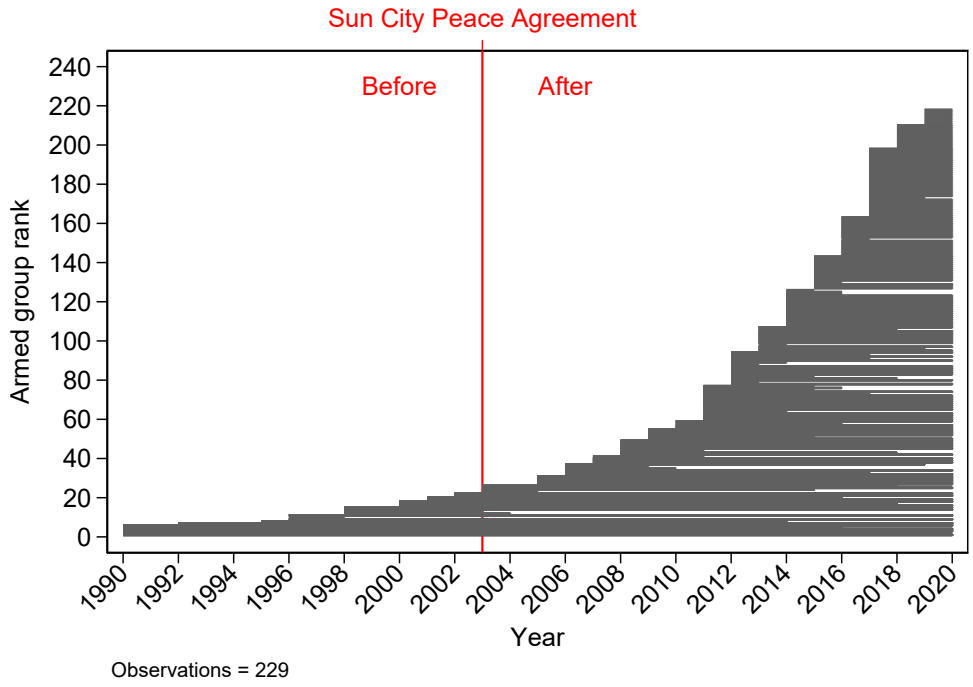
[55]For example Verweijen (2015, p.83) concludes: "*The 'open door policy' that was pursued towards military dissidents reinforced, rather than diminished, incentives for desertion and rebellion [. . .] Under the leadership of Laurent Nkunda, this group, which was supported by several other ex-RCD hardliners who were hostile to the transition, gradually built up a parallel military structure comprised of around 10,000 troops. Initially, these were mainly from the ex-RCD, but they included a growing number of new recruits. In this manner, Nkunda managed to establish an autonomous sphere of control within North Kivu, in particular in central Masisi [. . .] Building on a well-established network of businesspersons, administrators, and skilled officers . . . the CNDP rapidly became one of the strongest and most cohesive armed groups in the Congo (Stearns, 2008: 256–263).* " and "*Many combatants in the Kivus [. . .] were subsequently deployed within the Kivu provinces [. . .] facilitated the establishment of parallel chains of command.*"

[56]To determine whether the group is a mutiny from the Congolese army, we analyzed each armed group in KST (2021) and Vogel (2021) and complemented it with our own search.

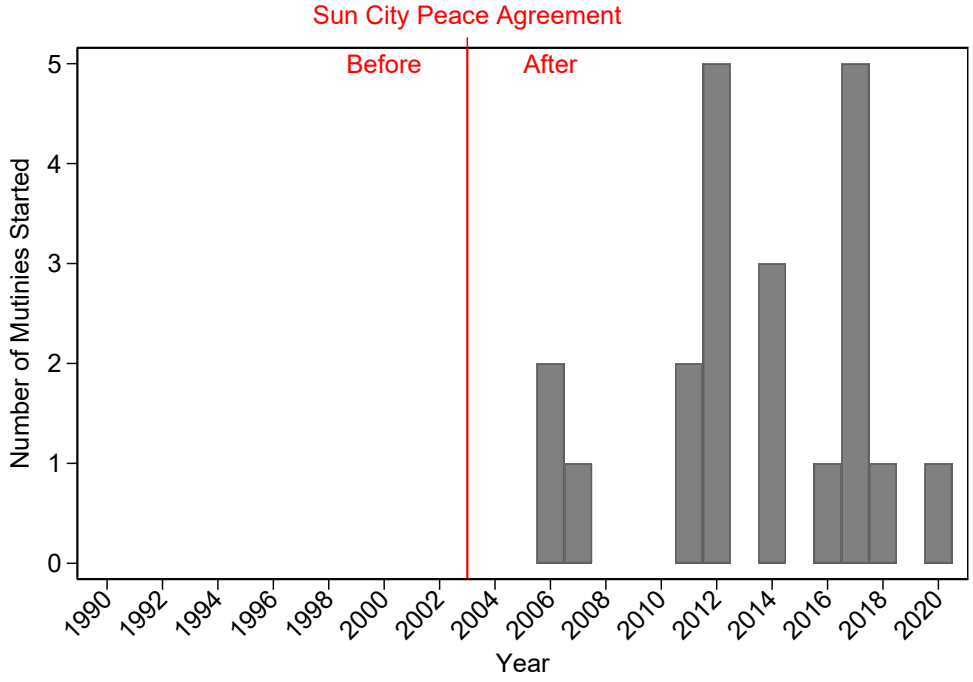
[57]"*However, most non-state armed groups managed to rapidly obtain the new [Congolese army] uniform [. . .] undermining the intention to make FARDC staff better distinguishable*." **Source**: Verweijen (2015, p.246).

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. **Sources**: KST (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.

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) 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."* **Source**: Verweijen (2015, p.113).[58]

In sum, while bargaining with the rebels over the spoils from the state in exchange for regaining the territory they formerly controlled did not lead to a rise in plunder, it led to the proliferation of new rebel groups and undermined the state's integrity in the long-run.

7. DISCUSSION: THE WEAK STATE'S TRADE-OFFS FROM TAXATION

A large share of states today and in history are too "weak" to assert control over the territory over which they have *de jure* sovereignty. Instead, large areas are controlled by rebels, who are often in *hard-to-access* "hinterland," where they can hide. Facing this challenge, governments have tended to use two responses: ousting or bargaining with the rebels.

Ousting the rebels from the territory they control and tax requires military resources, and even weak states have often been able to mobilize those. A common challenge faced by weak states is that rebels can often endure in the hinterland even if they concede territory, potentially exposing civilians to further violence if the state is unable to consolidate a monopoly of violence. To bargain over the territory, the state could make political concessions, or share the spoils of the state. In our case study, the state shared the spoils of the state by providing access to former rebels into lucrative positions inside the state army.

