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Can Audits Shift the Battleground? Supply Chain Certifications and Conflict Dynamics in the Congo

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Can Audits Shift the Battleground? Supply Chain Certifications and Conflict Dynamics in the Congo

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Abstract: We examine the impact of conflict-free certifications for small-scale gold mines, introduced to comply with the Dodd-Frank Act, on conflict dynamics in the Eastern Democratic Republic of the Congo. While Dodd-Frank enactment itself did not significantly alter conflict patterns, actual certifications displaced armed group-initiated conflicts from within a 10-kilometer radius around certified mines to other mining areas 25-75 kilometers away. Consistent with full displacement, overall conflict intensity within a 75-kilometer radius of certified mines remains largely unchanged, and conflict intensity is not decreasing in certification frequency at the aggregate territory level. Our findings suggest that conflict mineral certifications prompt armed groups to reallocate their protective activities from uncertified to certified mines but ultimately fail to contribute toward resolving the armed conflict in the Congo.

Keywords: Conflict-free certification, conflict, armed groups, small-scale mining, artisanal mining, mineral supply chain, supply chain due diligence, Democratic Republic of the Congo, DRC, Dodd-Frank Act Provision 1502.

JEL Codes: D74; E26; G18; J46; K22; K23; L72; M42; N47; N57; O13; Q34; Q38

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1. Introduction

Policy initiatives in developed countries increasingly seek to use global corporate supply chains to address human rights abuses in developing regions, where such issues have persisted due to weak public governance. A notable example is Dodd-Frank Act Section 1502, enacted in 2010 to mitigate the longstanding conflict in the Eastern Democratic Republic of the Congo (DRC). The DRC's extensive small-scale mining sector—integral to many corporations' global supply chains—has been linked to the financing of armed groups. Section 1502 aims to curtail these financial flows by requiring SEC-registered corporations to disclose their due diligence efforts to avoid minerals that fund armed groups. Local mine certifications that translate the Dodd-Frank mandate into local actions could disrupt equilibria of local armed group authority, which have long filled the public governance void (Sánchez de la Sierra, 2020; Henn et al., 2024; Marchais et al., 2024). However, without injecting functional institutions to replace these disrupted power structures, certifications may not contribute to resolving the ongoing armed conflict. In this study, we examine how conflict-free gold mine certifications, motivated by the Dodd-Frank Act, influence conflict dynamics in the DRC.[1]

More than 100 armed groups operate in the DRC, where the lines between governance and exploitation are often blurry. Some groups finance their activity by either violently looting mining communities or collecting tax payments through systems of governance that monopolize violence and provide "essential functions of a state" (Sánchez de la Sierra, 2020; Olson, 1993). While maintaining systems of governance is more resource intensive than looting, it may still be more lucrative

[1] Although Dodd-Frank requirements cover tin, tantalum, tungsten, and gold (3TG) mines, we focus on gold mine certifications to avoid confounding effects from several prior due diligence certification systems (e.g., CFSI, CTC, and iTSCi) that targeted 3T minerals. However, we also present corresponding analyses using 3T minerals.

for armed groups because it fosters local economic growth and thereby increases their tax revenues (Henn et al., 2024).

Several facts about the certification system help in predicting its impact. Specifically, the selection of mines for certification is determined by government officials, not mine owners. Auditors certify mines as conflict-free if there is no evidence that mines finance armed groups, allowing these mines to export minerals legally at higher prices than through unofficial smuggling channels (Alusala, 2017; Childs, 2014). Then, annual re-inspections of the selected mines reaffirm the conflict-free certification or provide another chance at certification, depending on the initial result. Although any financing to armed groups violates conflict-free certification criteria, auditors can more easily detect violent looting than taxation through governance systems. This is because audits are based on visual evidence of armed group presence at the mine, historic documentation, and interviews with the local population.

These features of the certification system incentivize armed groups to establish governance systems after the initial certification visit, which conceal their financial benefits by reducing conflict proximal to certified mines. This is because guaranteed annual re-inspection substantially increases the likelihood of future audits (from 2% to 100%) and detection of armed group financing leads to a reduction in the price of mineral output. Moreover, because certified mines have a higher expected mineral price than uncertified mines, certifications create a substitution effect where stationary armed groups with finite resources have an incentive to reallocate governance resources (e.g., protection) from uncertified to certified mines within their operational area. These mechanisms would prompt a displacement of conflict from certified to uncertified mining areas while not clearly predicting an aggregate effect.

To estimate the change in conflict near certified mines, we compare conflict intensity in the 10-kilometer area around a certified mine benchmarked against a control sample of uncertified mines within the same territory. Over a three-year period following the initial certification visit, we find a gradual decline in the probability of conflict within a 10-kilometer radius around certified gold mines, amounting to an 8.4 p.p. (31.0%) average decrease. Consistent with the reduction in conflict intensity being attributable to changes in armed group incentives, we observe a decline in the probability of conflict initiated by armed groups (i.e., battles, explosions, violence against civilians, and looting) after initial certification but little effect for non-armed group conflict (i.e., riots and protests against government elements).

If stationary armed groups with finite resources have an incentive to keep certified mines conflict-free, they could concentrate their protection efforts around certified mines and leave uncertified mines less protected, creating a substitution effect.[2] We examine the probability of conflict within rings of increasing distance away from mines. In contrast with the 8.4 p.p. decrease within 10 kilometers, we observe 1.9 p.p. (5.1 p.p.) increase in conflict probability within 25 to 50 (50 to 75) kilometers of certified mines. The overall increase in conflict probability from 25 to 75 kilometers offsets the reduction in conflict probability within 10 kilometers, resulting in a negligible aggregate change within 0 to 75 kilometers. Further corroborating the full displacement of conflict, we aggregate conflicts to the territory level (similar to Parker & Vadheim, 2017) and show that a higher count of certified gold mines in a territory is insignificantly but generally positively associated with conflict probability or fatalities. This full displacement is inconsistent with armed groups materially distributing increased resources gained from certified mines to protecting uncertified mines within their control (i.e., an income effect).

[2] Another related possibility is that stationary armed groups actively incite violence some distance away from certified mines to induce continued demand and payment for protection (Krauser, 2020).

The main challenge with interpreting these conflict dynamics as a causal result of certifications is that, even though mines are involuntarily selected for certification by the DRC government, selection is likely non-random. For instance, government officials may select mines for certification based on prior or predicted conflict patterns. Although a formal analysis of the selection process indicates that government officials do indeed select provinces for certifications based on past conflict dynamics, they do not seem to select individual mines within provinces or territories based on prior trends in local conflict. Moreover, the conflict dynamics we find around certifications are inconsistent with systematic selection unless government officials can predict localized conflict changes including displacement effects. They are also unlikely to be driven by other contemporaneous government policy changes unless it results in similar localized conflict patterns. Taken together, the evidence on selection criteria and the geographically nuanced conflict dynamics around initial certification visits alleviate concerns that systematic selection or contemporaneous policy changes are driving our results.

So far we have focused on conflict dynamics around actual certifications. We next consider how conflict intensity changes around the initiation of the certification system at the time of Dodd-Frank enactment, which has been the subject of analysis in prior papers (e.g., Parker & Vadheim, 2017; Stoop et al., 2018; Baik et al., 2024). Dodd-Frank and the contemporaneous prohibition of legal exports in the Eastern DRC reduced mineral prices for all DRC mines (Parker & Vadheim, 2017). The reduced price decreases stationary armed groups' incentives to invest in protection and governance by lowering the expected payoffs from taxation. However, the certification system also imposes a nonzero likelihood of audit (estimated at 2% per year), which may incentivize the protection of mines in anticipation of potential future audits. These two opposing forces make the net effect of certification program initiation on conflict intensity ambiguous.

To provide empirical evidence on the relative importance of these economic forces, we examine changes in conflict activity around Dodd-Frank enactment. Existing evidence suggests that Dodd-Frank enactment is weakly associated with increased conflicts around gold mines (Parker & Vadheim, 2017; Stoop et al., 2018; Bloem, 2023; Baik et al., 2024), but advances in data availability allow us to enhance the empirical design compared to previous studies. Regardless of whether we define treated localities using the count of nearby small-scale gold mines or inherent gold suitability (Girard et al., 2022), we find no significant change in conflict intensity at the time of Dodd-Frank enactment. The insignificant change in conflict dynamics is consistent with the opposing forces canceling out to an economically and statistically insignificant net effect.

Overall, our analyses suggest that conflict mineral certifications have little impact on aggregate conflict intensity in the DRC but do geographically shift conflicts. The lack of a significant net effect is perhaps expected given that the line between armed groups governing and exploiting local mining communities are often blurry. This evidence illustrates that a blunt policy instrument like a certification system can have unintended consequences when imposed in a complex geopolitical situation. Although our results do not suggest that certifications negatively affect the Congolese people, the extensive resources expended on the system by governments, NGOs, and corporations could potentially be directed towards initiatives that entail greater local benefits.

These findings contribute most directly to the literature exploring the impact of Dodd-Frank Section 1502 on conflict (Parker & Vadheim, 2017; Stoop et al., 2018; Bloem, 2023; Baik et al., 2024) and other humanitarian consequences (Parker et al., 2016) in the DRC, which has arrived at mixed conclusions. When focusing on gold mining areas, these studies generally find a positive association between Dodd-Frank enactment and conflict levels, which the authors argue is likely due to conflict displacement from tin, tantalum, and tungsten (3T) mines (which are easier to trace) to gold mines (which are harder to trace). When we revisit this question using recent improvements in data availability, we find no convincing evidence that Dodd-Frank enactment in 2010 had any material impact on conflict intensity in gold mining areas of the Eastern DRC. However, our main analysis makes a more substantial contribution to this literature by examining the implications of mine certifications triggered by Dodd-Frank, which is essential for evaluating the longer-run effects of the regulation and other mineral supply chain initiatives. Survey evidence (Jaillon et al., 2019) documents a strong difference in armed-group presence between certified (10%) and uncertified mines (40%), consistent with certifications reducing armed groups' involvement in certified mines. Our finding that conflict is displaced from certified mines to nearby areas but not reduced overall is consistent with the survey evidence but also provides an explanation for the limited success of Dodd-Frank in reducing overall conflict intensity in the DRC.

We also contribute to the recent literature that seeks to gain insight into the formation of early states by studying armed group activity in the Eastern DRC (Sánchez de la Sierra, 2020; Henn et al., 2024; Marchais et al., 2024). Many armed groups that begin as community defense mechanisms eventually establish systems of governance that monopolize violence, tax economic activity, and protect personal property. Extending this stream of literature, our study explores how external interference through global supply chains can disrupt such an equilibrium by altering the economic incentives of armed actors. Specifically, we show how a mineral certification scheme in the DRC, which yields an opportunity for increased armed group tax revenues from mining income, shifts spatial conflict dynamics in accordance with economic incentives. Our results underscore the risk that global supply chain initiatives can interact with complex local power dynamics to yield unforeseen and often adverse outcomes.[3]

[3] We also indirectly contribute to the broader literature showing the displacement of organized crime, often known as transference (Guerette & Bowers, 2009; Sandler, 2014). Deterministic theories of

2. Background and Theory

2.1 Armed Actors and Taxation Systems

The Congolese people have endured a longstanding conflict, which is concentrated in the eastern provinces and involves an estimated 120 armed groups (Matthysen & Gobbers, 2022).[4] The central government located in the far western capital of Kinshasa has little control over the eastern provinces or even its federal military personnel stationed in the area, leaving a power vacuum that has been occupied by many armed groups each controlling a subset of localities. In fact, these armed groups often blur the lines between exploitation and governance. While some violently loot settlements, others form as stationary community-based militias (Mai-Mai) that defend local communities and territories against abuse by other armed groups or even the federal military (Marchais et al., 2024).

To finance their operations, stationary groups extract rents from the economic activity in their communities, including small-scale mining (Henn et al., 2024). The prospect of continuous rent extraction gives stationary groups an interest in the long-term success of communities in their operational area. In addition to protection, stationary groups often resemble early-stage states by monopolizing violence and providing basic services such as schooling, healthcare,

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crime suggest that "blocking crime opportunities through situational alterations will inevitably lead offenders to seek out other crime opportunities" (Guerette & Bowers, 2009, pp. 1335-6). As theory predicts, empirical studies have shown that increasing the cost or difficulty of carrying out attacks in certain modes or venues shifts terrorist operations elsewhere (Enders & Sandler, 2006, 1993) and can even increase total attack frequency (Enders & Sandler, 2012). In our setting, armed group operations are indeed deterministic, as they are rooted in historical ethnic strife and continue to be fueled by statelessness. Consistent with the crime literature, we observe that the propensity of armed groups to geographically shift their operations undermines the ability of certifications to reduce aggregate conflict.

[4] At the center of DRC's conflict zone are the six far-eastern provinces (Haut-Uélé, Ituri, North Kivu, South Kivu, Tanganyika, and Haut-Katanga) and, to a lesser extent, the five neighboring provinces (Bas-Uélé, Tshopo, Maniema, Haut-Lomami, and Lualaba). See Figure 1 Panel A for a spatial representation of conflict intensity.

and judicial procedures (Sánchez de la Sierra, 2020; Olson, 1993). Hence, some stationary groups are not perceived as oppressive by local communities and are sometimes even respected locally as legitimate administrations (Henn et al., 2024). However, the situation evolves as the incentives of armed groups change. For instance, some Mai-Mai groups originally created to defend a community eventually evolve into oppressive organizations that violently loot. Relatedly, a group that protects and governs one community may perpetrate violence in another.

2.2 Mineral Due Diligence and Certification Requirements

Though most experts agree that the conflict is not primarily caused by mineral resources, recent policy interventions by governments in the US and Europe have focused on the role of minerals in financing armed groups. The first example of such a conflict minerals intervention is Dodd-Frank Act Section 1502 enacted in July 2010, requiring that SEC registrants perform and disclose the results of supply-chain due diligence on purchases of 3TG minerals.[5] Because Dodd-Frank designates all 3TG minerals from the DRC or an adjoining country as conflict minerals and requires disclosure of the geographic source, the immediate result was a widespread boycott of all 3TG minerals from the affected areas by SEC-registered companies (Parker & Vadheim, 2017; Parker et al., 2016). This boycott, along with a contemporaneous ban on artisanal mining by the Congolese government, forced artisanal miners to illegally sell (smuggle) minerals for lower prices (Alusala, 2017; Childs, 2014). In order to re-allow minerals to officially enter international markets, the International Conference on the Great Lakes Region (ICGLR) adopted a

[5] The European Union enacted similar regulation for 3TG minerals in 2017 (Regulation (EU) 2017/821). However, unlike the Dodd-Frank Act, the EU regulation focuses on mineral importers regardless of public listing status and is not limited to minerals from the DRC and neighboring countries. The EU regulation became effective in January 2021 toward the end of our sample. Such due diligence requirements focus on 3TG minerals because these are most often linked to armed conflicts and human rights abuses (see Dodd-Frank Act Provision 1502 and Regulation (EU) 2017/821).

conflict-free certification framework for artisanal and small-scale 3TG mining in 2011, covering mining areas in member states including the DRC.[6]

2.2.1 Prior Conflict Dynamics and Selection of Certified Mines

Limited resources are available for the mine certification program, and only 2% of known mines in our sample are initially audited each year. This creates a concern that mines are selected based on conflict dynamics, posing a direct threat to our research design. In addition, other economic and geographic characteristics that drive the selection process could also impact our conclusions. To address this concern, we provide evidence of the selection process based on available institutional information and observable data. From official documents and interviews with individuals involved in early inspections, we know that individual mines are selected by provincial offices of the Congolese Ministry of Mines, and mine owners cannot voluntarily subject themselves to audits. Based on this, we expect that a two-step selection approach is used in practice. First, provinces are targeted for certification in a given year by the central mining authority in Kinshasa. Then, mining offices in the selected provinces form a list of mines (or mining areas) for certification. As our estimation shows, actual selection patterns support this two-step approach.

We begin with the first stage of the selection process—the designation of specific provinces for inspections in a particular year. The distribution of gold mine certifications, shown in (Internet Appendix) Section IA1.1, is largely consistent with concentration in a few provinces each year. Provinces at the center of the conflict (e.g., North and South Kivu) seem to be selected sooner and more often.

[6] The ICGLR is an inter-governmental organization consisting of the DRC and several surrounding nations (not limited to those affected by Dodd-Frank). The certification scheme was initially financed and logistically supported by the German Institute for Geosciences and Natural Resources (BGR), a German governmental agency committed to sharing technology and supporting certification programs with the ICGLR.

To empirically test whether past conflict dynamics play a role in the provincial selection process, we estimate the following OLS regression:

$$\mathbb{1}(Cert_{p,t}) = \beta_1 \text{ asinh}(Conflict_{p,t-1}) + \beta_2 \text{ asinh}(Conflict_{p,t-1} - Conflict_{p,t-2}) + \beta_3 \text{ asinh}(Conflict_{p,t-2} - Conflict_{p,t-3}) + \delta_t + \varepsilon_{p,t} \quad (1)$$

($Cert$) is an indicator variable for whether any certifications occur in province p in year t. asinh($Conflict$) is the inverse hyperbolic sine of the count of conflict events occurring in province p in year t. δ_t is a vector of *Year* fixed effects. We cluster standard errors at the province level.

