

## Resource Cursed or Policy Cursed?

### U.S. Regulation of Conflict Minerals and Violence in the Congo<sup>\*</sup>

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June 3, 2016

forthcoming article in  
*Journal of the Association of Environmental and Resource Economists*

**Abstract:** There is widespread belief that civil conflict in poorly governed countries is triggered by surging international demand for their natural resources. We study the consequences of U.S. legislation grounded in this belief, the “conflict minerals” section of the 2010 Dodd-Frank Act. Targeting the eastern Democratic Republic of the Congo, it cuts funding to warlords by discouraging manufacturers from sourcing tin, tungsten, and tantalum from the region. Building from Mancur Olson’s *stationary bandit* metaphor, we describe some channels through which the legislation could backfire, inciting violence. Using geo-referenced data, we find the legislation increased looting of civilians, and shifted militia battles towards unregulated gold mining territories. These findings are a cautionary tale about the possible unintended consequences of imposing boycotts, trade embargoes, and resource certification schemes on war-torn regions.

**JEL Codes:** Q34, O17, D74

**Keywords:** civil conflict, resource curse, unintended consequences, conflict minerals, Dodd Frank Act, Democratic Republic of the Congo, resource certification

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<sup>\*</sup> For helpful comments, we thank P.J. Hill, Jennifer Alix-Garcia, Dan Phaneuf, Peter Hull, Lee Benham, Bevin Ashenmiller, Nathan Nunn, anonymous referees, and participants at workshops and seminars hosted by the Clinton Global Initiative, Property & Environment Research Center, University of Wisconsin, Clemson University, and Simon Fraser University. We also thank participants at conference sessions hosted by the Southern Economics Association, the Midwest International Economic Development Conference, the Occasional Workshop on Environmental and Resource Economics, the Society for Institutional and Organizational Economics, and the Ronald Coase Institute. Parker gratefully acknowledges funding from the Lone Mountain Fellowship program at the Property & Environment Research Center (summer 2012), and the University of Wisconsin Alumni Research Foundation (2013-2014). We thank Alexey Kalinin for GIS help, and Chris Fisher for help with data assembly.

## 1. Introduction

The belief that poorly governed countries are cursed by their natural resources is pervasive and it sometimes influences international policies towards trade in resources with those countries. When a country is afflicted by the rawest form of the curse, increased international demand for its resources leads to conflict rather than improved welfare. Increased demand is a curse because it generates revenue to fight over, and it provides funds for weapons and soldiers (Hirshleifer 1991, Grossman 1991, Collier and Hoeffler 2004, Olsson 2007, Janus 2011).

We study a region that is ostensibly afflicted by the curse: the eastern Democratic Republic of the Congo (DRC). The DRC is the world's 11<sup>th</sup> largest country by area and the 19<sup>th</sup> most populated. It ranks 156<sup>th</sup> out of 162 countries in assessments of peacefulness, and dead last in GDP per capita.<sup>1</sup> The eastern DRC contains minerals to supply surging global markets for mobile phones, tablets, and other modern electronic devices. The region is rich in natural resources, but poor by various measures of human welfare.

It is clear that some revenue from mineral sales goes to warlords who control mining areas and sometimes commit brutal acts of violence. This observation contributes to the widespread perception that the eastern DRC is cursed by surging global demand. This is why human rights advocacy groups have dubbed the endowments “conflict minerals”, which refers to tin, tungsten, tantalum, and gold from the region.

Convinced that the DRC suffers from the resource curse, advocacy groups, such as the Enough Project and Global Witness, have successfully lobbied for top-down policies to reduce international demand for Congolese minerals extracted with armed group involvement. The primary policy is the U.S. Dodd-Frank Act of 2010, Section 1502, which was endorsed by co-sponsor Barney Frank as a measure to “cut off funding to people who kill people” (Aronson 2011). Section 1502 regulates large U.S. manufacturing companies by requiring them to trace and report the origin of minerals used in products. Section 1502 has acted as an “intended or unintended boycott” on purchases of tin, tantalum, and tungsten – the “3Ts” - from the eastern DRC (Pöyhönen et. al 2010, 27). Gold, however, has been de facto exempt, because it is more difficult to trace. Two years after Dodd-Frank was passed, advocacy groups were claiming