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[58] This author also describes this loss of legitimacy: "*a prominent idea held by the more educated strata of self-styled autochthonous groups 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, which strongly draws upon the 'foreign invasion' trope that is a hallmark of the autochthony discourse as employed in the Kivus [. . .]. 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.*" (p.114) This has been ironically picked up by the Congolese population by referring to the Congolese army acronym, FARDC (which stands for Forces Armees Republique Democratique du Congo) as "Rwandan Armed Forces in Congo" (which translates to FARDC in French) (see Verweijen, 2013). The loss of legitimacy of the Congolese army had two further consequences: it led to violent dissent against the army, and it perpetuated trauma, as the Rwandan rebels had previously brutalized rural eastern Congo in counter-insurgency operations. Verweijen (2015, p.114) notes: *"These processes could clearly be detected in Fizi at the time of the fieldwork, where the Mai Mai Yakotumba tried to foment hostility against the 65th sector."*

Our findings provide a new economic perspective on these challenges. Namely, we have shown that, while asserting territory in a location surrounded by *hard-to-access* hinterland succeeded in *permanently* reducing the households' unofficial taxation burden and shift taxation away from rebels and towards state officials—even if they remain unofficial, they turn to financing the income of state officials rather than that of rebels— it can create incentives for rebels to relocate further into the hinterland, and start using brutality to obtain resources from the very location where they taxed and provided protection. In many weak states, rebels are in the hinterland, hence this finding is quite generalizable. 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, we also provided suggestive evidence that bargaining with rebels is plagued with the difficulty to commit, on both sides. Indeed, we showed that 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—although this type of concessions is quite generalizable too. Yet a central aspect of these costs—the proliferation of armed groups—is in large part explained by bargaining more generally: the decision to negotiate with rebels can send a signal that the state can be brought to make profitable concessions if extorted enough, creating incentives to form rebel organizations in order to bargain with the state 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 can alternatively take two other actions. First, they can provide incentives for rebels to integrate society (de-mobilization programs). While the success of this approach has been limited in the DRC (Stearns et al., 2013), it can complement military attempts to dislodge rebels: by reducing the risk of plunder when it is highest. Second, a common strategy by weak states, the state can also opt for the *status quo*: tolerating the existence of

the rebels. This will be *more* socially desirable when rebels' territorial control is stable and thus when they have incentives to tax and protect citizens, and when the state is *weaker*.

Lastly, what is socially optimal will often not be rational for politicians (Bates, 2014). First, even when *status-quo* may be socially desirable, it can be politically irrational given the cost to the legitimacy of leaders as it signals their inability to govern, which is likely to be particularly strong in states with democratic institutions. Second, the effects of demobilization are dissipated and hence hard to claim, thus states may under-invest in demobilization. Third, while bargaining may yield short-term political gains against costs in a distant future, military operations have short-term political costs (visible violence), hence bargaining may tend to be chosen even when not socially desirable. Fourth, if rebels are a foreign group, as in Kimia II, bargaining may not be rational even when it is socially optimal. Finally, state capacity is also a government choice (e.g., Besley and Persson, 2009). Given a set of fundamentals implying that it is socially optimal to bargain, a rational government may choose to bargain and at the same time deplete state capacity in order to generate private rents from creating a patrimonial state and buy political support, while not internalize the effect of such depletion on the long-run social costs of bargaining.[59]

8. CONCLUSION

Given the desire to consolidate the international state system, donors have invested huge resources to strengthen weak states (Stearns, 2011; LSE-Oxford Commission on State Fragility, Growth and Development, 2017). Much of this effort is motivated by a territorial notion of the state (Weber, 1946). However, it is not obvious that asserting the state's territory is even socially desirable. We provide evidence that asserting the state can disrupt the rebels' public finance, precisely the source of their stability, substituting it with plunder. These findings suggest that the ability of rebels to tax citizens can be a source of the citizens' own protection when the state is too weak to assert its monopoly of violence. An alternative strategy, negotiating positions inside the army for the rebels, does not, but weakens the state's integrity in the long-run. A state unwilling to integrate rebels may be left with two options: regain territory by force, exposing civilians to violent theft, or do

[59] Verweijen (2015, p.347) notes: "*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 force that is well controlled, well resourced and well organized*."

nothing, accepting its inability to tax some areas. Unwilling or unable to share the spoils of the state with the rebels, weak states face a trade-off between taxation and safety.

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Online Appendix

Contents

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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)
$\text{FDLR}_i \times 1(t > 2009)$	0.240***	0.181**	0.238***	0.272***	0.216**	0.233***	0.268***	0.292***	0.425***	0.240***	0.184***	0.406*
	(0.068)	(0.071)	(0.067)	(0.065)	(0.099)	(0.068)	(0.072)	(0.069)	(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. (2020) 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	3,438	3,474	3,096	2,554	2,282	2,987	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, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *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. (2020) Estimator:* estimates the average treatment effect using Borusyak et al. (2020)'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)
$\text{FDLR}_\text{i} \times 1(t > 2009)$	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)
$\text{FDLR}_i \times 1(t > 2009)$	0.237***	0.258***	0.256***	0.243***	0.240***	0.236***	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)
$\text{FDLR}_i \times 1(t > 2009)$	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)
$\text{Log(Distance to FDLR State+1)}_i \times 1(t > 2009)$								-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 $\text{Log(Distance to FDLR State+1)}_i$ where Distance to FDLR State is the distance between village i and the FDLR state (in km) multiplied with year indicators. In this regression, all villages in the FDLR state have zero distance. In Column (8), $\text{FDLR}_i \times 1(t > 2009)$, is replaced with $\text{Log(Distance to FDLR State+1)}_i \times 1(t > 2009)$. 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)
$\text{FDLR}_i \times 1(t > 2009)$	0.202***	0.205***	0.225***	0.202***	0.192***	0.204***	0.197***	0.215***	0.367***	0.202***	0.223*
	(0.055)	(0.056)	(0.051)	(0.055)	(0.062)	(0.054)	(0.058)	(0.054)	(0.106)	(0.043)	(0.070)
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 $\times$ 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	340	357	340	340	340	340	340	357	170	340	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, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *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 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)		
$\mathbf{1}(t > 2009)$							-0.813***	-0.813***	-0.812***	-0.812***	-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 clustered two-way 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 clustered two-way 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 Theft	Taxation					
		Any	*Market*	*Mill*	*Toll*	*Poll*	*Mine*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{FDLR}_i \times \mathbf{1}(t > 2009)$	0.236**	-0.289***	-0.208**	0.001	-0.622***	-0.714***	-0.341***
	(0.092)	(0.110)	(0.101)	(0.004)	(0.104)	(0.086)	(0.078)
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	0.52	0.42	0.19	0.65	0.57	0.43
Mean Dep. Var.	0.04	1.00	0.28	0.00	0.83	0.94	0.38
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, clustered two-way at the village level and the Chiefdom-Year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu, and the FDLR state is a subset of South Kivu. *Observations:* is the number of year-village observations in each estimation. *Village Clusters* and *Chiefdom-Year Clusters* are the number of clusters included in 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)
$SC_i \times 1(t > 2002)$	-0.069***	-0.051**	-0.124***	-0.118***	-0.060**	-0.179***	-0.069***
	(0.023)	(0.021)	(0.040)	(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	4,266	4,302	3,924	3,259	2,987	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, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu. *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

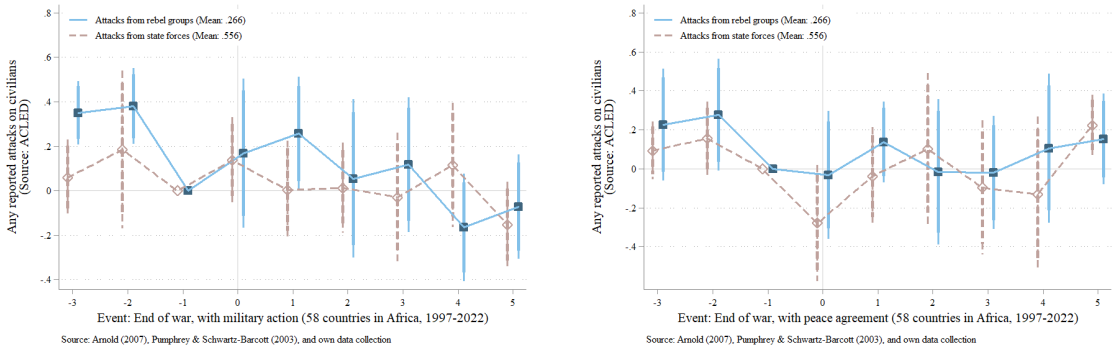
	Dep. Var.: *Disutility of Violence*				Dep. Var.: *Household Transfers to Armed Actors (USD)*					
	Village experiences:				*Pillage*	*Taxation*				*Total*
VARIABLES	*Rape*	*Death*	*Looting*	*Kidnapping*	*Theft*	*Market*	*Toll*	*Poll*	Total	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$SC_i \times 1(t > 2002)$	-0.032	-0.024	-0.039	-0.007	4.102	-1.216**	-2.958**	-6.504	-10.258*	-6.304
	(0.020)	(0.021)	(0.026)	(0.018)	(3.758)	(0.479)	(1.181)	(5.501)	(6.180)	(7.107)
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	1.937	8.890	27.87	40.52	41.06
Village Clusters	239	239	239	239	231	231	231	231	231	231
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	0.20	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, clustered two-way at the village level and the Chiefdom-year level, are in parentheses. *Village FE*: include village fixed effects. *Year-Province FE*: include year fixed effects separately estimated for each province. There are two provinces, North Kivu and South Kivu. *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.: Post-Colonial African States' Endings of Civil Wars, and Rebels Violence