We report the results in Table 1 Column (1). The coefficients are consistent with high-conflict provinces being selected for certification with higher probability, fulfilling the ostensible purpose of the certification system. Additionally, provinces where conflict intensity decreases in the preceding year are more likely to be selected for certification, which may suggest that officials have motives to show progress in reducing conflict. This selection motive, if not addressed, poses a concern for estimating the causal effects of certification.

Moving to the second step, we now explore the procedure by which provincial mining offices in the targeted provinces select individual mines for certification. To empirically test whether conflict dynamics play a role in the provincial selection process, we estimate the following OLS regression:

$$\mathbb{1}(Cert_{m,t}) = \beta_1 \text{ asinh}(Conflict_{m,t-1}) + \beta_2 \text{ asinh}(Conflict_{m,t-1} - Conflict_{m,t-2}) + \beta_3 \text{ asinh}(Conflict_{m,t-2} - Conflict_{m,t-3}) + \delta_{p,t} + \varepsilon_{m,t} \quad (2)$$

($Cert$) is an indicator variable for whether mine m is selected for certification in year t. asinh($Conflict$) is the inverse hyperbolic sine of the count of conflict events occurring within 10 kilometers of mine m in year t. $\delta_{p,t}$ is a vector of *Province*×*Year* fixed effects. We cluster standard errors at the province level.

The results are reported in Table 1 Columns (2)–(4). This sample only includes mine-years located in provinces that are targeted for certification in a particular year because provincial governments only select mines in their own province.[7] Regardless of whether we include *Province*×*Year* fixed effects, more granular *Territory*×*Year* fixed effects, or other potential geographic and economic determinants of within-province selection, we observe that the level of conflict in the prior year is significantly and positively associated with mine selection. However, consistent with the parallel pre-treatment trends observed in the later certification analyses, we see that prior trends in conflict are not significantly associated with the selection of mines for certification both within-province and within-territory.[8] Although officials could still have similar motives as they do at the provincial level, it would be more difficult to track conflict changes at the local level (within 10 kilometers of mines).

Overall, our determinants model is consistent with central government officials selecting provinces for certification based on prior conflict dynamics. However, although provincial mining offices select mines for certification based on known conflict levels, they do not seem to select based on prior conflict changes. Thus, to alleviate the concern of selection on conflict dynamics, we include granular *Territory*×*Year* fixed effects in our main research design, which control for time-variant and -invariant territory-level geographic and economic characteristics. We further include mine fixed effects to control for time-invariant mine-specific features, such as local conflict levels. With these controls, we argue that the variation remaining in the selection of certified mines is plausibly

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[7] (Internet Appendix) Section IA1.1 details the determinants sample selection and composition and provides descriptive statistics.

[8] The coefficients on the other determinants in Column (5) also provide interesting insight. First, distance to the nearest certified mine is negatively associated with probability of selection, consistent with inspectors performing certifications on geographically concentrated missions. Second, nighttime luminosity and distance to the nearest road are negatively and positively associated with selection, respectively, indicating that mines that are less accessible and have lower economic activity are more likely to be selected. Third, distance to the nearest industrial mine is negatively associated with selection, consistent with well-known mines serving as a benchmark for the detection of mining areas. Finally, mines in protected areas are significantly less likely to be selected, indicating that inspection missions are unlikely to target areas where mining is illegal.

exogenous to past conflict dynamics. It is still possible that government officials are able to predict future conflict dynamics in specific geographic regions. However, we argue that this is unlikely given that selecting certifications in a way that fits the pattern of conflict dynamics we document would be very difficult and provides no clear benefit for provincial governments (see Section 5.3).

2.2.2 *Inspection Process and Certification Status*

After mines are selected for certification, auditors "physically visit and inspect the mine site and its immediate surroundings in person, gathering data by visual inspection, by reviewing the documentation and records of mine site owners or mine site operators, [and] by interviewing mine site operators, individual miners, security personnel, or others" (ICGLR, 2011, p. 9). The objective is to establish whether armed groups are directly or indirectly financed by the mine. If armed groups are violently looting mines or mining communities, then auditors can relatively easily detect armed group financing. However, establishing whether a mine contributes to armed groups is not straightforward if financing occurs by indirect taxation through governance of nearby mining communities. Mines that the auditors judge to be compliant are given a "green" certificate, allowing them to sell minerals to official exporters after the boycott (ICGLR, 2023).[9] If requirements are not met, a mine instead receives a "yellow" or "red" designation. Yellow-rated mines must be reevaluated after six months but can begin official sales immediately. Red-rated mines remain prohibited from selling officially for a minimum of six months but are then reevaluated for a higher status. Follow-up visits for mines are conducted at least annually with the same criteria and can result in downgrades if

[9] Of gold mines chosen for certification, more than 80% are initially rated green (see Internet Appendix Section IA3.1).

armed group financing is detected.[10] Hence, the selection of a mine for certification increases the likelihood of future audit to 100%.

2.3 Theoretical Framework

In practice, there are many armed groups in the DRC and many small-scale mines within a stationary group's area of operation. However, for simplicity, consider one representative mine controlled by one armed group. We assume that global gold prices are exogenous, and the amount miners receive for their output is fixed.[11] A certified mine can sell its gold officially to a refiner/smelter for export at the global gold price while an uncertified mine must sell unofficially to a smuggler at some discount. Thus, the mine owner's utility can be expressed as:

$$U(q) = \begin{cases} u(q), & \text{if passed certification} \\ (1-\alpha)u(q), & \text{if failed certification or not considered} \end{cases} \tag{3}$$

where $u(q)$ is the revenue received by a mine selling mineral output q, and α is the discount from selling illegally. $u(\cdot)$ is a linear function because both official and unofficial markets are assumed to be perfectly competitive.

An armed group controlling a mine extracts economic rents equal to a portion τ of the mine's revenue from gold sales (through taxation or looting). A mine has some nonzero probability p of being audited by an inspector in a given year. If audited, the mine will fail the audit (i.e., armed-group financing will be detected) with probability $r(c)$, which is decreasing and convex in the cost c the stationary group expends to protect the mine. For instance, armed groups that tax mines through monopolizing violence and establishing a governance system would correspond to a high cost c and low detection risk $r(c)$ while those that violently

[10] Mines can also be issued "blue" designations if inspectors are unable to obtain adequate information about armed group presence around the mine. In these cases, inspectors likely will not return to the mine in subsequent years.

[11] Other models (e.g., Parker & Vadheim, 2017) explicitly consider an outside option from which armed groups can obtain financing, such as looting farms. We do not explicitly consider such an option but implicitly assume that either taxing or looting mines is more profitable.

loot would correspond to a low cost c and high detection risk $r(c)$. Mathematically, the value that the group obtains from protecting and taxing the mine is:

$$V(p,\tau,q,c) = (1-p)(1-\alpha)\tau u(q) + p[(1-r(c))\tau u(q) + r(c)\tau(1-\alpha)u(q)] - c \tag{4}$$

Maximizing over protection cost c, we get the following first order condition:

$$r'(c^*)\tau\alpha u(q) = -\frac{1}{p} \tag{5}$$

which indicates that a lower discount α or a higher audit probability p for the mine will increase the optimal amount c^* the armed group spends on its protection.

The predicted conflict dynamics around mine certification are nuanced because certifications may have both an income and a substitution effect. For an uncertified mine, $p < 1$ is the probability of becoming certified (i.e., visited for the first time by an inspector). For a certified mine, regardless of certification outcome, there is certainty that the mine will be audited at some point within the next year ($p = 1$). This makes it relatively more beneficial to armed groups to conceal financing by protecting the certified mine, which then creates a substitution effect where an armed group under budget constraints optimally redistributes protection to certified mines from uncertified mines. On the contrary, since certified mines yield more tax revenue, the income effect could allow stationary groups to expand protection across all mines in their operational area, effectively reducing overall looting and violence. Thus, both the income and substitution effects predict a decrease in conflict proximal to mines, but the relative importance of the substitution and income effects determine the extent to which we observe a displacement of conflicts and, therefore, the overall changes in conflict.

3. Data and Sample

As in most prior studies on conflict in the DRC (e.g., Parker & Vadheim, 2017; Stoop et al., 2018), we limit all of our analyses to the eleven provinces in or adjacent to the Eastern DRC—where the certification scheme is centered, most of the small-scale mining is located, and conflict intensity is the highest (see Figure 1). Because we are interested in localized zones where armed groups operate, we limit our primary analyses to localities or mines with at least one conflict incident within 10 kilometers over the entire sample period. Finally, while Dodd-Frank requirements cover tin, tantalum, tungsten, and gold (3TG) mines, we focus on gold mine certifications to avoid confounding effects from several prior due diligence certification systems (e.g., CFSI, CTC, and iTSCi) that targeted 3T minerals.[12]

We have two main samples: one that we use in our primary analysis to examine the effect of certifications (*Certification Sample*) and one that we use in our additional analysis to examine the effect of initial Dodd-Frank enactment (*ITT Sample*). Below we first discuss the conflict data that we use in all our analyses and then discuss justifications of our sample selection criteria for the *Certification Sample* and *ITT Sample*, respectively. Section IA1 in the Internet Appendix details the number of observations excluded by each of the sample selection criteria in both samples.

3.1 Armed Conflict Data

We obtain geolocated conflict data from the *Armed Conflict Location & Event Database* (ACLED, see Raleigh et al., 2010).[13] The database includes information about the date, location, type, and fatality count of conflict events. In our analyses, we restrict conflicts to those at least geolocated to a small part of a territory, though most are geolocated to a precise village, town, or city. This criterion affords us more confidence in the spatial precision of conflict incidents (Berman et al., 2017). Next, we exclude most events that ACLED classifies as

[12] In (Internet Appendix) Section IA2, we present a corresponding certification analysis for 3T mines. Though the results are still consistent with a decrease in conflict around mines and displacement to areas further away, magnitudes and statistical significance are both much attenuated.
[13] This dataset can be found at https://acleddata.com/curated-data-files/.

"strategic developments," which do not reflect conventional violent conflict.[14] We classify the remaining events into armed group conflict (i.e., battles, explosions, violence against civilians, and looting) and non-armed group conflict (i.e., riots and protests against government elements). Finally, our sample period begins in 2004 to avoid the Second Congo War and its immediate aftermath (Parker & Vadheim, 2017) and because this timing provides us with a sufficient pre-period for Dodd-Frank enactment in 2010 and the certification scheme that started in 2011.

ACLED collects conflict events from local and regional news, humanitarian agencies, and research publications. One source of potential bias is the variation in media coverage across regions and time (Berman et al., 2017). Specifically, media coverage likely increases in areas that become safer. If certifications reduce conflicts, this would bias documented conflict incidence upwards and any post-certification decrease in measured conflict towards zero. More concerning, armed groups or locals have a financial interest in hiding conflicts from the public eye to prevent costly detection by certification inspectors. To alleviate these concerns, we also perform our analyses using the number of fatalities as an alternative conflict measure. It is likely harder to conceal events resulting in fatalities than less consequential conflict events.

To gauge variation in data coverage over time, we plot the number of conflict incidents and fatalities by year in Figure 2 with significant DRC conflict events superimposed. There is an upward trend in conflict observations, likely (at least partly) a result of increased database coverage. We attempt to address this issue by controlling for territory-level time trends using territory-year fixed effects, but this would still bias our estimates if within-territory trends line up with

[14] We exclude all sub-categories within "strategic developments" except for "looting/property destruction." Excluded categories are: "agreement," "arrests," "change to group/activity," "disrupted weapons use," "headquarters or base established," "non-violent transfer of territory," and "other."

certification selection. Consistent with the above argument that fatalities are more accurately measured, there is no apparent time trend in fatalities, and there are multiple spikes coinciding with various events that likely affected conflict intensity, such as the end of the Second Congo War.

3.2 Mining Data (Certification Sample)

We obtain location and certification information on small-scale mines from *IPIS Open Data*, which combines information collected from various missions in the DRC (IPIS, 2021).[15] These include ICGLR certification visits as well as ground visits by other organizations.[16] We define the treatment year as the year of (year after) initial certification if the initial visit occurs in the first (last) six months of the year. Since our conflict data is aggregated to the yearly level, this half-year adjustment ensures we do not define treatment too early for mines that are certified late in the year. We also exclude mines certified after 2019 for two reasons. First, an updated certification scheme was implemented in October 2019 which overhauled certification criteria and handed the responsibility for physical audit visits over to local auditors (BGR, 2019). Importantly, several non-conflict requirements (corresponding to OECD Due Diligence Guidelines) were added, which introduced other incentives to the certification mechanism. The competence and oversight of local auditors could also interfere with the effectiveness of recent audits. Second, 142 initial gold-mine certification visits were conducted in 2021 alone under the new scheme, and we are unable to track changes in conflict for an adequate number of years after these visits.[17]

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[15] This dataset can be found at https://ipisresearch.be/home/maps-data/open-data/.

[16] The IPIS documentation (IPIS, 2021) notes that only mines visited within these programs are documented, so the database is not an exhaustive list of small-scale mine sites. Given the extensive resources expended for these missions, we believe the sample includes most economically significant small-scale mines.

[17] Since we only have 135 initial certification visits between 2011 and 2019 in our current sample, including a large group of 142 certifications occurring in 2021 would result in a discontinuity in yearly regression estimates between post-2 (2023) and post-3 (nonexistent "2024").

3.3 Locality and Gold Suitability Data (Intent-to-Treat Sample)

We obtain locality data from *Open Street Map*, which documents all known populated settlements in the DRC.[18] We keep only cities, hamlets, towns, and villages, which are typically considered local population centers.[19] This data is defined as of January 2020 though we assume the locations of the population centers do not change throughout our sample period (2004–2023).

To proxy for artisanal mining activity around these localities, we use the gold suitability measure from Girard et al. (2022) who map geological bedrocks suitable for artisanal gold mining in Africa.[20] We classify a locality as gold-suitable if any such bedrock exists within 10 kilometers of the population center. Alternatively, we also use a continuous measure of the percentage of gold-suitable area within a 10-kilometer radius of a locality. While gold suitability does not necessarily indicate that artisanal gold mining is occurring, it is a more comprehensive measure than the limited surveys of artisanal mines in the area. We also have reasonable assurance that our control group consists of non-mining localities, as gold-unsuitable areas are unlikely to have mining activity. Moreover, gold suitability is more granular than prior studies that compare Dodd-Frank treatment territories with non-treatment territories (e.g., Parker & Vadheim, 2017; Stoop, 2018; Baik et al., 2024) and allows us to more precisely estimate within-territory effects.

4. Conflict Dynamics Around Initial Certification

4.1 Descriptive Statistics

[18] This dataset can be found at https://data.humdata.org/dataset/democratic-republic-of-congo-drc-localities-openstreetmap-export.

[19] These constitute the majority of documented localities. Other types include islands, refugee camps, and neighborhoods, which are not typically independent communities.

[20] We thank Victoire Girard and coauthors (Girard et al., 2022) for providing this data.

Table 2 reports descriptive statistics for all mine-year observations. Specifically, 6.7% of the gold mines are selected for certification. On average, 27.1% (26.2%) of mine-years have at least one conflict (armed group conflict) documented within 10 kilometers, so conflicts are reasonably prevalent but not universal in gold mining areas. There are an average of 3.2 fatalities from armed group conflict per year within 10 kilometers of a gold mine, but the variable is highly right skewed. We use conflict incidence as our main conflict proxy, but we also present specifications using fatality count. (Internet Appendix) Section IA1.2 details the certification sample selection and composition and provides additional descriptive statistics.

4.2 Conflict Near Mines

Both the income and substitution effects outlined in our theoretical framework predict a decrease in conflict proximal to certified mines after initial certification. Thus, we first estimate a staggered difference-in-differences design that compares the probability of conflict within a 10-kilometer radius of certified gold mines to the same probability around uncertified gold mines within the same territory. To observe conflict probability over time, we plot yearly coefficient estimates of the treatment effect from the following OLS regression:

$$\mathbb{1}(All\ Conflicts_{m,t}) = \beta_1\ CFC_m \times Year\ Relative\ to\ Cert_{m,t} + \alpha_m + \delta_{r,t} + \varepsilon_{m,t} \quad (6)$$

(*All Conflicts*) is an indicator variable for whether any conflict occurs within 10 kilometers of gold mine *m* during year *t*. *CFC* is an indicator equal to one if mine *m* is selected for certification. *Year Relative to Cert* is a set of indicators for each year defined relative to the year of the initial certification visit.[21] We omit the indicator for the year before certification, which serves as the benchmark period.

[21] As described in Section 3.3, we use a half-year adjustment, which pushes the certification year forward to the next year if initial certification occurs in the latter half of the year. In (Internet Appendix) Section IA3.3, we show that our results hold for both conflict incidence and fatalities without the half-year adjustment.

α_m is a vector of mine fixed effects, which control for differences in conflict intensity arising from time-invariant (or slow-moving) factors specific to each mine (e.g., geographic location, property size). $\delta_{r,t}$ is a vector of *Territory×Year* fixed effects, which control for prior conflict dynamics (see Section 2.2.1) and other time-varying institutional and political factors correlated with each territory. We estimate Conley (1999) standard errors that account for spatial correlation within a 100-kilometer radius and infinite serial correlation.

In Figure 3 Panel A, we graph the yearly *CFC×Year Relative to Cert* coefficient estimates and their corresponding 95% confidence intervals. In support of the parallel-trends assumption, treated and control cells have similar trends in conflict over the years leading up to certification. Consistent with certifications decreasing local conflict, certified gold mines exhibit a gradually decreasing probability of conflict within 10 kilometers relative to uncertified gold mines beginning the year after the initial visit. The treatment effect stabilizes at an approximately 13 p.p. lower level from 3 years after initial certification, corresponding to a statistically significant 48.0% reduction in conflict incidence per certified mine-year relative to the sample mean of 27.1 p.p.

Next, we estimate Eq. (6) using the inverse hyperbolic sine of fatality count in place of conflict incidence as the dependent variable.[22] As mentioned before, fatalities are likely measured more accurately than less severe outcomes, partially alleviating concerns over data coverage. Similar to the effect on conflict incidence, Figure 3 Panel B shows that certified gold mines exhibit a gradually decreasing count of fatalities within 10 kilometers relative to uncertified gold mines beginning the year after the initial visit. The treatment effect stabilizes at an approximately

[22] In (Internet Appendix) Section IA3.2, we also show that using the count of conflict occurrences as the dependent variable yields similar results. Magnitudes indicate a 37% reduction in conflict count by 3 years after initial certification.

43% lower level from 2 years after initial certification, corresponding to an effect size of 1.4 deaths per certified mine-year relative to the sample mean of 3.2 deaths.

In Figure 3 Panel C, we estimate the average treatment effect for all post certification years and several sensitivity tests. Specifically, we estimate Eq. (6) replacing the individual *Year Relative to Cert* indicators with a single *PostCert* indicator, which equals one for all years during or after the initial certification year (with half-year adjustment). The baseline ATE is negative and statistically significant at the 95% level, corresponding to an 8.4 p.p. (31.0%) average decrease. Next, we explore the sensitivity of the average treatment effect to various sample restrictions, fixed-effect structures, alternative tests, and standard error clusters that address several empirical concerns. First, we include only the provinces of Nord- and Sud-Kivu, where most conflicts and small-scale mining are concentrated and where the certification program began.[23] Second, we exclude non-green certifications, which may cause discontinued mine operations and resultant spillovers.[24] Third, we include zero-conflict mines to capture all mining areas rather than only conflict zones.[25] Fourth, we exclude mines within 2 kilometers of another mine in an attempt to partially alleviate the concern of spillovers from nearby mines while maintaining adequate statistical power.[26] Fifth, we exclude mines within 10 kilometers of a large mine to ensure our results are not driven by the influence of

[23] In (Internet Appendix) Sections IA3.4 and IA3.5, we show that the treatment effect is not driven by any one province or certification year.

[24] In (Internet Appendix) Section IA3.1, we estimate differential treatment effects for each initial certification outcome. While the decrease in conflict is the most salient after green certifications, there is a similar effect around yellow and red but not blue certifications. These findings are consistent with future audit risk driving armed groups' incentives to protect certified mines.

[25] It is plausible that mining areas that would have experienced an escalation of conflict in the absence of certification benefited from a reduction in future conflict. Supporting this story, the average effect magnitude attenuates slightly after including zero-conflict mines but remains significant at the 90% level.

[26] In (Internet Appendix) Section IA3.6, we show that the results hold when using smaller radii (i.e., 2 kilometers and 1 kilometer) and excluding all overlapping areas. This indicates that our results are not driven by double counting of conflict occurrences. If we were to exclude all overlapping areas with a 10-kilometer radius, the remaining sample would be too small to obtain reliable estimates.

industrial mines that are not included in the certification scheme. Sixth, we only retain observations three years before and after mine visits to reduce noise from openings and closures of mines.[27] Seventh, we shift to broader fixed-effect structures by replacing *Territory×Year* with either *Year* or *Province×Year* fixed effects.[28] Eighth, we perform a Stacked DiD, splitting certified mines into cohorts by certification year, to alleviate concerns over control group biases (Cengiz et al., 2019; Callaway & Sant'Anna, 2021). Ninth, we cluster standard errors by territory rather than spatially. Tenth, we cluster standard errors spatially within 50, 200, and 500 kilometers rather than 100 kilometers. A statistically significant (at least at the 10% level) and negative coefficient is maintained for each of these sensitivity tests, and the average effect magnitudes remain relatively stable and within the 95% confidence interval of the baseline specification across all sensitivity tests.

In our theoretical framework, the reduction in conflict intensity relies on the financial interest that stationary armed groups have in the continued success of the mine. Certifications allow mines to sell minerals for higher prices on official export markets while they risk the loss of such a privilege if armed group financing from the mine is detected during a subsequent follow-up visit. The framework predicts that armed groups should invest more resources in maintaining a monopoly of violence to ensure that the mine maintains conflict-free certification status. Accordingly, we expect to observe a reduction in armed group conflict (i.e., battles, explosions, violence against civilians, and looting) but no such effect for non-armed group conflict (i.e., riots and protests against government elements). In Figure 3 Panel D, we estimate Eq. (6) separately for armed group conflict and non-armed group conflict. Consistent with theoretical predictions, the conflict effect is driven

[27] For control mines, we have a date of visit because they are visited by the data collection agency (IPIS) rather than a certification inspector. For treated mines, we assume the date of certification is the date of visit.

[28] We observe an increase in the estimated average effect with less granular fixed effects, which is consistent with prior conflict trends documented in Section 2.2.1.

solely by a reduction in armed group conflict. There is no indication of a parallel-trends violation for either conflict type, but there is a salient decrease in armed group conflict commencing the year after initial certification while non-armed group conflict remains relatively constant. The treatment effect for armed group conflict stabilizes at an approximately 15 p.p. lower level from 3 years after initial certification, corresponding to a statistically significant 55.4% reduction in conflict incidence per certified mine-year relative to the sample mean of 27.1 p.p. Overall, consistent with theoretical predictions, we observe a significant and robust decline in conflict incidence and fatalities near certified mines after initial certification.

4.3 Geographic Displacement and Aggregate Conflict

Our theoretical framework indicates that initial mine certification should have two effects—an income effect and a substitution effect. The income effect predicts an overall decrease due to increased tax revenue available for armed groups to expand protection. The substitution effect predicts a redistribution of protective resources toward certified mines due to higher audit risk and higher mineral prices from remaining certified. While both effects would produce a decrease in conflict proximal to certified mines, they diverge as to whether this conflict is permanently eliminated or simply displaced to uncertified areas. To disentangle these two effects, we now explore evidence of conflict displacement and changes in aggregate conflict after certifications.

To assess the extent to which gold mine certifications displace conflict to surrounding areas, we examine changes in the probability of conflict within rings of various radii around certified mines after initial certification visits. Specifically, we estimate Eq. (6) but replace the dependent variable with an indicator for whether any conflict occurs between 10–25, 25–50, 50–75, and 75–100 kilometers from each mine during each year. We only report average effects for parsimony. In Figure 4 Panel A we graph the *CFC×PostCert* coefficient estimates and their corresponding 95% confidence intervals for each radius range including the 0–10 kilometer benchmark. The average effect is significant and negative within 10 kilometers of the mine, consistent with Figure 3 Panel C, and close to zero between 10 and 25 kilometers. However, the effect on conflict probability becomes positive between 25 and 50 kilometers of the mine and the positive effect gains statistical significance between 50 and 75 kilometers. The coefficient magnitude indicates a 1.9 p.p. (5.1 p.p.) increase in the probability of conflict between 25 and 50 (50 and 75) kilometers of a certified gold mine as compared to the 8.4 p.p. decrease within 10 kilometers. We also estimate the aggregate effect of initial certification on conflict probability within 75- and 100-kilometer radii, respectively. In either case, we find a negligible and statistically insignificant coefficient, indicating that the increase in conflict between 25 and 75 kilometers of a certified gold mine offsets the decrease in conflict within 10 kilometers of the mine. This full displacement of conflict supports that certifications have a significant substitution effect but not a significant income effect.

Next, we repeat the above estimation using the inverse hyperbolic sine of fatality count in place of conflict incidence as the dependent variable. Similar to the results for conflict incidence, Figure 4 Panel B shows a negative and significant average effect on fatalities within 10 kilometers of the mine and an effect close to zero between 10 and 25 kilometers. Fatalities increase between 25 and 50 kilometers of the gold mine and the effect becomes even larger between 50 and 75 kilometers, though neither estimate reaches conventional significance levels. The coefficient magnitude indicates an 8.6% (20.3%) increase in the count of fatalities between 25 and 50 (50 and 75) kilometers of a certified gold mine as compared to the 18.4% decrease within 10 kilometers. The aggregate effect within 75- and 100-kilometer radii is again statistically insignificant and close to zero, indicating that the increase in fatalities between 25 and 75 kilometers of a certified gold mine offsets the decrease in fatalities within 10 kilometers of the mine.

In Figure 4 Panel C, we estimate the average treatment effect within a 75-kilometer radius at the initial certification year and several sensitivity tests. The baseline ATE is statistically insignificant and close to zero, as shown in Panel A. We repeat each sensitivity test from Section 4.2. With the exception of changing fixed-effect structures, all sensitivity tests yield an ATE that is insignificant and negligible. Interestingly, using *Year* or *Province*×*Year* fixed effects yields a larger treatment effect that is positive and statistically significant at the 90% level. This is likely because territory-level increases in conflict are positively correlated with mine certification frequency (see Table 3 and the discussion later in this section). Overall, the effect of initial certification on conflict incidence within 75 kilometers is largely close to zero, inconsistent with the income effect of certification.

If armed group incentives indeed drive displacement, we should observe clear displacement for armed group conflict only. In Figure 4 Panel D we repeat the displacement analysis separately for armed group conflict and non-armed group conflict. While estimates are noisy for non-armed group conflict, the displacement effect remains clear for armed group conflict. The average effect for armed group conflict incidence is significant and negative within 10 kilometers of the gold mine and close to zero between 10 and 25 kilometers. The effect becomes positive between 25 and 50 (50 and 75) kilometers of the mine, indicating a 3.0 p.p. (3.8 p.p.) increase in the probability of armed group conflict as compared to the 8.4 p.p. decrease within 10 kilometers. The aggregate effect on armed group conflict incidence within 75 and 100 kilometers is also insignificant and close to zero. For non-armed group conflict, the estimates are quite noisy, even becoming positive and significant between 10 and 25 kilometers. However, the pattern and aggregate effect are inconsistent with the geographic displacement of conflict.

Finally, we aggregate both conflict intensity and certified gold mines to the territory level to assess the overall effect of gold mine certifications on conflict intensity at the territory level. This analysis is more aligned with prior studies that use mining propensities aggregated over large geographical cells to assess effects on conflict (e.g., Berman et al., 2017; Parker & Vadheim, 2017; Baik et al., 2024). It also utilizes the variation previously excluded by *Territory*×*Year* fixed effects to evaluate the overall success of certifications at improving territory-level conflict. Specifically, to capture the aggregate effect of certifications on conflict by territory, we estimate the average treatment effect from the following OLS regression:

$$\text{asinh}(\textit{All Conflict}_{r,t}) = \beta_1 \, \text{asinh}(\textit{Gold Cert Count}_{r,t}) + \alpha_r + \delta_t + \varepsilon_{r,t} \qquad (7)$$

asinh(*All Conflicts*) is the inverse hyperbolic sine of the count of all conflicts within territory r in year t. asinh(*Gold Cert Count*) is the inverse hyperbolic sine of the count of certified gold mines in territory r and year t. α_r is a vector of territory fixed effects, which control for differences in conflict arising from time-invariant (or slow-moving) factors specific to each territory (e.g., geographic location, population demographics). δ_t is a vector of *Year* fixed effects, which control for time-varying institutional and political factors that are correlated with each of the 11 provinces in the Eastern DRC. We estimate standard errors clustered at the territory level to account for serial correlation within a given territory across observation years.

We present average effects on the count of all conflicts, armed group conflicts, and fatalities in Table 3. With *Year* fixed effects, we observe a statistically significant and positive correlation between gold certification frequency and both all conflict and armed group conflict intensity. This can be interpreted as an elasticity—a 1% increase in the number of certified gold mines in a territory is associated with a 0.05% (0.06%) increase in the count of all conflicts (armed group conflicts) in that territory. Even though this is a modest correlation, it becomes much smaller and statistically insignificant after adding *Province*×*Year* fixed effects. While there could be concern that aggregating makes treatment effects too weak to be detected, the fact that most coefficients are positive or close to zero gives us confidence that certifications do not lead to an overall reduction in conflict. This aggregate territory result corroborates our previous finding that the reduction in conflict is limited to the immediate proximity around certified mines and, rather than being permanently mitigated, conflicts are displaced to uncertified areas. Altogether, our displacement and aggregate conflict analyses support the substitution effect of certification but are inconsistent with the income effect.

4.4 Endogeneity and Certification Selection

The main challenge with interpreting the conflict dynamics we document in Section 5.2 and 5.3 as causal effects of certification is that, even though mines are involuntarily selected for certification by government officials, selection is likely non-random. In Section 2.2.1, we conduct a formal analysis to substantiate how our research design addresses selection on prior conflict dynamics. In this section, we discuss the plausibility of government officials selecting mines for certification based on their knowledge of future conflict dynamics.

First, we may be concerned that officials predict localized conflict patterns and select mines for certification based on these predictions. In order to be a concern for our results, this selection would need to reflect both the decrease in conflict near mines (within 10 kilometers) and displacement to areas further away (between 25 and 75 kilometers). This would be operationally difficult without entailing any obvious gains to provincial mining administrations. Second, a more plausible concern is that certifications are bundled with other federal or provincial government action that affects local conflict dynamics such that we observe an effect around certified mines that is not attributable to the certification itself. One possibility is that military action, directed by the federal or provincial government, relocates protective resources towards certified mines from areas further away. Alternatively, the government could consciously reduce military offensives against armed actors around certified mines. However, both of these are unlikely given evidence that the federal army stationed in the area essentially also operates as an armed group without centralized control. Any observed changes in federal army activity are also likely to be driven by the same incentives as those of other non-state armed actors. In summary, our combined evidence makes it unlikely that certification selection based on prior or predicted conflict dynamics materially affects our ability to draw causal inferences.

5. Conflict Dynamics Around Dodd-Frank Enactment

In this section, we perform an additional analysis of conflict intensity around the time of Dodd-Frank enactment in 2010, which has been the focus of prior empirical evidence. Our theoretical framework would be consistent with two opposing forces around Dodd-Frank enactment. On the one hand, legal exports were banned for uncertified mines shortly after Dodd-Frank, introducing a mineral discount which should cause armed groups to decrease their level of protection in mining areas. On the other hand, the anticipated start of the certification system introduces an unconditional audit probability and provides audited conflict-free mines the chance to resume their sales at the higher global price, which should incentivize armed groups to increase protection around mines. To assess which of these effects, if either, dominates in equilibrium, we explore changes in conflict levels for mining localities around Dodd-Frank enactment. While prior studies evaluating the impact of Dodd-Frank compared changes in conflict intensity in territories with a higher prior level of violence to those of other territories (Parker & Vadheim, 2017; Stoop et al, 2018; Bloem, 2023; Baik et al., 2024), we use a more granular research design that compares localities within territories that are geologically suitable for gold mining with those that are not.[29] This allows us to use

[29] Prior studies assign treatment to 27 territories that were subject to the DRC Government's short-term embargo (see Section 2.2) and/or the US State Department's Section 1502 map of conflict zones (using other Eastern DRC territories as the control sample). The 27 territories are all located

a tighter control group of gold-unsuitable localities that strips out systematic time-varying differences in territory-level conflict intensity.

5.1 *Descriptive Statistics*

Table 4 reports descriptive statistics for all locality-year observations. Specifically, 51.0% of localities have any gold-suitable bedrock within 10 kilometers.[30] On average, 32.9% (31.2%) of locality-years have at least one conflict (armed group conflict) documented within 10 kilometers, consistent with high conflict intensity in the Eastern DRC. There are an average of 5.7 fatalities from armed group conflict per year within 10 kilometers of a locality. Since both conflict counts (not tabulated) and fatality counts are severely right-skewed, we follow prior literature (e.g., Berman et al., 2017) and use conflict incidence (i.e., probability of conflict) within specified regions as our main proxy for conflict intensity. We also present specifications using fatality counts, which are important in larger cells with little variation in conflict incidence. (Internet Appendix) Section IA1.3 details the ITT sample selection and composition.

5.2 *Dodd-Frank Act and Conflict Near Mines*

We estimate a difference-in-differences design that compares the probability of conflict within a 10-kilometer radius of gold-suitable localities to the same probability around gold-unsuitable localities within the same territory. To observe conflict probability over time, we plot yearly coefficient estimates of the treatment effect from the following OLS regression:

$$\mathbb{1}(\textit{All Conflicts}_{v,t}) = \beta_1\, \textit{Gold Suitable}_v \times \textit{Year}_t + \alpha_v + \delta_{r,t} + \varepsilon_{v,t} \quad (8)$$

in or adjacent to North and South Kivu, which have long been the center of violent conflict and small-scale mining. Dodd-Frank is not restricted to minerals from these territories, but they are likely most affected because of high conflict intensity and small-scale mining frequency.

[30] Since our sample is limited to Eastern DRC localities, the proportion of gold-suitable areas is very high (see Figure 1 Panel B). However, we also perform an analysis using a more conservative measure—the number of known artisanal gold mines.