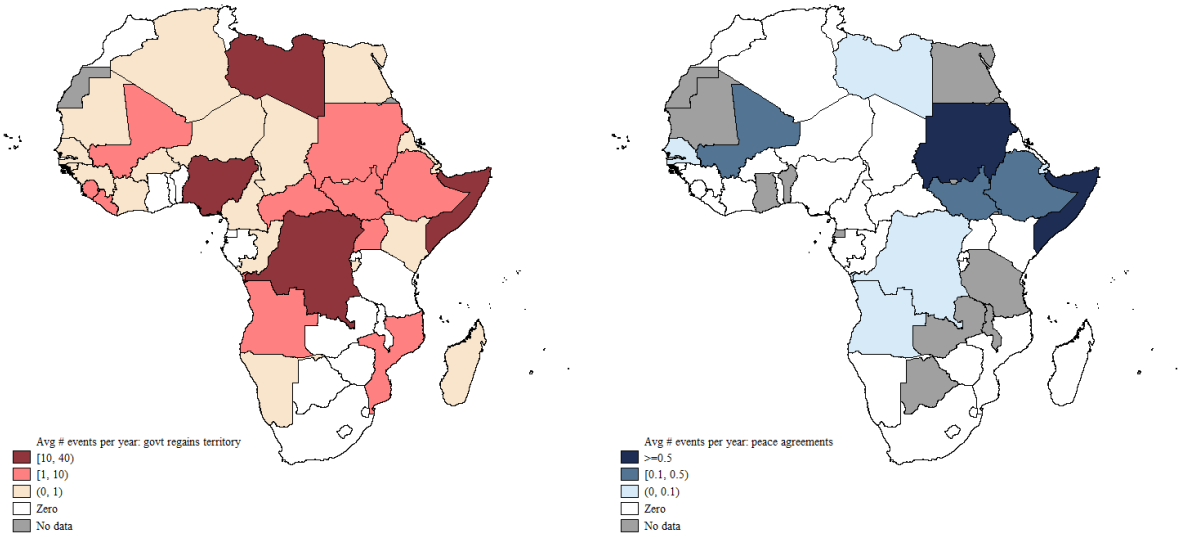
A. By Way of Military Operation *B. By Way of Peace Agreement*



Notes: This figure presents the event study estimates for rebel violence around the events of civil wars ending with the post-colonial 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.: Post-colonial African States' Regaining Territory from Rebels

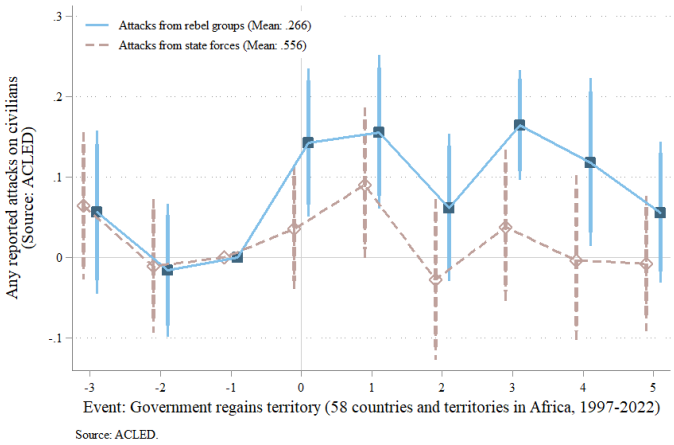
A. By Way of Military Operation *B. By Way of Peace Agreement*

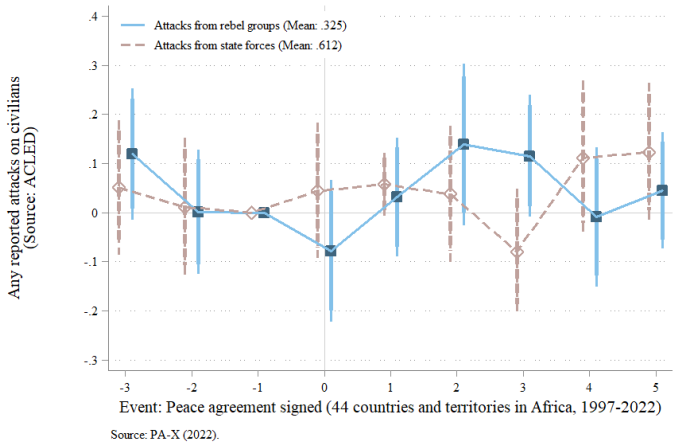


Notes: This figure presents the incidence across countries of events in which the post-colonial state regained territory by military operation (Panel A) and the prevalence of peace agreements signed by post-colonial states to regain territory from rebels (Panel B). The former uses data from ACLED (2022), 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)*