$\mathbb{1}$(*All Conflicts*) is an indicator variable for whether any conflict occurs within 10 kilometers of locality v during year t. *Gold Suitable* is an indicator equal to one if locality v has any gold-suitable bedrock within 10 kilometers (Girard et al., 2022). *Year* is a set of indicators for each calendar year. We omit the indicator for the year 2010, which serves as the benchmark period. α_v is a vector of locality fixed effects, which control for differences in conflict intensity arising from time-invariant (or slow-moving) factors specific to each locality (e.g., geographic location, population). $\delta_{r,t}$ is a vector of *Territory*×*Year* fixed effects, which control for prior conflict dynamics (see Section 2.2.1) and other time-varying institutional and political factors correlated with each territory. We cluster standard errors at the territory level for consistency with prior literature.

In Figure 5 Panel A, we graph the yearly *Gold Suitable*×*Year* coefficient estimates and their corresponding 95% confidence intervals. Other than the year 2009—when the Congolese and Rwandan militaries launched a joint offensive against the Democratic Forces for the Liberation of Rwanda (FDLR) in North and South Kivu—conflict levels seem similar for gold-suitable and gold-unsuitable localities in each year.[31] There is no indication of a differential change in conflict incidence after Dodd-Frank enactment in 2011.

Next, we estimate Eq. (8) using the inverse hyperbolic sine of fatality count in place of conflict incidence as the dependent variable.[32] Fatalities likely have less coverage bias, as they represent severe events that are difficult to hide from the public eye. Figure 5 Panel B shows that, compared to conflict incidence, the coefficients for fatalities are less noisy and provide somewhat stronger evidence

[31] In (Internet Appendix) Section IA4.3, we show that this abnormally high coefficient is indeed driven by the North and South Kivu provinces. One potential explanation is that areas suitable for mining are differentially targeted by the military operation because they have higher populations and more economic activity.
[32] In (Internet Appendix) Section IA4.2, we also show that using the count of conflict occurrences as the dependent variable yields similar results.

that conflict levels do not change differentially for gold-suitable and gold-unsuitable localities around Dodd-Frank enactment.

In Figure 5 Panel C, we estimate the average treatment effect at Dodd-Frank enactment (2011) and several sensitivity tests. Specifically, we estimate Eq. (8) replacing the individual *Year* indicators with a single *Post2011* indicator, which equals one for 2011 and all subsequent years. Corresponding to the lack of differential change in conflict incidence shown in Panel A, the baseline ATE is statistically insignificant and close to zero. Next, we explore the sensitivity of the average treatment effect to various sample restrictions, fixed-effect structures, alternative treatment variables, and standard error clusters that address several empirical concerns. First, we include only the provinces of Nord- and Sud-Kivu, where most conflicts and mining are concentrated. Second, we include zero-conflict localities to capture all populated areas rather than only those in conflict zones. Third, we only retain observations three years before and after 2011 to reduce noise from long-term population displacement or demographic shifts.[33] Fourth, we shift to broader fixed-effect structures by replacing *Territory×Year* with either *Year* or *Province×Year* fixed effects. Fifth, we replace *Gold Suitable* with *% Gold Suitable*, which is a continuous treatment variable between 0 and 1 measuring the fraction of the 10-kilometer radius around a locality that lies on gold-suitable bedrock. Sixth, we replace *Gold Suitable* with *Gold Mine Count*, which represents the number of known gold mines (from the IPIS database) within 10 kilometers of the locality. Seventh, we replace *Gold Suitable* with *Gold Mine Indicator*, which is an indicator variable for whether any known gold mines exist within 10 kilometers of the

[33] In (Internet Appendix) Section IA4.4, we show that the results are similar when using smaller radii (i.e., 2 kilometers and 1 kilometer) and excluding all overlapping areas. If we were to exclude all overlapping areas with a 10-kilometer radius, the remaining sample would be too small to obtain reliable estimates.

locality.[34] Eighth, we cluster standard errors at the province and village levels rather than at the territory level. A statistically insignificant and negligible coefficient is maintained for each of these sensitivity tests, giving us confidence that there is little differential change in conflict incidence for mining areas relative to non-mining areas after Dodd-Frank enactment.

The theoretical framework suggests that changes in conflict around Dodd-Frank enactment should be driven by armed group incentives to increase or decrease protection around mines. Thus, any effect that exists should be observed only for armed group conflict. In Figure 5 Panel D, we estimate Eq. (8) separately for armed group conflict and non-armed group conflict. For either type, there is no indication of a significant change in conflict incidence around Dodd-Frank enactment.

Overall, we find no evidence of a significant change in conflict incidence or fatalities around Dodd-Frank enactment. This likely indicates that armed groups' responses to the decline in mineral price and the increase in audit risk cancel out so that no net effect is observed. This finding contrasts with prior literature that largely finds an increase in conflict for gold-mining territories after Dodd-Frank enactment (e.g., Parker & Vadheim, 2017; Stoop et al., 2018; Baik et al., 2024). Our study uses within-territory estimation at the locality level, so we exploit different variation from prior studies. We strip out territory-level trends in conflict, which are driven by many contemporaneous events, to obtain a granular estimate of changes in conflict around Dodd-Frank enactment.

6. Conclusion

[34] In (Internet Appendix) Section IA4.1, we map out conflict incidence and fatalities using an indicator for known small-scale gold mines. We do not observe a salient change in conflict around Dodd-Frank enactment.

Recognizing the violence and humanitarian tragedies caused by conflict in the Eastern DRC, governments of many developed countries have enacted regulations to curb the purchase of conflict minerals that financially benefit armed group operations. These conflict-mineral regulations, such as the US Dodd-Frank Act Provision 1502, require corporations to conduct and disclose due diligence on mineral sourcing. In response to the need for supply-chain tracing, the ICGLR implemented a small-scale mining certification scheme in 2011, shortly after the passage of Dodd-Frank. We provide evidence on the impact of small-scale mine certifications on conflict dynamics in the Eastern DRC.

Within three years after the initial certification visit, the probability of conflict (count of fatalities) within 10 kilometers of certified gold mines decreases by 48% (43%), and this effect is only observed for armed group-initiated conflict. Consistent with anecdotal evidence on the lack of overall improvement, conflict is not permanently mitigated but only displaced further away from certified mines. If anything, aggregate territory-level conflict intensity is positively associated with gold mine certification frequency. These findings are consistent with certifications having a substitution effect but not an income effect on armed group activity.

Our results suggest that supply chain certification systems can meaningfully influence conflict dynamics by altering armed group incentives. However, we also provide a cautionary tale that local improvements are likely to be accompanied by the displacement of conflict to nearby uncertified areas, which may not contribute to the resolution of complex geopolitical challenges such as the humanitarian crisis in the DRC.

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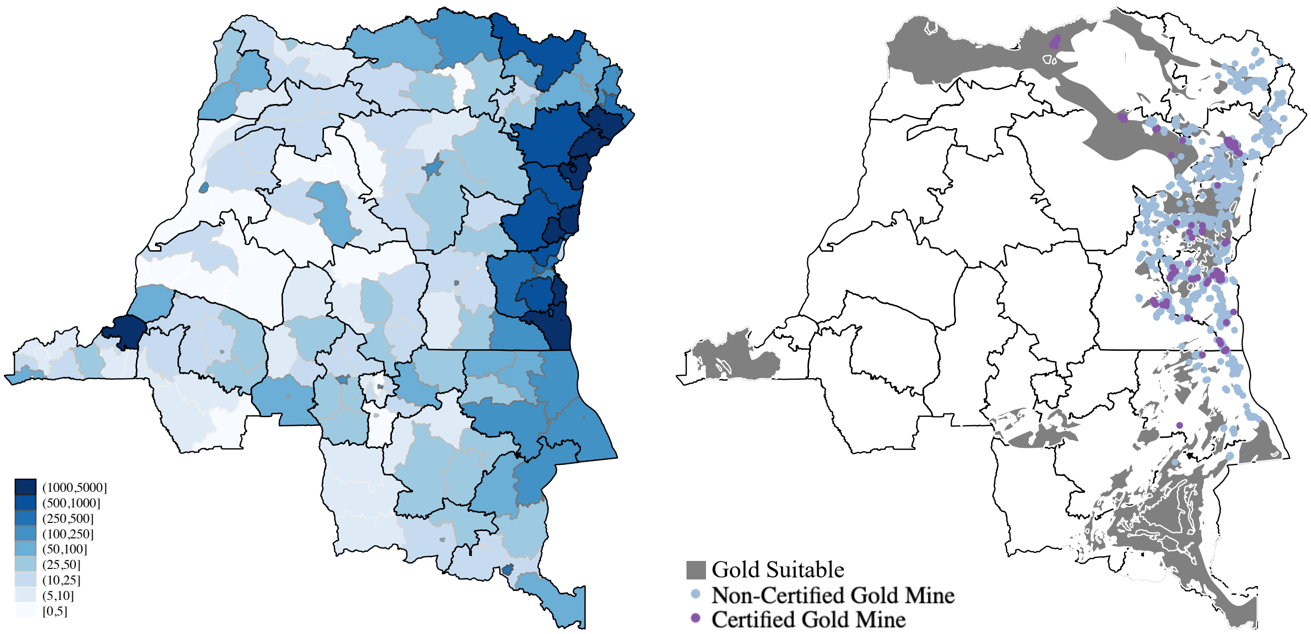
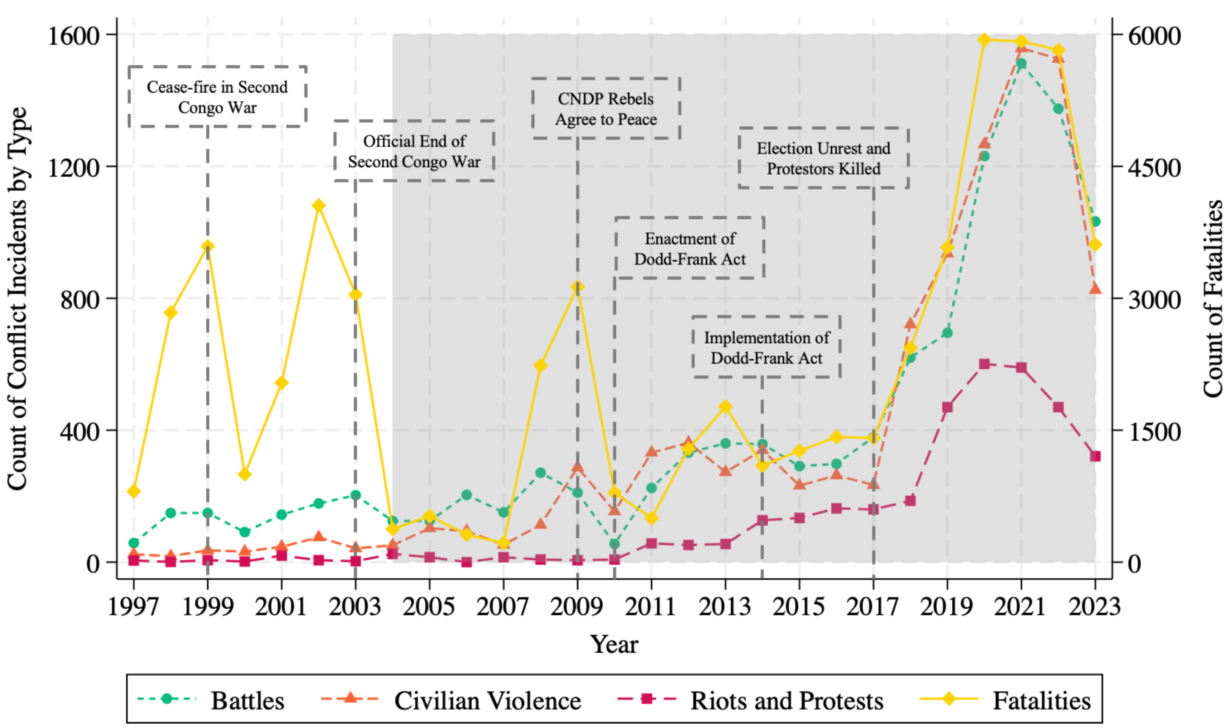
Figure 1. Conflicts and Small-Scale Mines

Panel A: Conflicts by Territory **Panel B: Gold Suitability and Gold Mines**



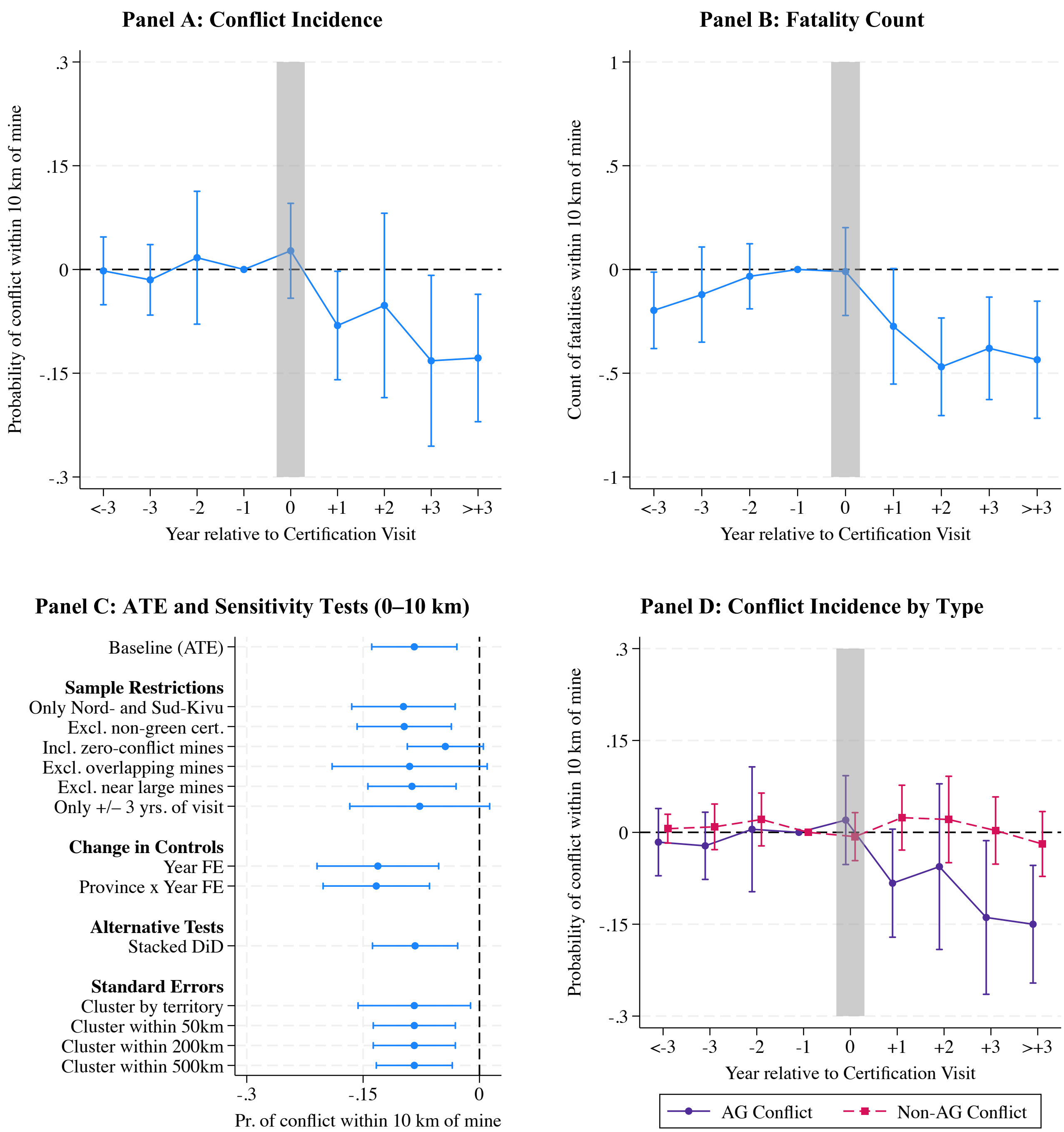
Notes: Panel A is a heat map showing conflict incidents by territory in our sample period (2004–2023). Panel B shows gold suitability (Girard et al., 2022) and small-scale mine locations by certification status.