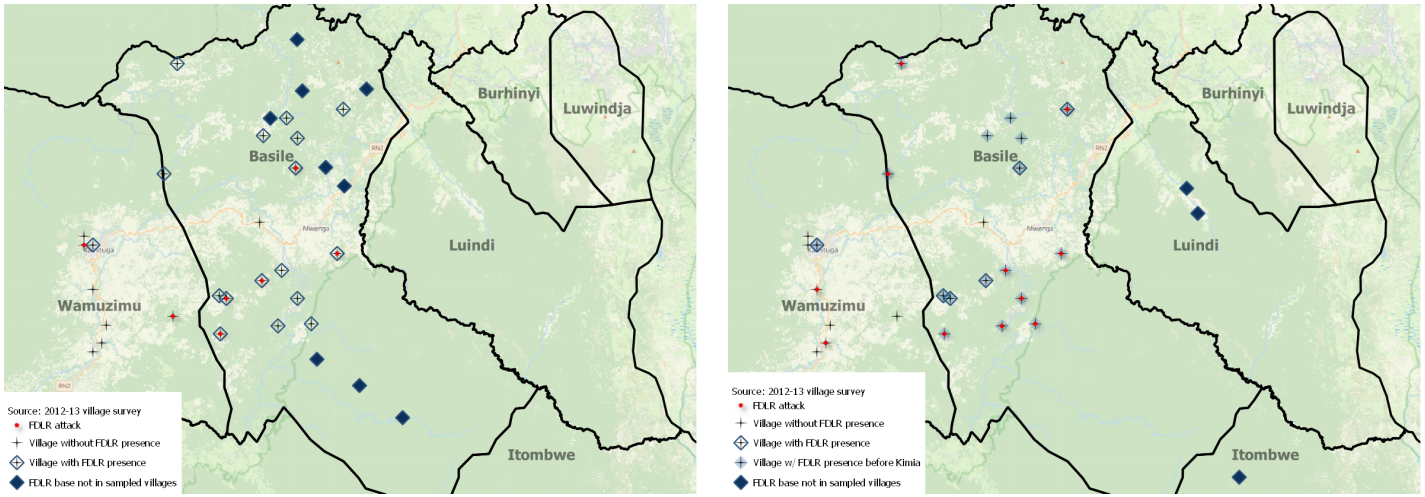




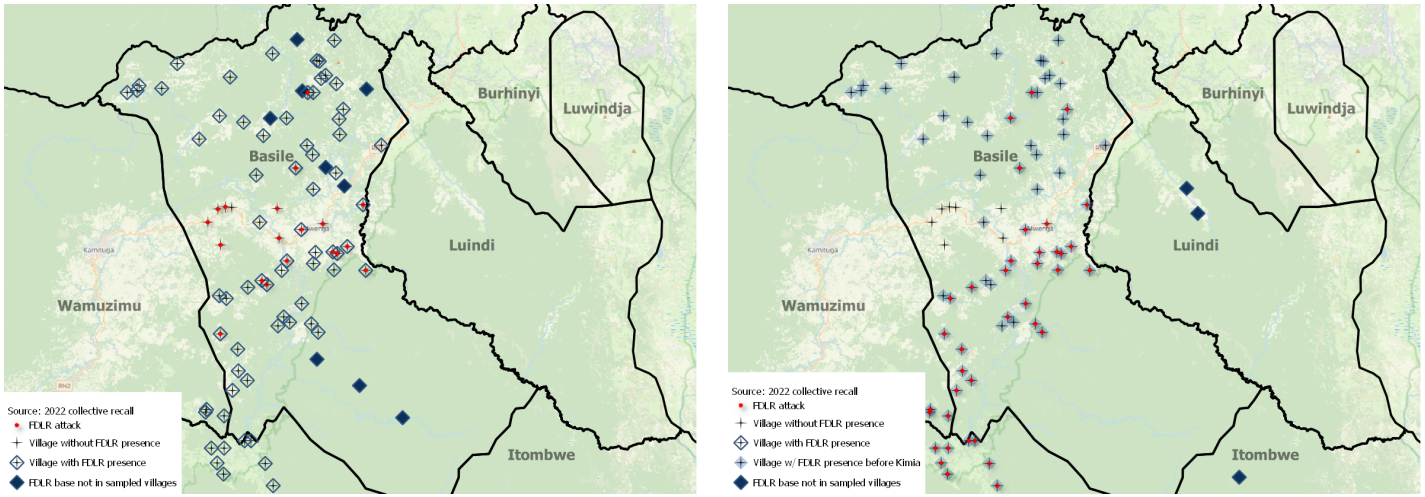
Notes: This figure shows estimates for the coefficients β_k estimated with OLS from the following equation: $Y_{it} = \alpha_i + \alpha_t + \sum_{k=-3}^{k=5} \beta_k \, 1(t = r_{it} + k) + \epsilon_{it}$ where α_i and α_t are country and year fixed effects, respectively. The dependent variable Y_{it} 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)

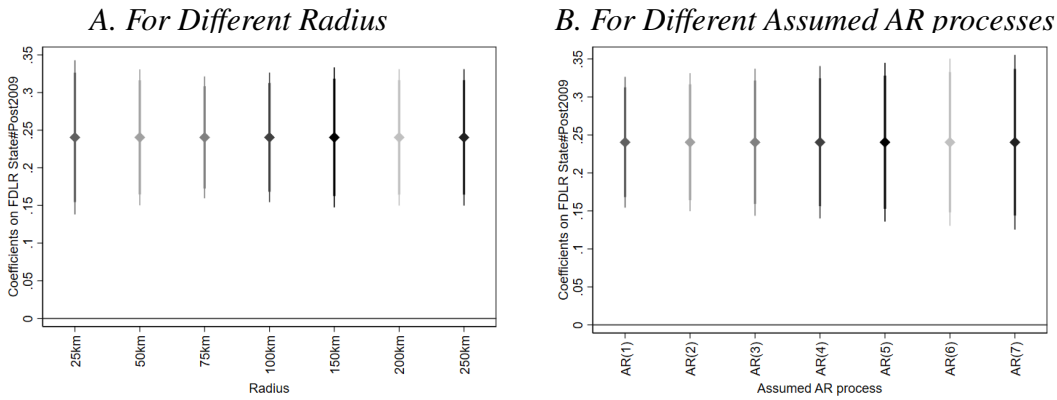


B. Against Universe of Villages (left: before Kimia II; right: after Kimia II)



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.

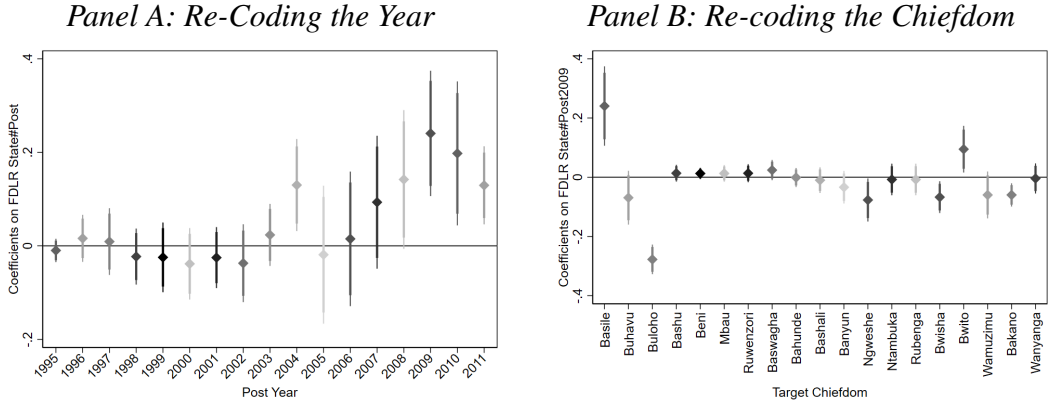
Figure B.5.: Accounting for Spatially Correlated Shocks with Conley (1999) Standard Errors



22

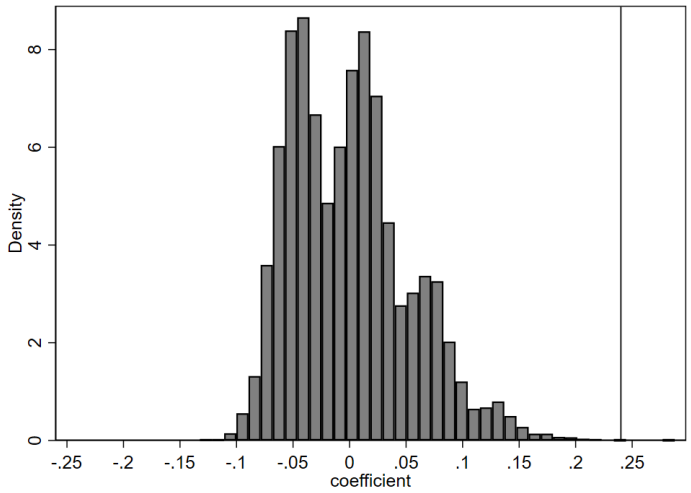
Notes: This figure shows the coefficients and standard errors estimated from Equation 2 but including Conley (1999) Standard Errors. Panel A shows standard errors for AR process 1, varying the radius (from 25km to 250km). Panel B shows standard errors with 100km radius, varying the AR process (from 1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

Figure B.6.: Alternative Treatment Definitions



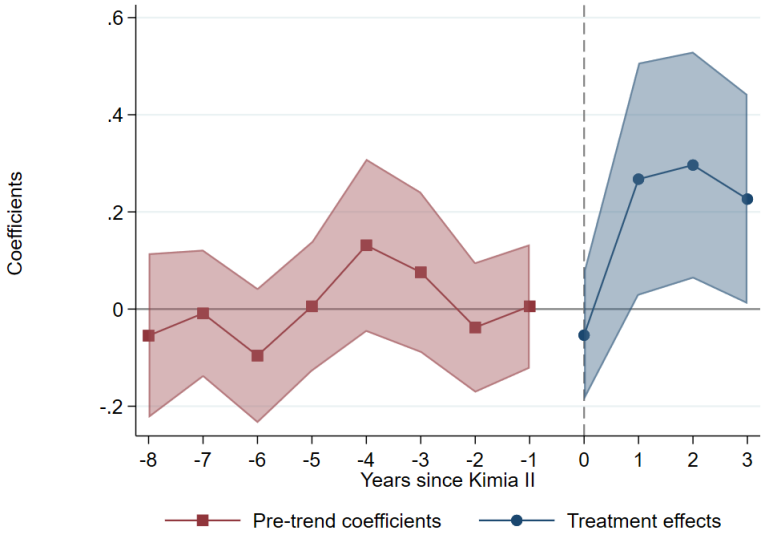
Notes: Panel A estimates Equation 2 for each possible cutoff year in defining the variable Post. The cutoff years for $1(t > 2009)$ are reported in the x-axis, while the y-axis are the magnitude of each coefficient and standard errors. Panel B does the same for each administrative division called Chiefdom. 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 clustered two-ways 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.

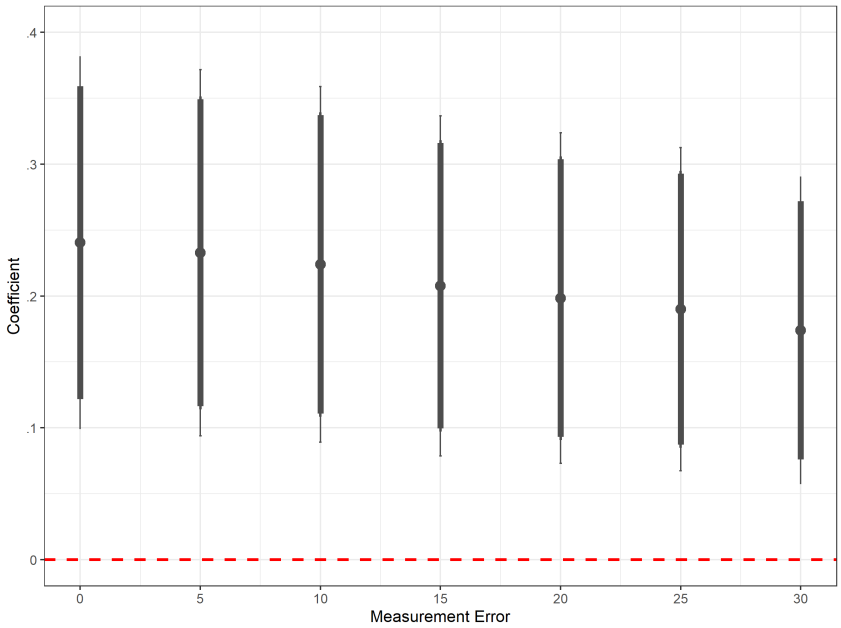
Figure B.8.: Pre-trends



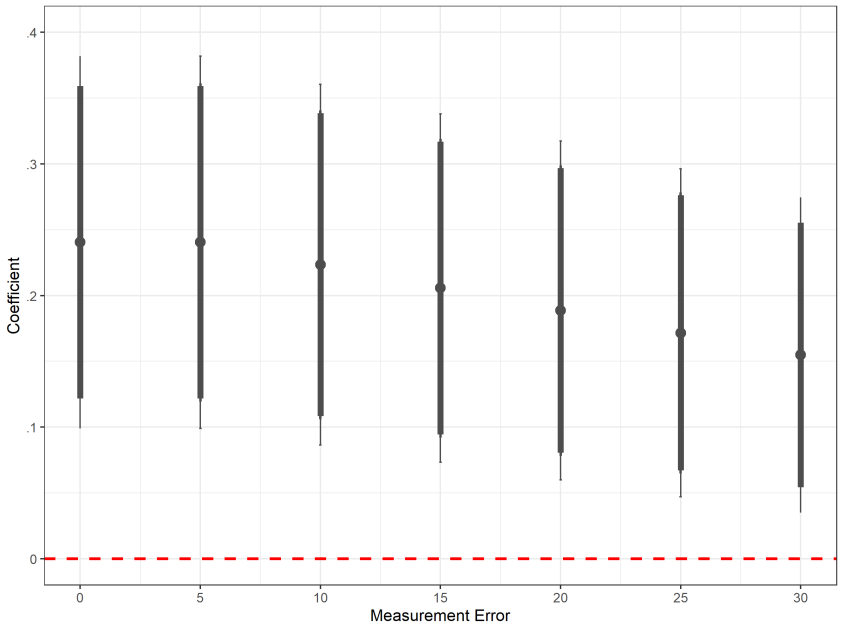
Notes: This figure shows the coefficients β_k, $k = -8, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020) estimator. The regression includes village and year fixed effects. The dependent variable is an indicator for whether the FDLR attacks 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 clustered two-ways at the village level and the Chiefdom-Year level.

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

A. Overreporting of Attacks in FDLR State



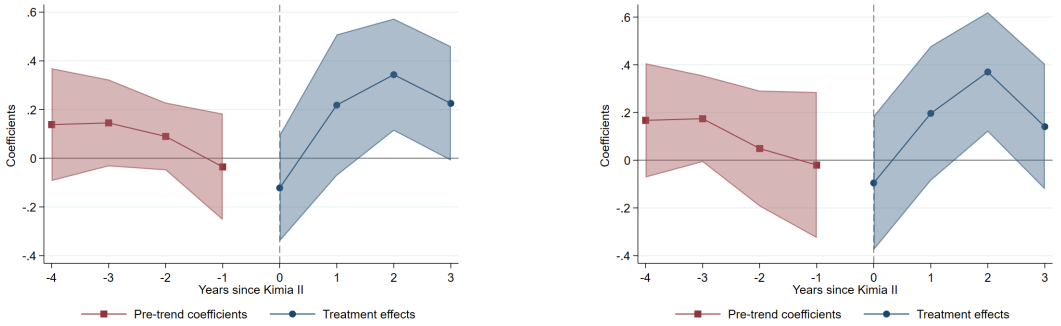
B. Overreporting of Attacks in FDLR State in Post



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.

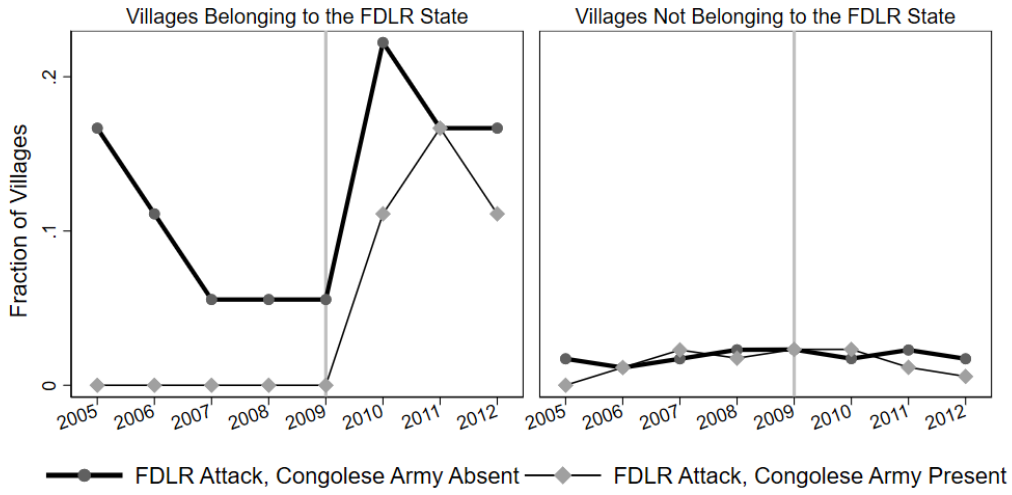
Figure B.10.: Validating the 2022 data collection with the 2013 data collection

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. (2020) 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.