Figure 2. Conflicts by Type and Fatalities



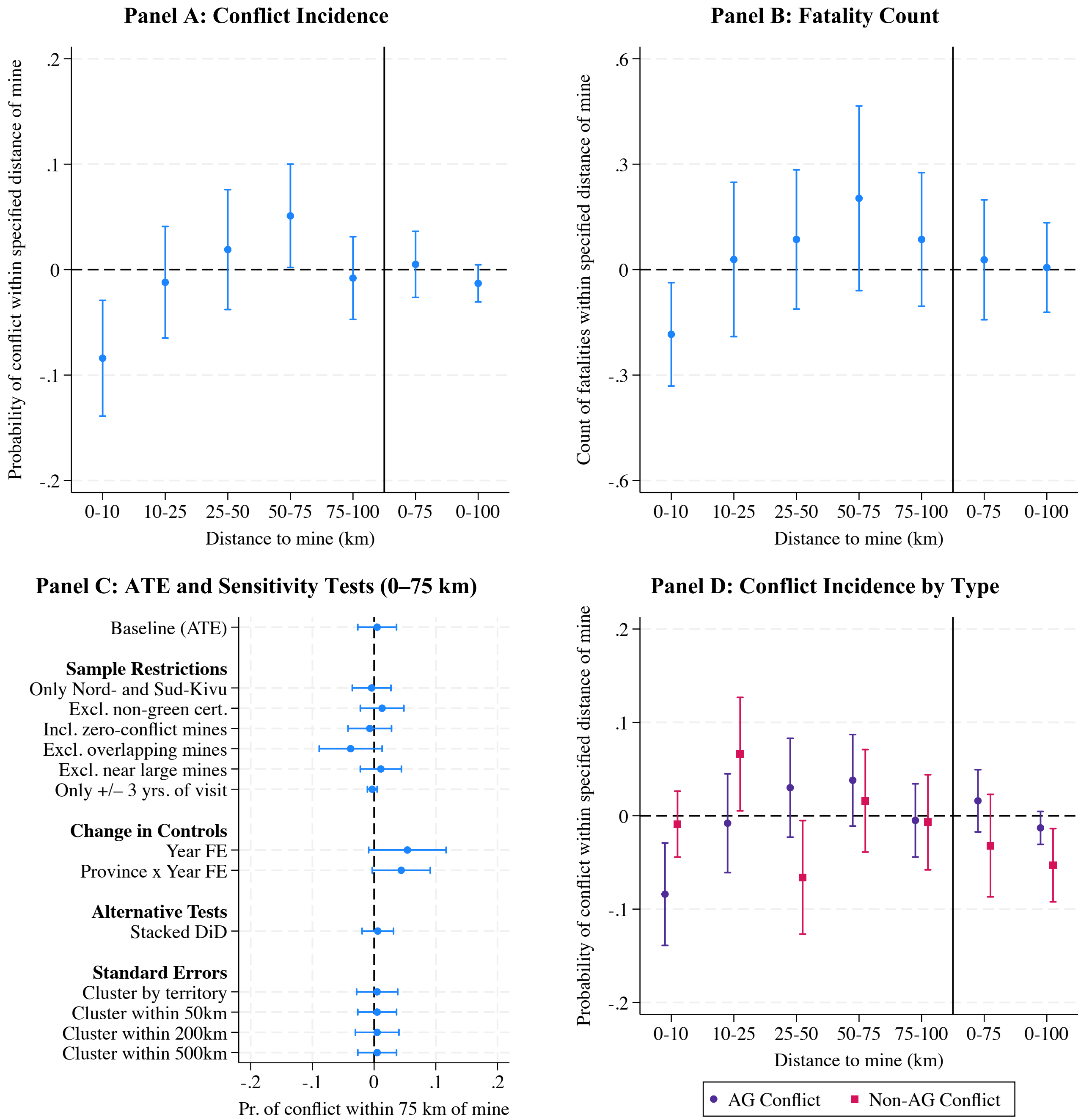
Notes: This figure illustrates the evolution of conflict across time in the DRC. Periodic conflict-related events (aligned to the year of occurrence) are superimposed onto the graph for ease of interpretation. Our sample period is shaded in gray.

Figure 3. Change in Local Conflict Around Initial Certification Visit



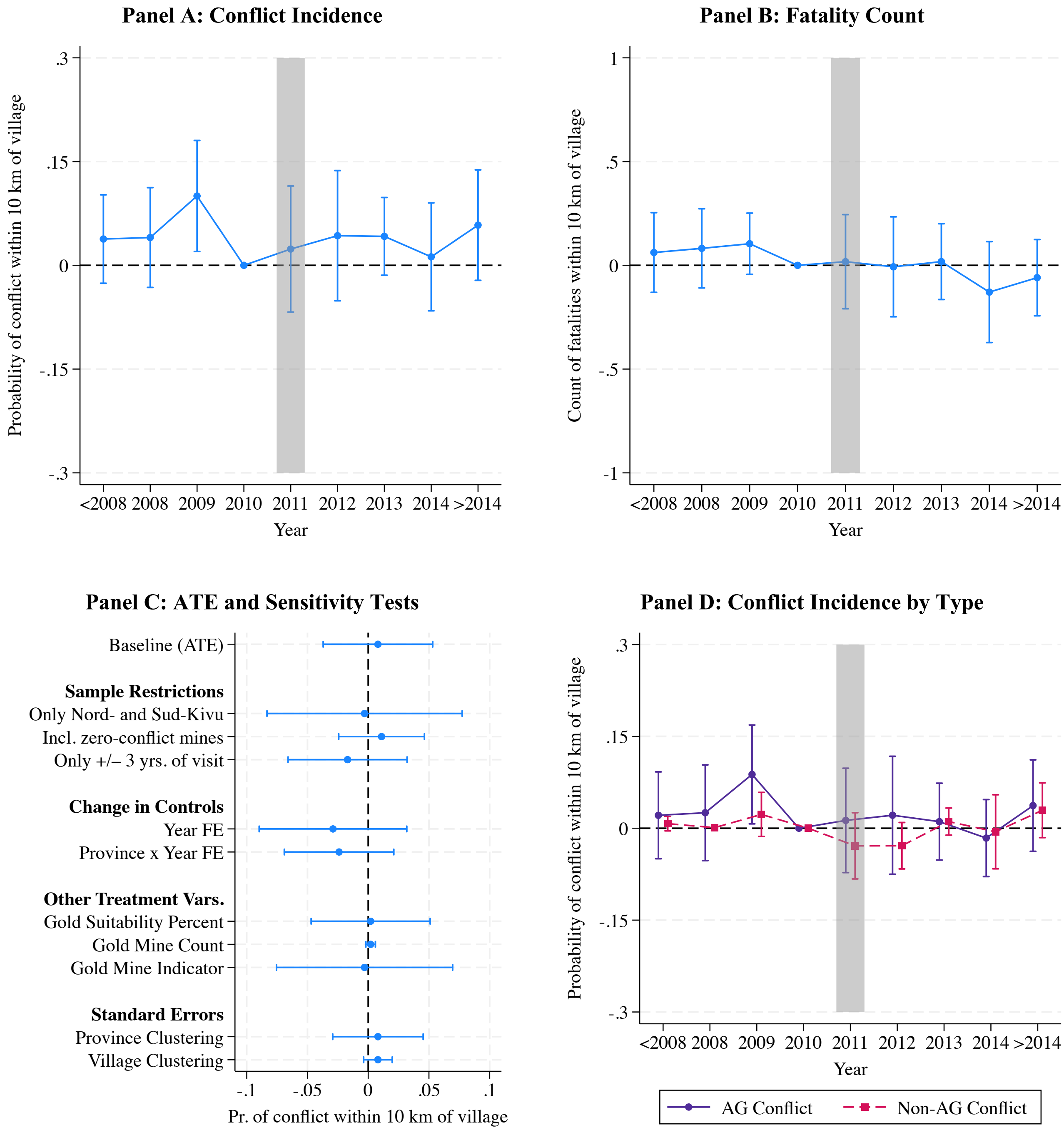
Notes: This figure shows the results of our proximal analysis around initial gold mine certification. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of initial mine certification on the probability of conflict within 10 kilometers of certified relative to uncertified mines. Panel B shows the effect of initial mine certification on fatality count rather than conflict probability. Panel C shows the average treatment effect at the initial certification year in addition to several alternative specifications. Panel D separately estimates the effect of initial mine certification on the probability of armed group (AG) conflict (i.e., battles, explosions, violence against civilians, and looting) and non-armed group (non-AG) conflict (i.e., riots and protests against government elements). We describe the sample selection in Section IA1.2 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. The baseline sample is a balanced panel of mines from 2004 to 2023 containing 29,580 observations. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

Figure 4. Displacement of Conflict Around Initial Certification Visit



Notes: This figure shows the results of our displacement analysis around initial gold mine certification. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the average treatment effect of initial mine certification on the probability of conflict within rings of various radii. Panel B shows the displacement of fatalities rather than conflict incidence. Panel C shows the average treatment effect within 75 kilometers in addition to several alternative specifications. Panel D separately estimates the effect of initial mine certification on the probability of armed group (AG) conflict (i.e., battles, explosions, violence against civilians, and looting) and non-armed group (non-AG) conflict (i.e., riots and protests against government elements). We describe the sample selection in Section IA1.2 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. The baseline sample is a balanced panel of mines from 2004 to 2023 containing 29,580 observations. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

Figure 5. Change in Conflict Around Dodd-Frank Enactment



Notes: This figure shows the results of our intent-to-treat analysis around Dodd-Frank enactment. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of Dodd-Frank enactment on the probability of conflict within 10 kilometers of localities in gold-suitable areas relative to gold-unsuitable areas. Panel B shows the effect of Dodd-Frank enactment on fatality count rather than conflict probability. Panel C shows the average treatment effect at the year 2011 in addition to several alternative specifications. Panel D separately estimates the effect of Dodd-Frank enactment on the probability of armed group (AG) conflict (i.e., battles, explosions, violence against civilians, and looting) and non-armed group (non-AG) conflict (i.e., riots and protests against government elements). We describe the sample selection in Section IA1.3 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. The baseline sample is a balanced panel of localities from 2004 to 2023 containing 118,300 observations. Standard errors are clustered at the territory level unless otherwise specified.

Table 1. Determinants of Certification Selection

Dep. Var.: 𝟙(*Cert*)	Step 1: Province Selection	Step 2: Within-Province Mine Selection		
	(1)	(2)	(3)	(4)
asinh($Conflict_{t-1}$)	0.100**	0.008	0.010***	0.009**
	(0.036)	(0.005)	(0.002)	(0.002)
asinh($Conflict_{t-1}$) – asinh($Conflict_{t-2}$)	-0.032**	-0.006	-0.006	-0.004
	(0.013)	(0.006)	(0.004)	(0.004)
asinh($Conflict_{t-2}$) – asinh($Conflict_{t-3}$)	-0.010	-0.004	-0.003	-0.003
	(0.012)	(0.005)	(0.005)	(0.005)
asinh(*Dist to Cert*$_t$)				-0.039***
				(0.006)
asinh(*Avg Lum*$_{t-1}$)				-0.010**
				(0.004)
EVI 1km$_{t-1}$				-0.080
				(0.070)
asinh(*Dist to Road*)				0.004**
				(0.001)
asinh(*Dist to Maj Road*)				-0.004***
				(0.001)
asinh(*Dist to Village*)				0.003*
				(0.001)
asinh(*Dist to Ind Mine*)				-0.004
				(0.003)
𝟙(*Protected Area*)				0.002
				(0.009)
Fixed Effects	Year	Prov x Year	Terr x Year	Terr x Year
Model	OLS	OLS	OLS	OLS
Adj. R-squared	0.195	0.111	0.214	0.258
Observations (Province-Year)	99			
Observations (Mine-Year)		7,738	7,737	7,737

Notes: This table reports coefficient estimates of OLS regressions estimating the determinants of mine certification selection. Columns (1) and (2) estimate the selection of provinces for inspection while Columns (3)–(5) estimate the within-province selection of mines for inspection. The samples are both panels from 2011 to 2019 (when the certifications occur). We describe the sample selection in Section IA1.1 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. Standard errors are clustered at the province level. asinh(*Conflict*)$_{t-1}$ is either the inverse hyperbolic sine of the count of conflict incidents within the province (province-year sample) or within 10km of the mine (mine-year sample) in the prior year. asinh(*Dist to Cert*)$_t$ is the distance to the nearest mine certified in the current year. asinh(*Avg Lum 1km*)$_{t-1}$ is the inverse hyperbolic sine of the average luminosity (measured between 0 and 63) within 1km of the population center (i.e., city, town, village, or hamlet) nearest to the mine in the prior year. *EVI 1km*$_{t-1}$ is the enhanced vegetation index within 1km of a mine in the prior year. asinh(*Dist to Road*) and asinh(*Dist to Maj Road*) are the inverse hyperbolic sines of the time-invariant distances to the nearest road and major road, respectively. asinh(*Dist to Pop*) is the inverse hyperbolic sine of the time-invariant distance to the nearest city, town, village, or hamlet. (*Protected Area*) is a time-invariant binary indicator for whether the mine is located within a protected area (e.g., national park).

Table 2. Descriptive Statistics for Certification Sample

Observations: 29,620 mine-years	Mean (1)	SD (2)
Treatment Variable:		
CFC	0.067	
Conflict Indicators:		
𝟙(*All Conflict*)	0.271	
𝟙(*AG Conflict*)	0.262	
𝟙(*Non-AG Conflict*)	0.054	
Fatality Counts:		
Count(*All Fatalities*)	3.226	15.052
Count(*AG Fatalities*)	3.170	14.921
Count(*Non-AG Fatalities*)	0.019	0.243

Notes: This table presents descriptive statistics for conflict incidence and fatalities within 10 kilometers of gold mines for the certification sample. The sample is a balanced panel from 2004 to 2023. *CFC* is an indicator equal to one if a mine is selected for certification. 𝟙(*All Conflicts*), 𝟙(*AG Conflict*), and 𝟙(*Non-AG Conflict*) are binary indicators equal to one if any conflicts, armed group conflicts (i.e., battles, explosions, violence against civilians, and looting), or non-armed group conflicts (i.e., riots and protests), respectively, are documented within 10 kilometers of a mine. Count(*All Fatalities*), Count(*AG Fatalities*), and Count(*Non-AG Fatalities*) are count variables for the number of fatalities from all conflicts, armed group conflicts, or non-armed group conflicts, respectively, documented within 10 kilometers of a mine.

Table 3. Certifications and Territory-Level Conflict Intensity

	asinh(*All Conflict*)		asinh(*AG Conflict*)		asinh(*All Fatalities*)	
	(1)	(2)	(3)	(4)	(5)	(6)
asinh(*Gold Cert Count*)	0.053**	0.016	0.063***	0.024	0.048	-0.000
	(0.021)	(0.022)	(0.023)	(0.022)	(0.030)	(0.023)
Territory FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No
Province x Year FE	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.696	0.740	0.684	0.734	0.550	0.637
Observations (Territory-Year)	1,700	1,700	1,700	1,700	1,700	1,700

Notes: This table reports coefficient estimates of OLS regressions estimating the aggregate effect of small-scale gold mine certification visits on territory-level conflict incidence and fatalities. The sample is a balanced panel from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are reported in parentheses. asinh(*All Conflict*), asinh(*AG Conflict*), and asinh(*All Fatalities)* are the inverse hyperbolic sines of the count of all conflicts, armed group conflicts (i.e., battles, explosions, violence against civilians, and looting), and all fatalities within a territory in a given year. asinh(*Gold Cert Count*) is the inverse hyperbolic sine of the count of certified gold mines within a territory.

Table 4. Descriptive Statistics for ITT Sample

Observations: 118,440 locality-years	Mean (1)	SD (2)
Treatment Variable:		
Gold Suitable	0.510	
Conflict Indicators:		
𝟙(*All Conflict*)	0.329	
𝟙(*AG Conflict*)	0.312	
𝟙(*Non-AG Conflict*)	0.098	
Fatality Counts:		
Count(*All Fatalities*)	5.847	25.695
Count(*Battle Fatalities*)	5.670	25.320
Count(*Non-AG Fatalities*)	0.069	0.709

Notes: This table presents descriptive statistics for conflict incidence and fatalities within 10 kilometers of gold mines for the ITT sample. The sample is a balanced panel from 2004 to 2023. *Gold Suitable* is an indicator equal to one if a locality has any gold-suitable bedrock within 10 kilometers (Girard et al., 2022). 𝟙(*All Conflicts*), 𝟙(*AG Conflict*), and 𝟙(*Non-AG Conflict*) are binary indicators equal to one if any conflicts, armed group conflicts (i.e., battles, explosions, violence against civilians, and looting), or non-armed group conflicts (i.e., riots and protests), respectively, are documented within 10 kilometers of a mine. Count(*All Fatalities*), Count(*AG Fatalities*), and Count(*Non-AG Fatalities*) are count variables for the number of fatalities from all conflicts, armed group conflicts, or non-armed group conflicts, respectively, documented within 10 kilometers of a mine.

Online Appendix to

Can Audits Shift the Battleground? Supply Chain Certifications and Conflict Dynamics in the Congo

SAMUEL CHANG and HANS B. CHRISTENSEN

September 2024

Contents

Section IA1. Sample Selection and Composition

IA1.1 Determinants Sample

This section provides additional details on sample selection and composition for the determinants sample. Table IA1.1a delineates the overall sample selection process for each portion of data cleaning. Table IA1.1b outlines the number of gold mines in targeted provinces available for selection each year as well as the number and percent selected. Table IA1.1c provides a detailed listing of the count of gold mine certifications by province and year, which make up our treatment sample. Table IA1.1d provides additional descriptive statistics to complement Table 1 in the paper. These should be helpful for detailed data concerns.

Table IA1.1a. Sample Selection for Determinants Analysis (Mine-Year Sample)

Selection Criterion		Obs. Count
IPIS DRC Mine Database		6,375
Less:	Mines certified after 2019	(238)
Less:	Duplicate visits (cert. or not)	(2,847)
Less:	Non-gold mines	(1,176)
Times:	9-year panel	x 9
Less:	Certified mines after certification	(365)
Less:	Located in uncertified province	(10,923)
Final mine sample		7,738

Notes: This table presents the sample selection process for the determinants sample by step. The sample is from 2011 to 2019.