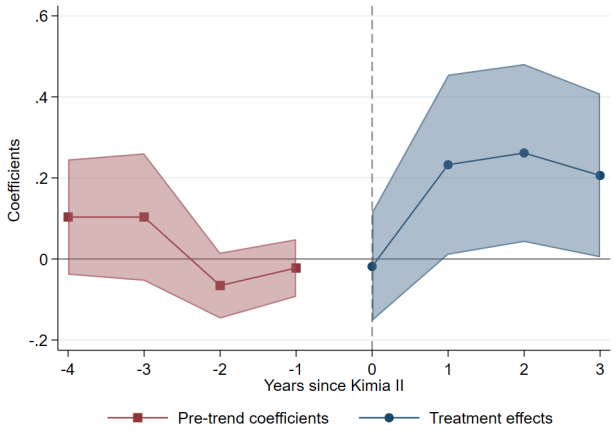
Figure B.11.: Trends of FDLR Taxation and Attacks, by Congolese Army Control



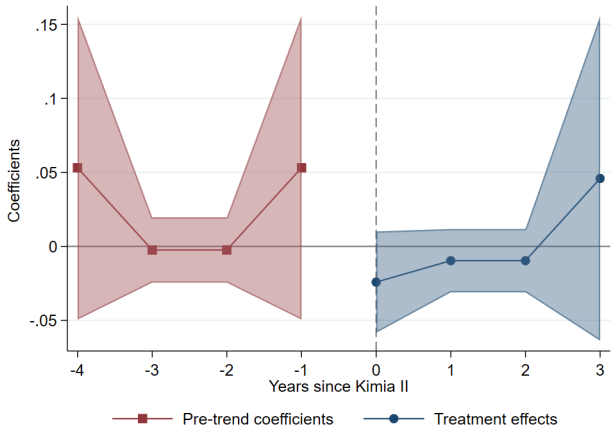
Notes: This figure shows the incidence of FDLR attacks separately for whether the Congolese army controlled the village and was present in the village at the time of the attack and whether the reported attack took place in a village either not controlled by the Congolese army or controlled by the Congolese 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

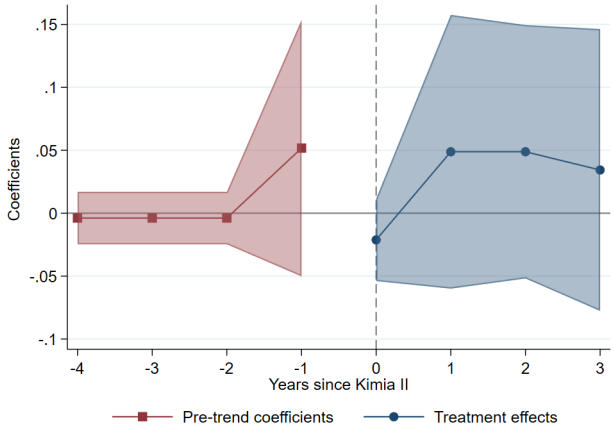
A. Attack with Intention to Pillage



B. Attack with Intention to Conquest

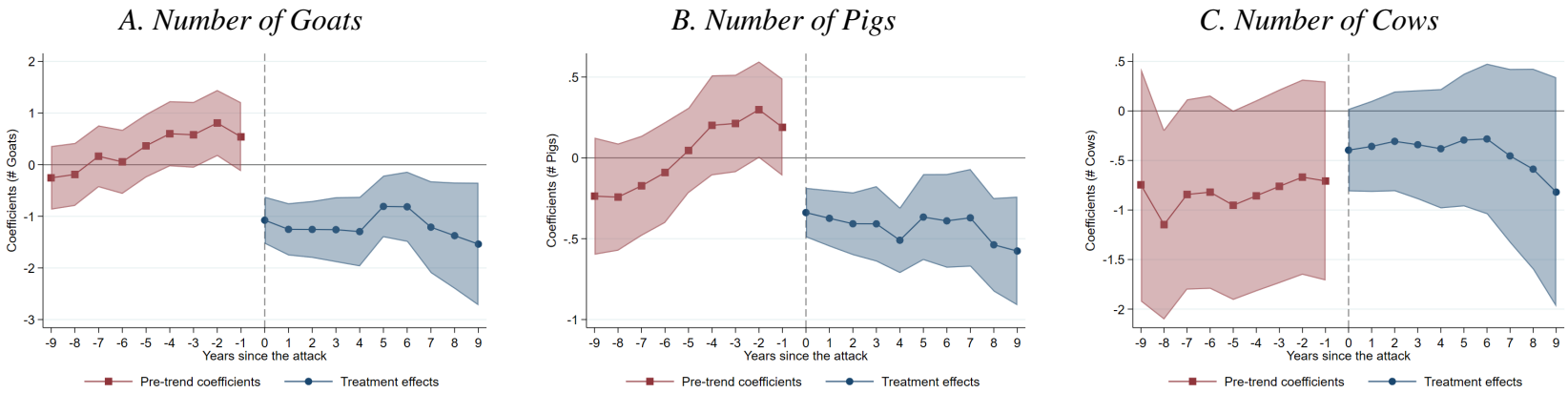


C. Attack with Intention to Punish



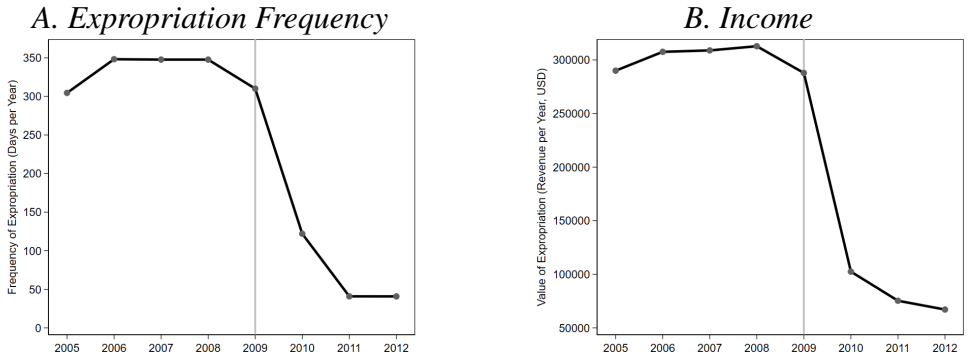
Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020) estimator. The dependent variable is an indicator for: whether the FDLR attacks village i in year t with the intention to pillage (Panel 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. (2020)'s efficient and robust estimator for the effect of a pillage on the household's assets.

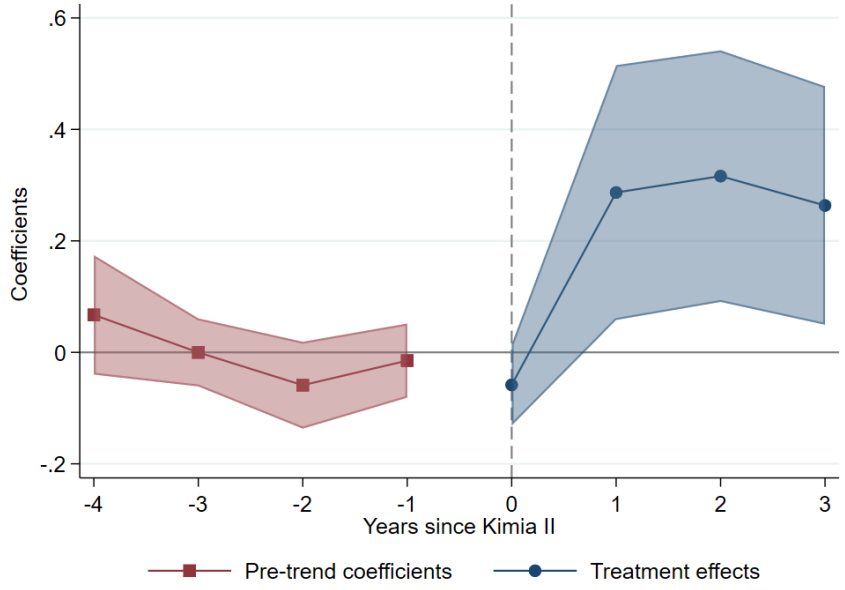
Figure B.14.: Mechanism: Expropriation, Value of Future Expropriations—Validation