Table IA1.1b. Certification Selection by Year

Year	# Certified	# Not Certified	# in Certified Province	% Certified
2011	14	1058	1072	1.31%
2012	0	0	0	0.00%
2013	5	331	336	1.49%
2014	2	1056	1058	0.19%
2015	19	564	583	3.26%
2016	20	1268	1288	1.55%
2017	10	764	774	1.29%
2018	57	1551	1608	3.54%
2019	2	1017	1019	0.20%
Total	129	7609	7738	1.67%

Notes: This table shows the annual number of gold mine certification visits in the Eastern DRC from 2011 to 2019 separated by certification status.

Table IA1.1c. Gold Mine Certifications by Province and Year

Year	Bas-Uele	Haut-Katanga	Haut-Lomami	Haut-Uele	Ituri	Lualaba	Maniema	Nord-Kivu	Sud-Kivu	Tanganyika	Tshopo	**Total**
2011	0	0	0	0	0	0	0	1	13	0	0	**14**
2012	0	0	0	0	0	0	0	0	0	0	0	**0**
2013	0	0	0	0	0	0	3	0	0	2	0	**5**
2014	0	0	0	0	0	0	0	1	1	0	0	**2**
2015	12	0	0	0	6	0	0	0	0	0	1	**19**
2016	0	0	0	0	0	0	10	2	8	0	0	**20**
2017	0	0	0	0	0	0	8	0	2	0	0	**10**
2018	0	0	0	0	22	0	0	6	19	0	10	**57**
2019	0	0	0	0	0	0	0	1	1	0	0	**2**
Total	**12**	**0**	**0**	**0**	**28**	**0**	**21**	**11**	**44**	**2**	**11**	**129**

Notes: This table shows the annual number of initial gold mine certification visits in the Eastern DRC from 2011 to 2019 separated by province. We observe a total of 129 initial certification visits from the IPIS database.

Table IA1.1d. Descriptive Statistics for Determinants Sample

	Obs	Mean	SD	P1	P25	P50	P75	P99
𝟙(*Treat*)	7,738	0.017	0.128	0.000	0.000	0.000	0.000	1.000
asinh(*Conflict*$_{t-1}$)	7,738	0.629	2.509	0.000	0.000	0.000	0.000	12.000
asinh(*Conflict*$_{t-1}$) – asinh(*Conflict*$_{t-2}$)	7,738	0.105	1.726	-4.000	0.000	0.000	0.000	6.000
asinh(*Conflict*$_{t-2}$) – asinh(*Conflict*$_{t-3}$)	7,738	-0.037	2.164	-6.000	0.000	0.000	0.000	5.000
asinh(*Dist to Cert*$_t$)	7,738	86.385	64.633	0.747	34.739	71.107	127.067	245.266
asinh(*Avg Lum*$_{t-1}$)	7,738	0.072	0.656	0.000	0.000	0.000	0.000	3.000
EVI 1km$_{t-1}$	7,738	0.472	0.055	0.332	0.439	0.475	0.510	0.582
asinh(*Dist to Road*)	7,738	4.026	5.777	0.003	0.167	1.363	5.889	27.609
asinh(*Dist to Maj Road*)	7,738	6.143	8.223	0.004	0.295	2.267	8.862	32.476
asinh(*Dist to Village*)	7,738	9.641	13.448	0.058	1.805	4.723	12.058	75.012
asinh(*Dist to Ind Mine*)	7,738	181.293	93.891	10.283	98.372	178.204	251.984	360.337
𝟙(*Protected Area*)	7,738	0.035	0.184	0.000	0.000	0.000	0.000	1.000

Notes: This table presents a selection model to descriptively compare characteristics of mines selected and not selected for certification. This mine-year sample is composed of all untreated mines as of the beginning of each year, so the sample size decreases as more mines are treated over time. The sample is an unbalanced panel from 2011 to 2019, which are the possible treatment years. Panel A presents descriptive statistics for the determinants sample. 𝟙(*Treat*) is a binary indicator for whether the mine is chosen for certification in that year. asinh(*Conflict*)$_{t-1}$ is either the inverse hyperbolic sine of the count of conflict incidents within the province (province-year sample) or within 10km of the mine (mine-year sample) in the prior year. asinh(*Dist to Cert*)$_t$ is the distance to the nearest mine certified in the current year. asinh(*Avg Lum 1km*)$_{t-1}$ is the inverse hyperbolic sine of the average luminosity (measured between 0 and 63) within 1km of the population center (i.e., city, town, village, or hamlet) nearest to the mine in the prior year. *EVI 1km*$_{t-1}$ is the enhanced vegetation index within 1km of a mine in the prior year. asinh(*Dist to Road*) and asinh(*Dist to Maj Road*) are the inverse hyperbolic sines of the time-invariant distances to the nearest road and major road, respectively. asinh(*Dist to Pop*) is the inverse hyperbolic sine of the time-invariant distance to the nearest city, town, village, or hamlet. 𝟙(*Protected Area*) is a time-invariant binary indicator for whether the mine is located within a protected area (e.g., national park).

IA1.2 Certification Sample

This section provides additional details on sample selection and composition for the certification sample. Table IA1.2a delineates the overall sample selection process for each portion of data cleaning. Table IA1.2b provides additional descriptive statistics to complement Table 2 in the paper. These should be helpful for detailed data concerns.

Table IA1.2a. Sample Selection for Certification Analysis (Mine-Year Sample)

Panel A: Conflict Data

Selection Criterion		Obs. Count
ACLED DRC Conflict Database		31,505
Less:	Geographically imprecise	(599)
Less:	Non-conflict events	(2,518)
Less:	Non-eastern provinces	(3,270)
Less:	Before 2004 or after 2023	(2,080)
Final conflict sample		23,038

Panel B: Final Mine Sample

Selection Criterion		Obs. Count
IPIS DRC Mine Database		6,375
Less:	Mines certified after 2019	(238)
Less:	Duplicate visits (cert. or not)	(2,847)
Less:	Non-gold mines	(1,176)
Less:	Non-conflict mines	(633)
Times:	20-year panel	x 20
Final mine sample		29,620

Notes: This table describes the sample selection process for the certification sample by step. The sample is from 2004 to 2023. The observation counts in the analyses do not exactly match the numbers in this table because we drop singleton observations for various fixed effect structures. Panel A describes selection for the ACLED DRC conflict database. Panel B describes selection for the IPIS DRC Mine Database.

Table IA1.2b. Additional Descriptive Statistics for Certification Sample

Obs. Count = 29,620	0-10 km		10-25 km		25-50 km		50-75 km		75-100 km	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Conflict Indicators										
$\mathbb{1}$(*All Conflict*)	0.271		0.505		0.722		0.816		0.890	
$\mathbb{1}$(*AG Conflict*)	0.262		0.494		0.708		0.806		0.882	
$\mathbb{1}$(*Non-AG Conflict*)	0.066		0.190		0.351		0.418		0.511	
Conflict Counts										
Count(*All Conflict*)	1.693	5.692	7.331	18.193	25.106	55.566	34.070	64.431	48.685	82.115
Count(*AG Conflict*)	1.518	5.171	6.579	16.506	21.493	47.657	30.291	56.924	42.773	71.575
Count(*Non-AG Conflict*)	0.175	1.125	0.751	2.754	3.613	11.550	3.778	12.026	5.912	14.054
Fatality Counts										
Count(*All Fatalities*)	3.226	15.052	15.286	48.194	52.746	137.338	73.611	163.591	102.485	193.768
Count(*AG Fatalities*)	3.170	14.921	15.037	47.794	51.584	135.358	72.283	161.507	100.375	190.782
Count(*Non-AG Fatalities*)	0.056	0.539	0.250	1.273	1.162	4.617	1.329	4.962	2.110	6.117

Notes: This table presents more detailed descriptive statistics for the certification sample, which is a balanced panel of mine- and village-year observations from 2004 to 2023. This is an extension of Table 2. $\mathbb{1}$(*All Conflicts*), $\mathbb{1}$(*AG Conflict*), and $\mathbb{1}$(*Non-AG Conflict*) are binary indicators equal to one if any conflicts, armed group conflicts (i.e., battles, explosions, violence against civilians, and looting), or non-armed group conflicts (i.e., riots and protests), respectively, are documented within 10 kilometers of a mine. Count(*All Conflict*), Count(*AG Conflict*), Count(*Non-AG Conflict*) are count variables for the number of conflicts from all conflicts, armed group conflicts, or non-armed group conflicts, respectively, documented within 10 kilometers of a mine. Count(*All Fatalities*), Count(*AG Fatalities*), and Count(*Non-AG Fatalities*) are count variables for the number of fatalities from all conflicts, armed group conflicts, or non-armed group conflicts, respectively, documented within 10 kilometers of a mine.

IA1.3 ITT Sample

This section provides additional details on sample selection and composition for the ITT sample. Table IA1.3 delineates the overall sample selection process for each portion of data cleaning. These should be helpful for detailed data concerns.

Table IA1.3. Sample Selection for ITT Analysis (Locality-Year Sample)

Panel A: Conflict Data

Selection Criterion		Obs. Count
ACLED DRC Conflict Database		31,505
Less:	Geographically imprecise	(599)
Less:	Non-conflict events	(2,518)
Less:	Non-eastern provinces	(3,270)
Less:	Before 2004 or after 2023	(2,080)
Final conflict sample		23,038

Panel B: Localities Data

Selection Criterion		Obs. Count
OSM DRC Localities Database		19,901
Less:	Non-population centers	(879)
Less:	Duplicate localities	(50)
Less:	Not within any territory	(41)
Less:	Non-eastern provinces	(8,803)
Less:	Non-conflict localities	(4,206)
Times:	20-year panel	x 20
Final mine sample		118,440

Notes: This table describes the sample selection process for the ITT sample by step. The sample is from 2004 to 2023. The observation counts in the analyses do not exactly match the numbers in this table because we drop singleton observations for various fixed effect structures. Panel A describes selection for the ACLED conflict database. Panel B describes selection for the OSM DRC localities database.

Section IA2. Certification Analysis for 3T Mines

IA2.1 Sample Selection and Descriptive Statistics

While there were prior initiatives that certified artisanal 3T mines (e.g., CFSI, CTC, and iTSCi), the ICGLR certifications also certified many 3T mines. We have no information on whether these mines were previously certified by other systems. Thus, we conduct an analysis to assess whether 3T mines also experienced a change in conflict after the initial ICGLR certification visit. Table IA1.2a Panel A shows the sample selection process for 3T mines. Descriptively, we can see in Table IA1.2a Panel B that 30.4% of 3T mine-years have conflict within 10 kilometers, and there is an average of 2.4 fatalities within 10 kilometers of a mine in a year. Table IA1.2b provides a detailed listing of the count of 3T mine certifications by province and year, which make up our treatment sample. In Sections IA2.2 and IA2.3, we conduct corresponding analyses with 3T mines.

Table IA1.2a. Sample Selection for Certification Analysis (3T Mines)

Panel A: Sample Selection

Selection Criterion		Obs. Count
IPIS DRC Mine Database		6,375
Less:	Mines certified after 2019	(238)
Less:	Duplicate visits (cert. or not)	(2,847)
Less:	Non-3T mines	(2,319)
Less:	Non-conflict mines	(385)
Times:	20-year panel	x 20
Final mine sample		11,720

Panel B: Descriptive Statistics

	Certification Sample	
	Mean	SD
	(1)	(2)
Treatment Variables		
CFC	0.435	
Conflict Indicators		
𝟙(*All Conflict*)	0.304	
𝟙(*AG Conflict*)	0.283	
𝟙(*Non-AG Conflict*)	0.080	
Fatality Counts		
Count(*All Fatalities*)	2.441	9.950
Count(*Battle Fatalities*)	2.364	9.880
Count(*Non-AG Fatalities*)	0.078	0.887
Observations (count)	11,720 mine-years	

Notes: This table describes the sample selection process for the 3T mines by step. The sample is from 2004 to 2023. The observation counts in the analyses do not exactly match the numbers in this table because we drop singleton observations for various fixed effect structures. Panel A describes selection for the IPIS DRC Mine Database. Panel B presents descriptive statistics for conflict incidence and fatalities within 10 kilometers. *CFC* is an indicator equal to one if a mine is selected for certification. 𝟙(*All Conflicts*), 𝟙(*AG Conflict*), and 𝟙(*Non-AG Conflict*) are binary indicators equal to one if any conflicts, armed group conflicts (i.e., battles, explosions, violence against civilians, and looting), or non-armed group conflicts (i.e., riots and protests), respectively, are documented within 10 kilometers of a mine. Count(*All Fatalities*), Count(*AG Fatalities*), and Count(*Non-AG Fatalities*) are count variables for the number of fatalities from all conflicts, armed group conflicts, or non-armed group conflicts, respectively, documented within 10 kilometers of a mine.

Table IA2.1b. 3T Mine Certifications by Province and Year

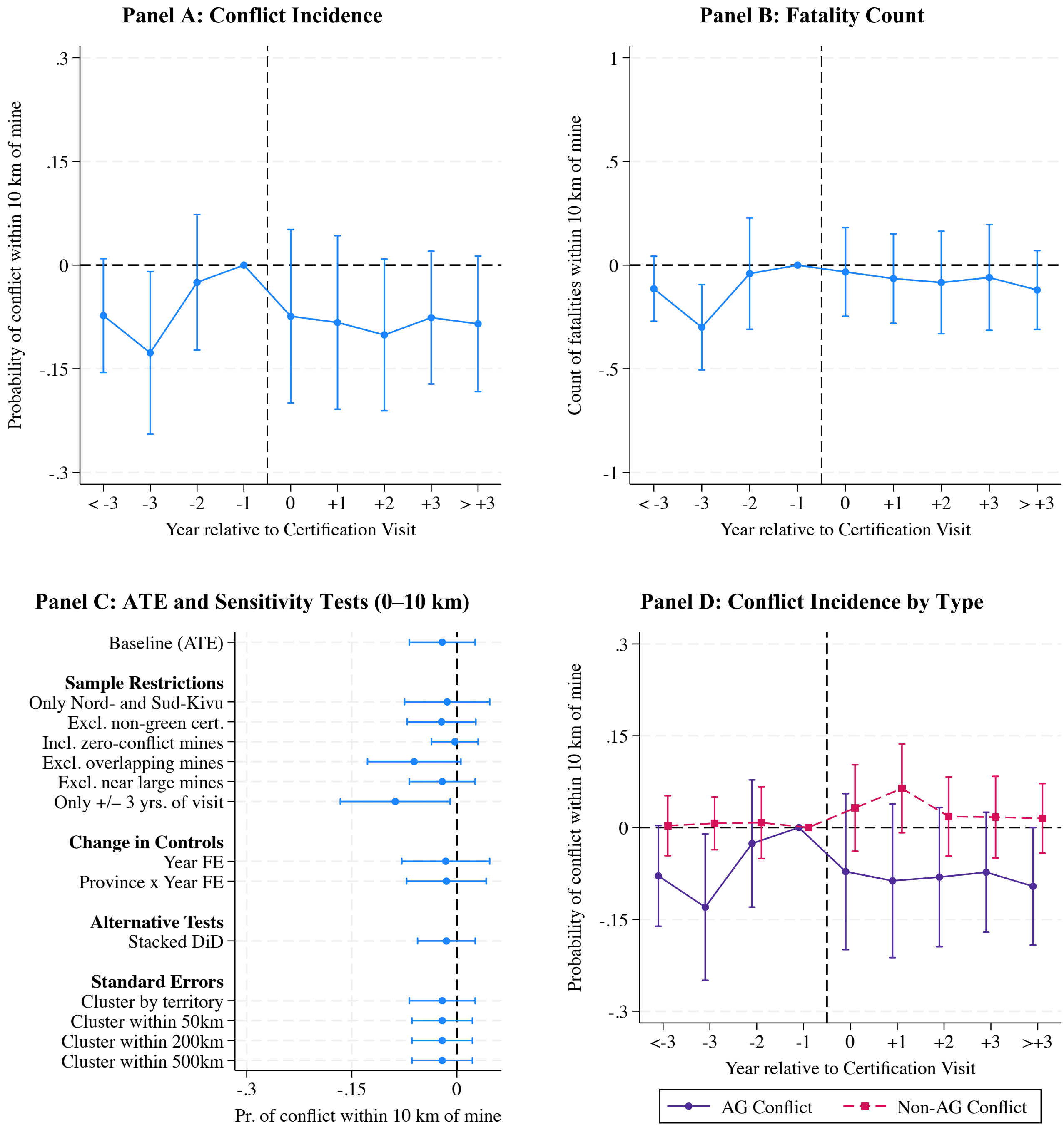
Year	Bas-Uele	Haut-Katanga	Haut-Lomami	Haut-Uele	Ituri	Lualaba	Maniema	Nord-Kivu	Sud-Kivu	Tanganyika	Tshopo	**Total**
2011	0	0	0	0	0	0	0	13	14	0	0	**27**
2012	0	0	0	0	0	0	26	0	5	0	0	**31**
2013	0	2	16	0	0	0	14	0	2	35	0	**69**
2014	0	7	11	0	0	8	0	16	8	5	0	**55**
2015	0	6	2	0	0	0	17	21	7	3	0	**56**
2016	0	0	0	0	0	0	5	13	5	0	0	**23**
2017	0	0	1	0	0	0	11	10	28	35	0	**85**
2018	0	0	0	0	0	0	0	12	19	0	0	**31**
2019	0	0	7	0	0	0	1	3	13	2	0	**26**
Total	**0**	**15**	**37**	**0**	**0**	**8**	**74**	**88**	**101**	**80**	**0**	**403**

Notes: This table shows the annual number of initial 3T mine certification visits in the Eastern DRC from 2011 to 2019 separated by province. We observe a total of 403 initial certification visits from the IPIS database.