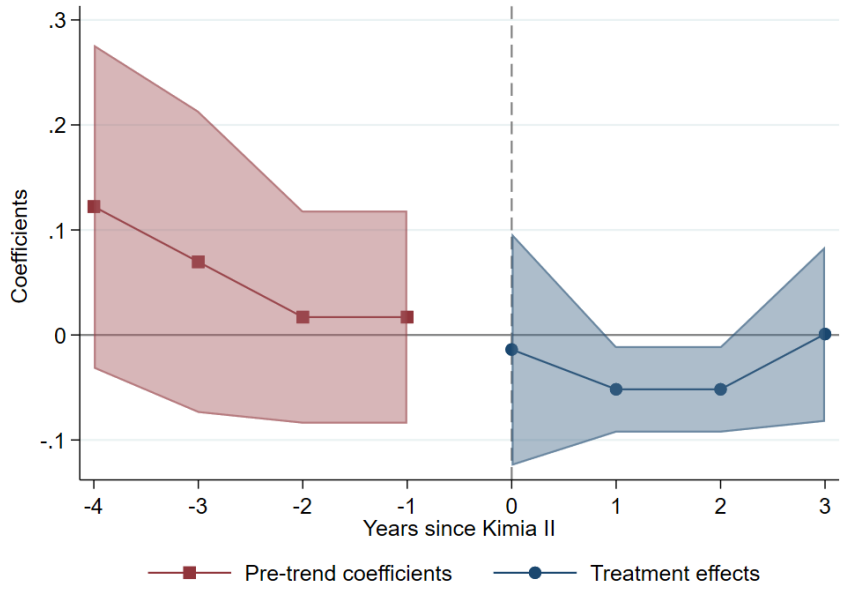
Notes: Panel A shows the trend of frequency of FDLR expropriation in the FDLR state. The frequency of expropriation is the sum of pillaging and tax payment events per year. Panel B shows the total income in USD generated by the FDLR in the FDLR state, through taxation and through pillage confounded.

Figure B.15.: Unpacking The Expropriation Channel

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



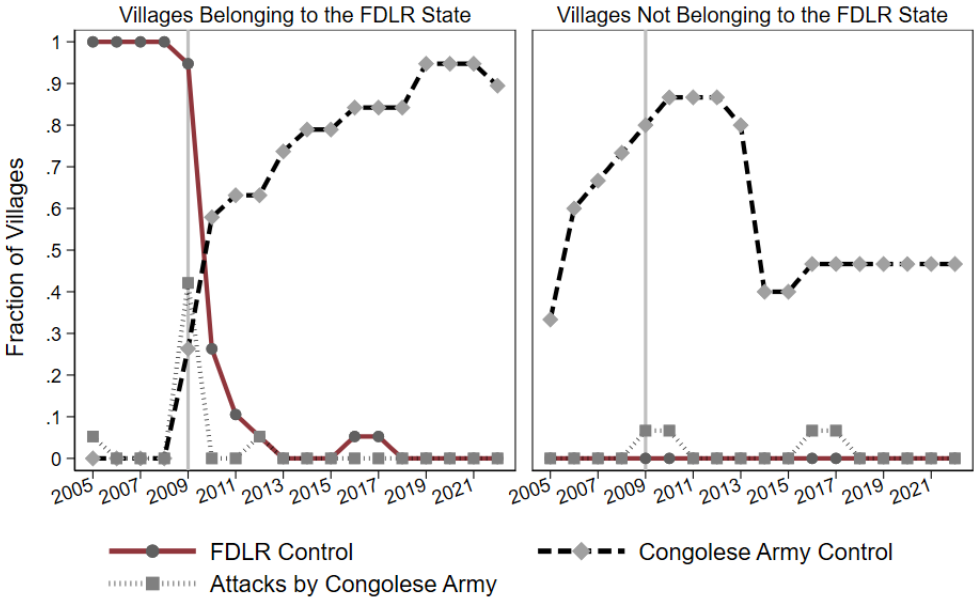
B. Dependent Variable: FDLR Violent Attack And Expropriation Frequency is High



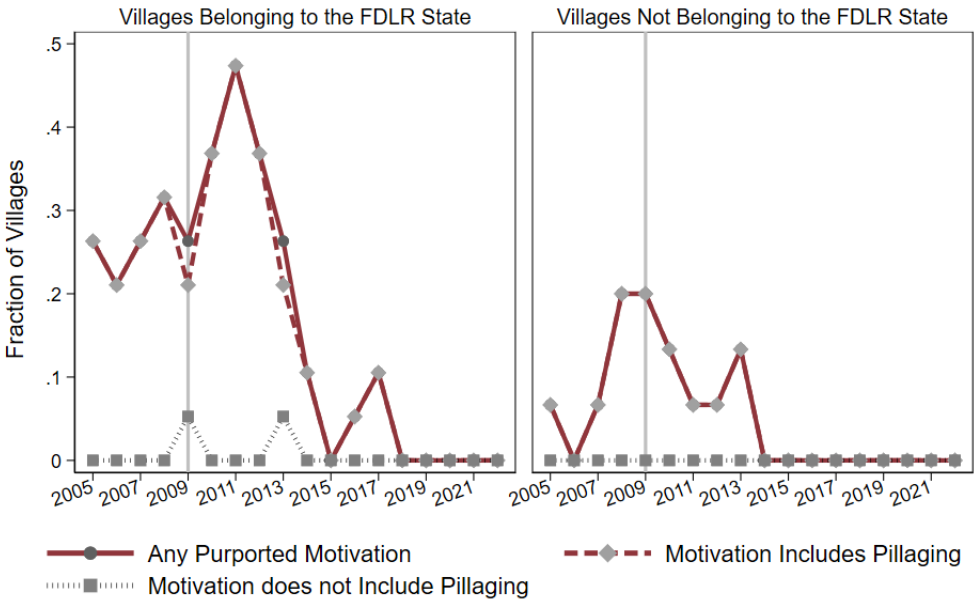
Notes: This figure shows the coefficients β_k, $k = -4, ..., 3$ and their corresponding 95% confidence intervals, estimated from Equation 1 using Borusyak et al. (2020)'s estimator. In the top panel, the dependent variable is an indicator for whether the FDLR attacks village i in year t and the FDLR 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.