IA2.2 Conflict Around 3T Mines

Corresponding to Section 5.2 in the paper, we examine changes in conflict around the first certification visit for certified 3T mines. In Figure IA2.2 Panels A and B, we find a weaker but similar reduction in conflict probability and fatalities after certification for 3T mines. However, fulfillment of the parallel trends assumption is much less clear, as a clear positive pre-treatment trend is observed. In Figure IA2.2 Panel C, we see a negative but insignificant average effect that remains relatively stable across several sensitivity tests. Finally, Figure IA2.2 Panel D also shows a decrease after certification for armed group conflicts but not non-armed group conflicts.

Figure IA2.2. Change in Conflict Around Initial Certification Visit (3T Mines)

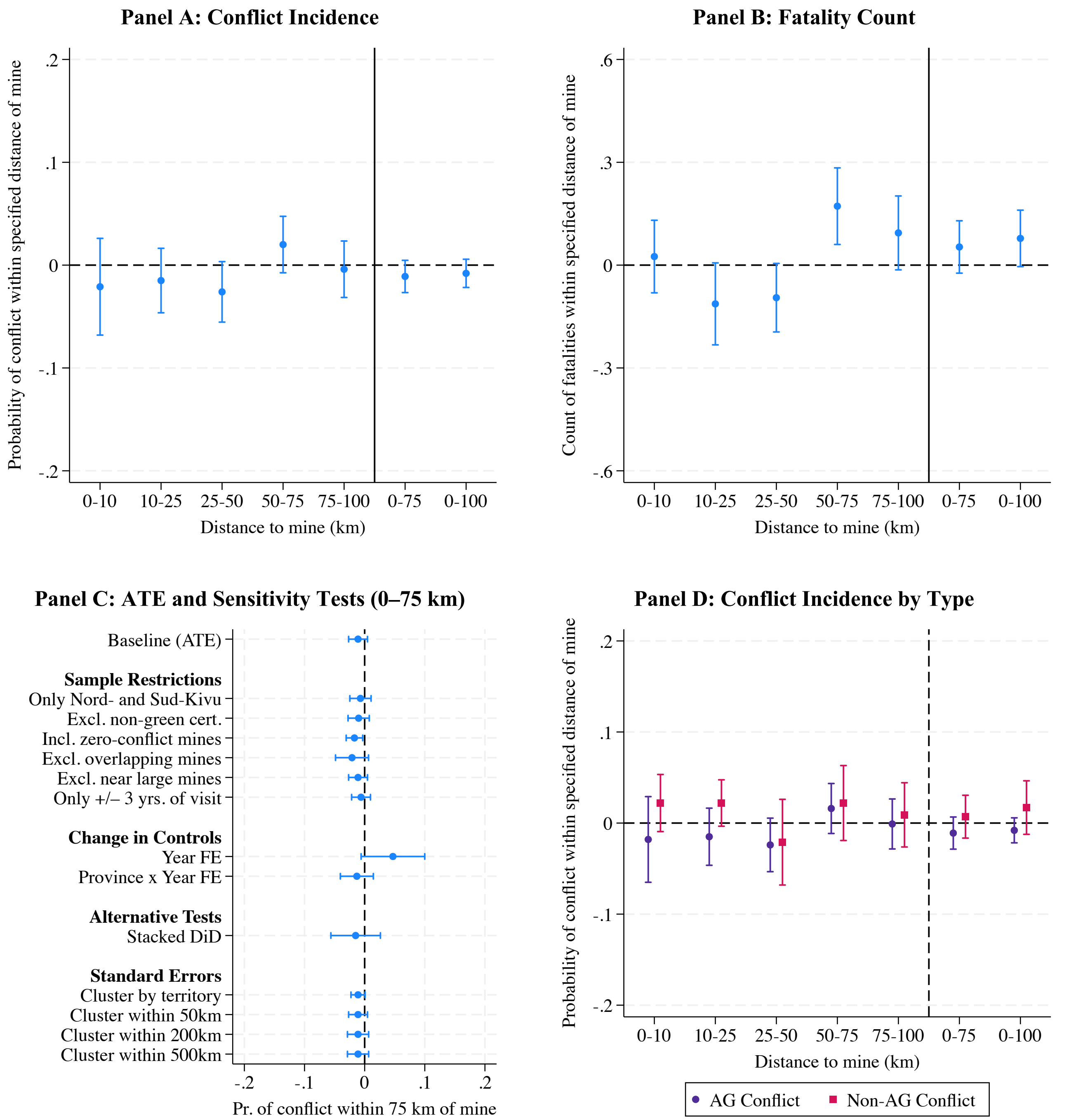


Notes: This figure shows the results of our proximal analysis around initial certification visits for 3T mines. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of initial mine certification on the probability of conflict within 10 kilometers of certified relative to uncertified mines. Panel B shows the effect of initial mine certification on fatality count rather than conflict probability. Panel C shows the average treatment effect at the initial certification year in addition to several alternative specifications. Panel D separately estimates the effect of initial mine certification on the probability of armed group (AG) conflict (i.e., battles, explosions, violence against civilians, and looting) and non-armed group (non-AG) conflict (i.e., riots and protests). We describe the sample selection in Section IA4 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. The baseline sample is a balanced panel of mines from 2004 to 2023 containing 11,720 observations. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

IA2.3 Geographic Displacement and Aggregate Conflict

Corresponding to Section 5.3 in the paper, we examine conflict displacement around the first certification visit for certified 3T mines. In Figure IA2.3 Panels A and B, we find a weaker but similar displacement in conflict probability and fatalities from areas near the mine to areas further away after certification. In Figure IA2.3 Panel C, we again see an insignificant average effect within 75 kilometers that remains relatively stable across several sensitivity tests. Finally, Figure IA2.3 Panel D indicates displacement after certification for armed group conflicts but not non-armed group conflicts.

Figure IA2.3. Displacement of Conflicts After Initial Certification Visit (3T Mines)



Notes: This figure shows the results of our displacement analysis around initial certification visits for 3T mines. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the average treatment effect of initial mine certification on the probability of conflict within rings of various radii. Panel B shows the displacement of fatalities rather than conflict incidence. Panel C shows the average treatment effect within 75 kilometers in addition to several alternative specifications. Panel D separately estimates the effect of initial mine certification on the probability of armed group (AG) conflict (i.e., battles, explosions, violence against civilians, and looting) and non-armed group (non-AG) conflict (i.e., riots and protests). We describe the sample selection in Section IA4 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. The baseline sample is a balanced panel of mines from 2004 to 2023 containing 11,720 observations. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

Table IA2.3. Certifications and Territory-Level Conflict Intensity

	asinh(*All Conflicts*)		asinh(*AG Conflict*)		asinh(*All Fatalities*)	
	(1)	(2)	(3)	(4)	(5)	(6)
asinh(*3T Cert Count*)	0.035	-0.085	0.072	-0.038	0.137*	-0.024
	(0.060)	(0.053)	(0.061)	(0.053)	(0.073)	(0.061)
Territory FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No
Province x Year FE	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.692	0.741	0.679	0.734	0.551	0.637
Observations (Territory-Year)	1,700	1,700	1,700	1,700	1,700	1,700

Notes: This table reports coefficient estimates of OLS regressions estimating the aggregate effect of certification visits on territory-level conflict incidence and fatalities. The sample is a balanced panel from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are reported in parentheses. asinh(*All Conflict*), asinh(*AG Conflict*), and asinh(*All Fatalities)* are the inverse hyperbolic sines of the count of all conflicts, armed group conflicts (i.e., battles, explosions, violence against civilians, and looting), and all fatalities within a territory in a given year. asinh(*Gold Cert Count*) is the inverse hyperbolic sine of the count of certified gold mines within a territory.

Section IA3. Supplementary Tests for Certification Analysis

IA3.1 Treatment Effect by Certification Outcome

In Table IA3.1 below, we estimate differential treatment effects for each initial certification outcome. While the decrease in conflict is the most salient after green certifications, there is a similar effect around yellow and red but not blue certifications. These findings are consistent with future audit risk driving armed groups' incentives to protect certified mines.

Table IA3.1. Certification Treatment Effect by Outcome

Panel A: By Certification Status

First Cert. Year	Green	Yellow	Red	Blue (No Status)
	(1)	(2)	(3)	(4)
2011	2	7	5	0
2012	0	0	0	0
2013	4	1	0	0
2014	2	0	0	0
2015	19	0	0	0
2016	20	0	0	0
2017	10	0	0	0
2018	47	2	1	7
2019	2	0	0	0
Total	106	10	6	7

Panel B: By Certification Outcome

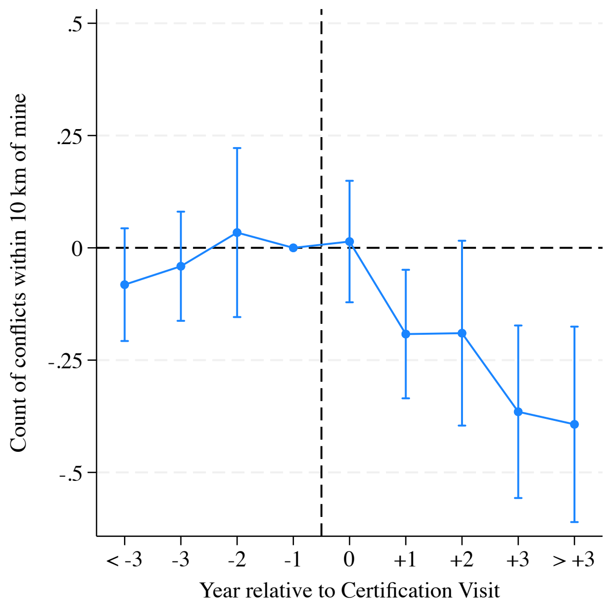
	𝟙(*All Conflict*)	𝟙(*AG Conflict*)	𝟙(*Non-AG Conflict*)	asinh(*All Fatalities*)
	(1)	(2)	(3)	(4)
CFC x *PostCert* x *Green*	-0.097***	-0.093***	-0.011	-0.201**
	(0.031)	(0.031)	(0.024)	(0.084)
CFC x *PostCert* x *Yellow*	-0.024	-0.048	0.126	-0.020
	(0.043)	(0.054)	(0.080)	(0.320)
CFC x *PostCert* x *Red*	-0.085	-0.089	-0.067*	-0.187
	(0.083)	(0.080)	(0.038)	(0.159)
CFC x *PostCert* x *Blue*	0.002	0.006	-0.078***	-0.191
	(0.116)	(0.117)	(0.019)	(0.251)
Mine FE	Yes	Yes	Yes	Yes
Territory x Year FE	Yes	Yes	Yes	Yes
R-squared	0.592	0.590	0.439	0.607
Observations (Mine-Year)	29,580	29,580	29,580	29,580

Notes: This table reports the annual number of initial certification visits and coefficient estimates of OLS regressions estimating differential treatment effects for each initial certification outcome. The sample is a balanced panel from 2004 to 2023. Panel A reports the annual number of initial mine certification visits in the Eastern DRC from 2011 to 2019 separated by certification status. Panel B presents coefficient estimates of OLS regressions estimating differential treatment effects for each initial certification outcome. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are reported in parentheses. 𝟙(*All Conflicts*), 𝟙(*AG Conflict*), and 𝟙(*Non-AG Conflict*) are binary indicators equal to one if any conflicts, armed group conflicts (i.e., battles, explosions, violence against civilians, and looting), or non-armed group conflicts (i.e., riots and protests), respectively, are documented within 10 kilometers of a mine. asinh(*All Fatalities)* is the inverse hyperbolic sines of the count of all fatalities within a territory in a given year. *CFC* is an indicator equal to one if a mine is selected for certification. *PostCert* is a binary indicator for whether the observation year is during or after the certification year for a certified mine. *Green, Yellow, Red,* and *Blue* are indicators for certification statuses.

IA3.2 Count of Conflicts as Dependent Variable

In Figure IA3.2 below, we present results that using the count of conflict occurrences as the dependent variable. Magnitudes indicate a 37% reduction in conflict count by 3 years after initial certification.

Figure IA3.2. Certification Analysis Using Conflict Count

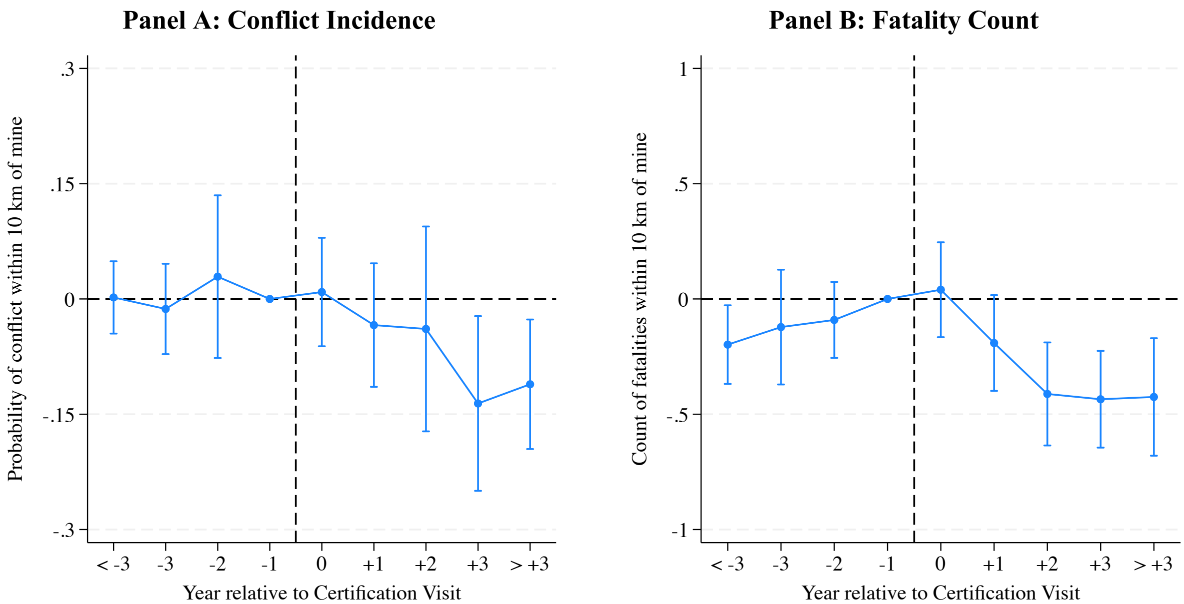


Notes: This figure shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of initial mine certification on the conflict count within 10 kilometers of certified relative to uncertified mines. The baseline sample is a balanced panel of mines from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

IA3.3 Without Half-Year Adjustment

Corresponding to Section 3.3 in the paper, we use a half-year adjustment, which pushes the certification year forward to the next year if initial certification occurs in the latter half of the year. In Figure IA3.3 below, we show that our results hold for both conflict incidence and fatalities without the half-year adjustment.

Figure IA3.3. Certification Analysis Without Half-Year Adjustment

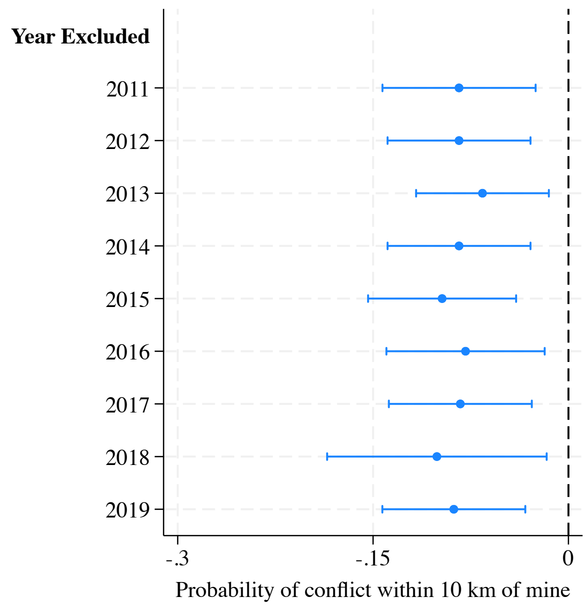


Notes: This figure presents results for both conflict incidence and fatalities without the half-year adjustment around initial certification visit. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of initial mine certification on the probability of conflict within 10 kilometers of certified relative to uncertified mines. Panel B shows the effect of initial mine certification on fatality count rather than conflict probability. We describe the sample selection in Section IA4 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. The baseline sample is a balanced panel of mines from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

IA3.4 Jackknife Excluding Certification Years

In Figure IA3.4 below, we present results dropping each year of certified mines from the sample in turn. We use the specification with only gold mines, from which we drew our main inferences. Our results remain robust and similar in magnitude across all columns, which indicates that the treatment effect is not driven by certifications in any given year.