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

A. 2022 data, timeline of control

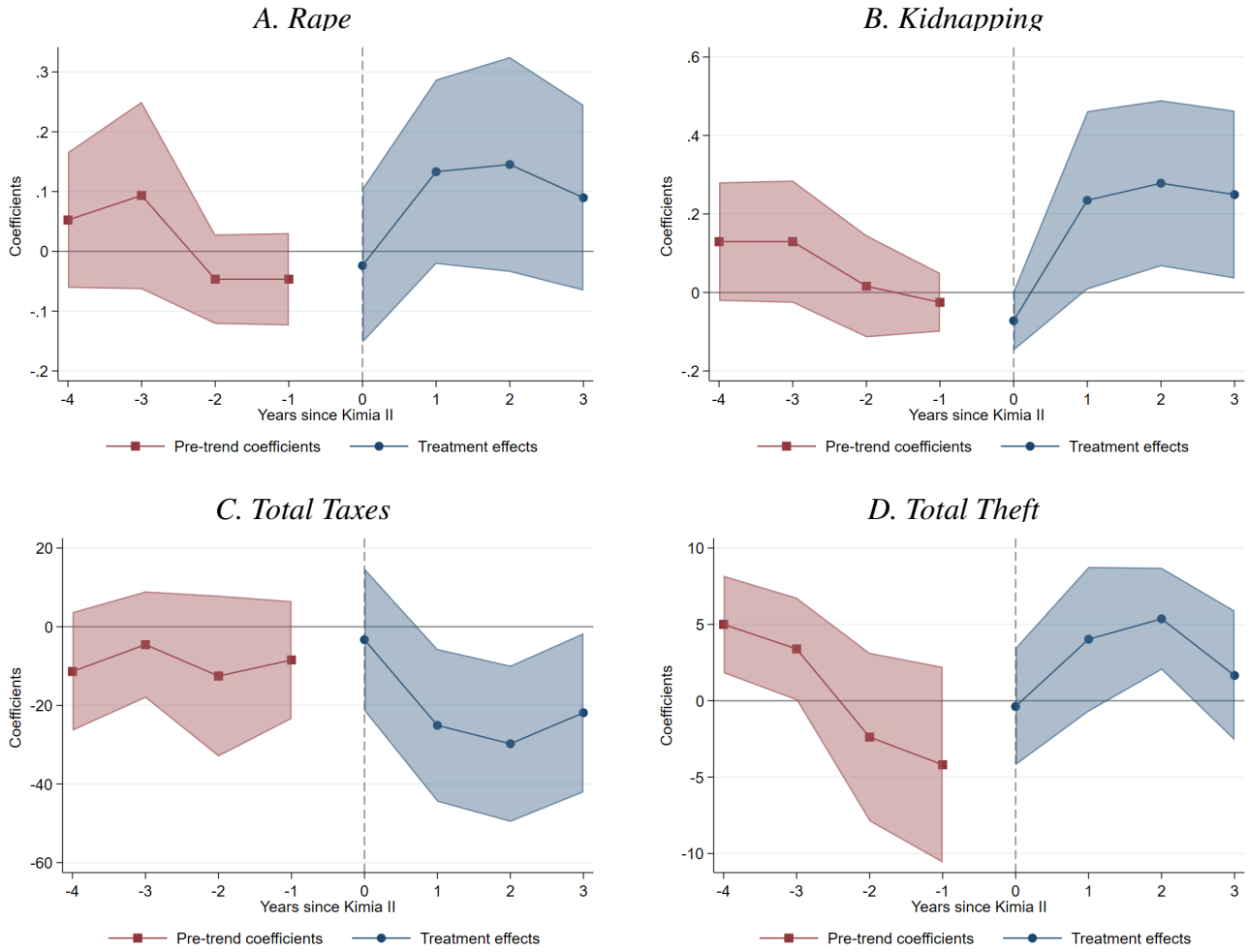


B. 2022 data, timeline of attacks



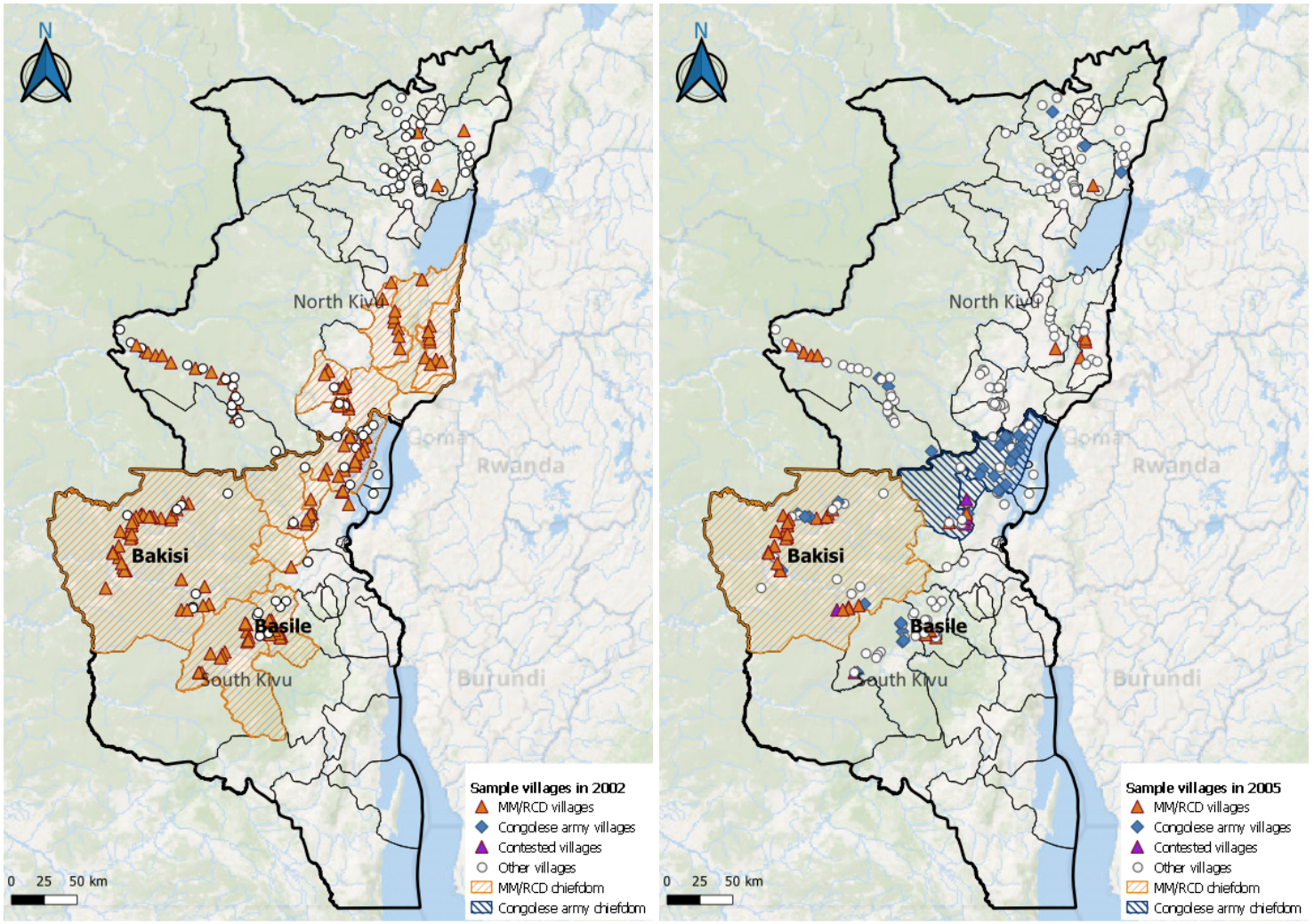
Notes: This figure is a replication of Figure III and Panel A of Figure IV. Panels A and B only use the data collected in 2022 for the villages that were also included in the 2013–2015 data collection.

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. (2020) 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.

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

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 $\sum_{t=0}^{\infty} \delta^t p^t u(\tau_t - s_t)$, where $\delta \in (0,1)$ is time preferences. $p\delta$ is the effective discount rate. Recursively,

$$V(a_t) = \max_{\tau_t, s_{t+1}} \{u(\tau_t - s_t) + \delta V(a_{t+1})\}, \tag{4}$$

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

$$\frac{u'(\tau_t)}{u'(\tau_{t+1})} = \delta p R \theta(s_{t+1}) \tag{5}$$

$$\frac{\theta'(s_{t+1})}{\theta(s_{t+1})} = (a_t - \tau_t). \tag{6}$$

Proof: envelope theorem and F.O.C. of the Bellman Equation and some algebra. Equation 5 is the Euler equation for τ_t. Equations 5 and 6 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.