Figure IA3.4. Jackknife Excluding Certification Years

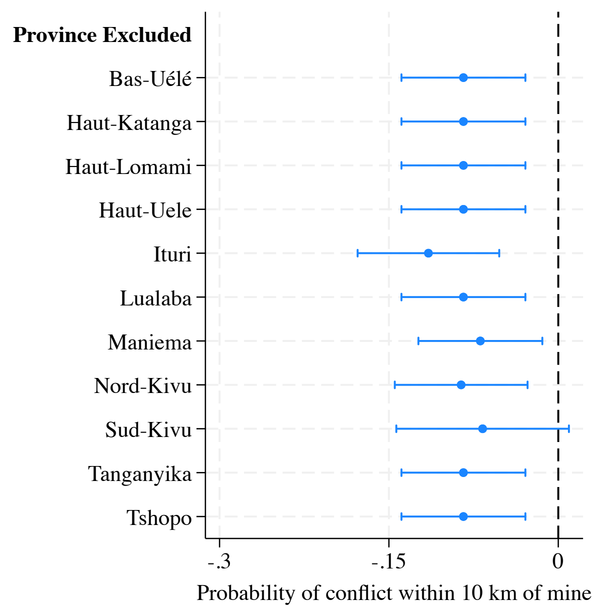


Notes: This figure reports coefficient estimates of OLS regressions estimating the effect of artisanal gold mine certification visits with "green" status on mine-level conflict incidence. We estimate the model from Panel A of Figure 4 in the paper as the baseline but separately exclude gold mines in individual years. The sample is a balanced panel from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and for infinite serial correlation are reported in parentheses.

IA3.5 Jackknife Excluding Provinces

In Figure IA3.5 below, we present results dropping each province from the sample in turn. We again use the specification with only gold mines, from which we drew our main inferences. We provide a specification excluding each province though some do not have any mines, yielding identical coefficients. Our results remain robust and similar in magnitude across all columns, which indicates that the treatment effect is not driven by any one province.

Figure IA3.5. Jackknife Excluding Provinces



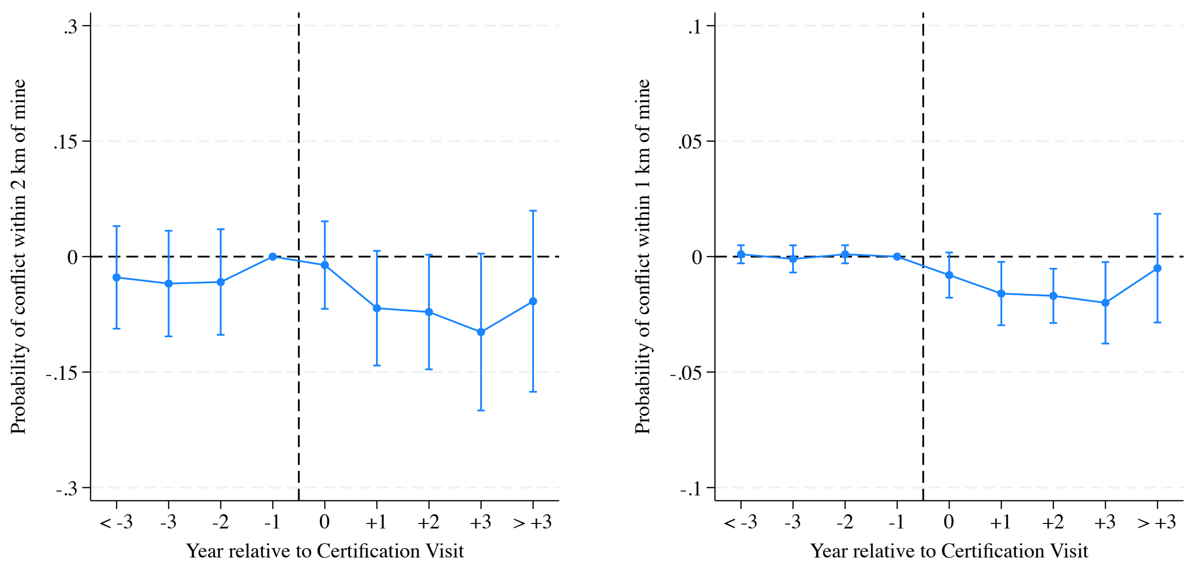
Notes: This figure reports coefficient estimates of OLS regressions estimating the effect of artisanal gold mine certification visits with "green" status on mine-level conflict incidence. We estimate the model from panel A of Figure 4 in the paper as the baseline but separately exclude gold mines in individual provinces. The sample is a balanced panel from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and for infinite serial correlation are reported in parentheses.

IA3.6 Smaller Radii and Excluding Overlapping Areas

In Figure IA3.6 below, we present results using smaller radii and completely excluding overlapping mines. Because excluding all mines within 10 kilometers of another mine would critically reduce the sample size, we were not able to do this in the main robustness figure. However, using smaller radii (2km and 1km), we are able to exclude all overlapping cells and still keep an adequate, albeit much smaller, sample. The baseline results for smaller radii (not excluding any observations) are presented in Panel A, and we exclude overlapping cells in Panel B. Our results for gold mines remain robust but are much smaller in magnitude, likely due to elimination of double-counted conflicts and reduction of the number of conflicts observed within the small radii. This indicates that gold mines experience a 4.9% (1.7%) decrease in conflict within 2 kilometers (1 kilometer) after an initial certification visit, and we are confident that this magnitude is a lower-bound.

Figure IA3.6. Certification Analysis Excluding Overlapping Areas

Panel A: Conflict Incidence within 2 km **Panel B: Conflict Incidence within 1 km**



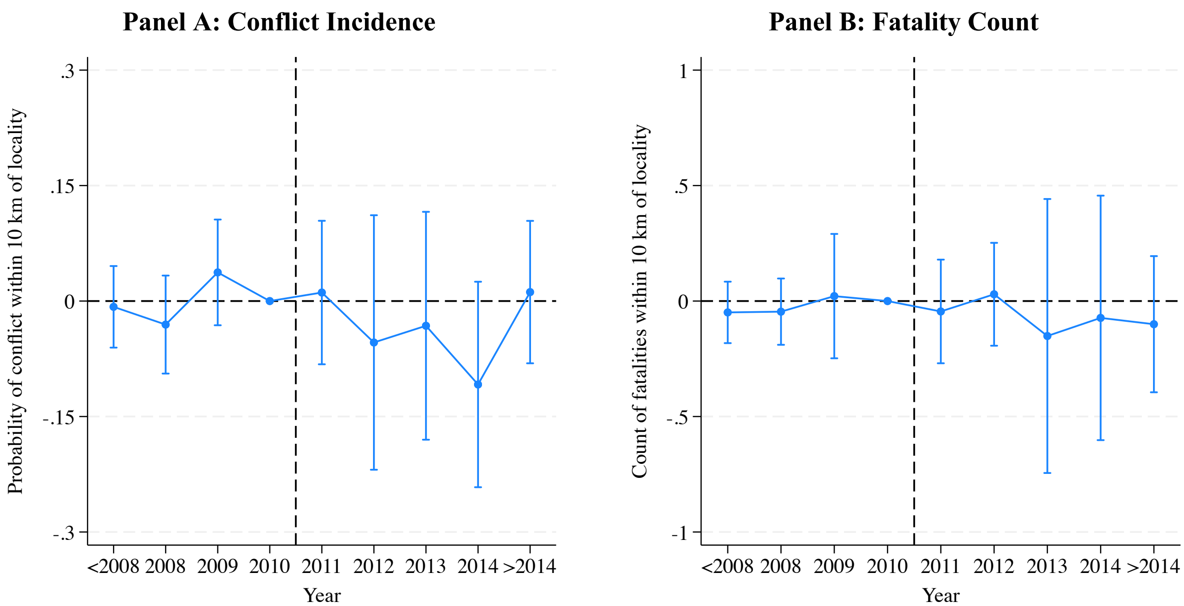
Notes: This figure reports coefficient estimates of OLS regressions estimating the effect of artisanal mine certification visits with "green" status on mine-level conflict incidence by mineral type. In Panel A, we estimate the model from panel A of Figure 4 but use radii of 2km or 1km instead of 10km. In Panel B, we replicate Panel A except excluding mines with overlapping radii (i.e., within 2km or 1km of another mine). The sample is a balanced panel from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and for infinite serial correlation are reported in parentheses. $\mathbb{1}$(*All Conflicts*) is a binary indicator for whether one or more incidents of conflict, defined according to Stoop et al. (2018), is observed within 2km or 1km of a mine.

Section IA4. Supplementary Tests for ITT Analysis

IA4.1 Treatment Using Known Gold Mines

In Figure IA4.1 below, we examine conflict incidence and fatalities around Dodd-Frank enactment for known small-scale gold mines. Similar to the primary ITT analysis, we do not observe a salient change in conflict around Dodd-Frank enactment.

Figure IA4.1. ITT Analysis Using Known Gold Mines

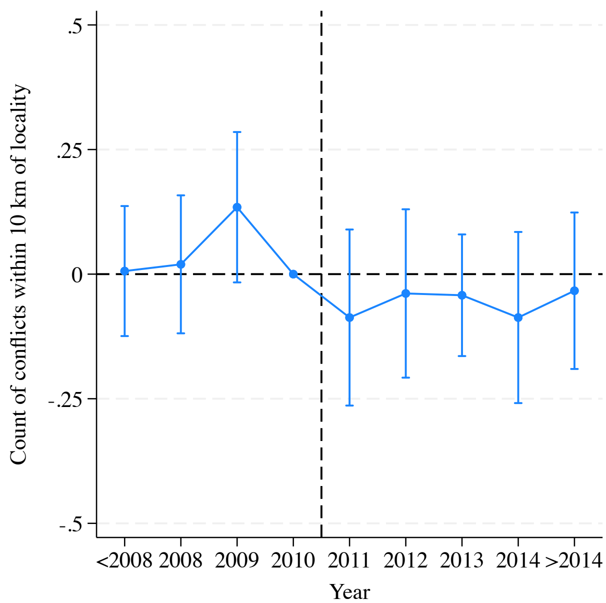


Notes: This figure presents results for both conflict incidence and fatalities around Dodd-Frank enactment using know gold mines. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of Dodd-Frank enactment on the probability of conflict within 10 kilometers of certified relative to uncertified mines. Panel B shows the effect of Dodd-Frank enactment on fatality count rather than conflict probability. We describe the sample selection in Section IA4 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. The baseline sample is a balanced panel of mines from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

IA4.2 Count of Conflicts as Dependent Variable

In Figure IA4.2 below, we examine the count of conflict occurrences around Dodd-Frank enactment. We still observe no significant change in conflict around Dodd-Frank enactment.

Figure IA4.2. ITT Analysis Using Conflict Count

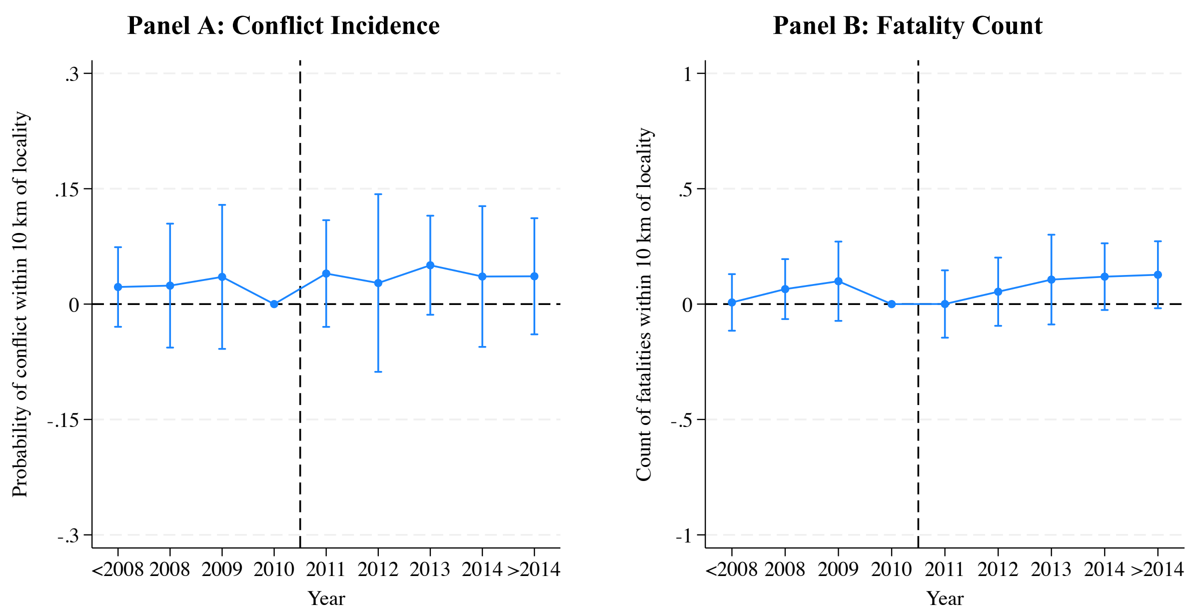


Notes: This figure shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of Dodd-Frank enactment on the conflict count within 10 kilometers of certified relative to uncertified mines. The baseline sample is a balanced panel of mines from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

IA4.3 Offensive against FDLR in 2009

In Figure IA4.3 below, we examine conflict incidence and fatalities around Dodd-Frank enactment excluding North and South Kivu provinces. We do not observe salient changes around Dodd-Frank enactment, which indicates that the abnormally high coefficient in year 2009 is indeed driven by the North and South Kivu provinces. One potential explanation is that areas suitable for mining are differentially targeted by the military operation because they have higher populations and more economic activity.

Figure IA4.3. ITT Analysis Excluding Nord- and Sud-Kivu

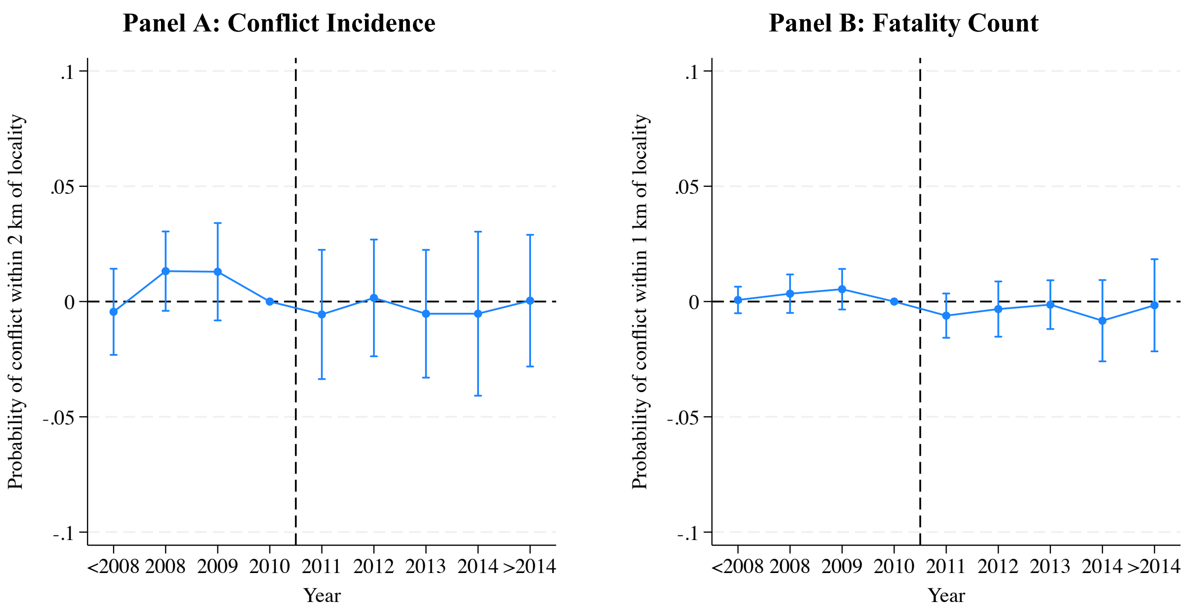


Notes: This figure presents results for both conflict incidence and fatalities around Dodd-Frank enactment excluding North and South Kivu provinces. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of Dodd-Frank enactment on the probability of conflict within 10 kilometers of certified relative to uncertified mines. Panel B shows the effect of Dodd-Frank enactment on fatality count rather than conflict probability. We describe the sample selection in Section IA4 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

IA4.4 Smaller Radii and Excluding Overlapping Areas

In Figure IA4.4 below, we present results for conflict incidence and fatalities around Dodd-Frank enactment when using smaller radii (i.e., 2 kilometers and 1 kilometer) and excluding all overlapping areas. If we were to exclude all overlapping areas with a 10-kilometer radius, the remaining sample would be too small to obtain reliable estimates. We do not observe large differential change in conflict incidence and fatality count for mining areas relative to non-mining areas after Dodd-Frank enactment.

Figure IA4.4. ITT Analysis Excluding Overlapping Areas



Notes: This figure presents results for both conflict incidence and fatalities around Dodd-Frank enactment excluding overlapping areas. Panel A shows coefficients and 95% confidence intervals for OLS regressions estimating the effect of Dodd-Frank enactment on the probability of conflict within 10 kilometers of certified relative to uncertified mines. Panel B shows the effect of Dodd-Frank enactment on fatality count rather than conflict probability. We describe the sample selection in Section IA4 of the Internet Appendix, and we further drop singletons corresponding to the fixed-effect structure. The baseline sample is a balanced panel of mines from 2004 to 2023. Conley (1999) standard errors allowing for spatial correlation within a 100km radius and infinite serial correlation are used unless otherwise specified.

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