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<sup>1</sup> See [www.ihrr.com/contry.php](http://www.ihrr.com/contry.php), [www.visionofhumanity.org/#/page/indexes/global-peace-index](http://www.visionofhumanity.org/#/page/indexes/global-peace-index), and data from the World Bank at <http://data.worldbank.org/indicator/NY.GDP.PCAP.CD>.

success, stating “the passage of conflict minerals legislation ... [has] helped lead to a 65% drop in armed groups’ profits from trade in tin, tantalum, and tungsten ...” (Johnson 2013, p. 53).

But did the legislation actually reduce violent conflict? In this paper, we examine the effects of Section 1502, and a related mining ban, on the incidence of conflicts in the eastern DRC. We theorize on mechanisms through which the policies could backfire in the short-run, causing violence in policy-targeted territories to increase rather than fall. We assess the policy effects by employing geo-referenced data on artisanal mining sites and on armed conflict before and after the policies, from 2004 through 2012. The evidence suggests the legislation increased the probability of civilian looting by at least 143%, and that it increased the probability of battles in territories endowed with unregulated gold. These quantitative findings contribute to the ongoing debate about Section 1502, and support qualitative descriptions of how the law may have harmed civilians in targeted mining regions (Geenan 2012, Aronson 2011, Sematumba 2011, Pöyhönen et. al. 2010, Seay 2012, Radley and Vogel 2015). The findings also contribute to debate about resource governance interventions, such as the Kimberley Process Certification Scheme, that may rely on “...unsupported assumptions regarding how natural resources are linked to the motivations of combatants” (Cuvelier et al. 2014, 2).

Our main contribution is in providing quantitative evidence of unintended consequences, but we also develop theory that emphasizes pathways from resource value shocks to conflict that differ from those highlighted elsewhere in the economics literature. The recent literature is predominantly concerned with two mechanisms that govern conflict in countries where property rights are weak: rapacity and opportunity cost. On one hand, a rise in endowment values means there are larger spoils to fighting, so the rapacity effect implies an increase in violence in response (Hirshleifer 1991, Grossman 1991, Olsson 2007).<sup>2</sup> On the other hand, positive shocks to resource values could increase the opportunity cost of fighting, drawing labor into non-conflict industries (Becker 1968, Grossman 1999, Chassangy and Miquel 2009).

Focusing on these theoretical effects suggests the key to understanding whether violence rises or falls with resource-value shocks lies in understanding the extent to which resource

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<sup>2</sup> Recent empirical support for the dominance of the rapacity effect is provided by Angrist and Kugler (2008) who find a positive relationship between coca prices and violence in Colombian municipalities dependent on coca production. Besley and Persson (2008) also find a positive relationship between world prices of a country’s main commodities and civil wars using a broad sample of countries. Also consistent with the rapacity effect is the finding that U.S. food aid has increased the incidence and duration of civil conflicts, particularly in countries with a history of civil conflict, presumably because the food aid creates spoils to fight over (Nunn and Qian 2014).

commodity production is labor rather than capital intensive (Dube and Vargas 2013). In cases where production is labor intensive (e.g., agriculture), the opportunity cost effect can dominate, but in cases where production is capital intensive (e.g., oil extraction), the rapacity effect can dominate.<sup>3</sup> Artisanal mining in the eastern DRC is labor intensive, with almost no capital inputs (see section 2). This suggests the opportunity cost effect may dominate in our setting. If it does, conflict should have increased as a result of Section 1502, which lowered the net value of mineral endowments to local civilians and militias. Thus, opportunity cost is one candidate explanation for our empirical findings.

The opportunity cost concept, however, does not fully explain patterns of conflict in the data. First, the policies primarily increased looting of civilians, rather than militia battles, and it is not clear why falling opportunity costs would have this asymmetric effect. Second, to the extent the policies increased battles between militias, they did so in territories dominated by gold mining. This nuance is not explained by general decreases in the opportunity cost of militia participation; it is better explained by rapacity to fight over unregulated gold.

Rather than relying solely on rapacity and opportunity cost tradeoffs for theoretical guidance, we develop an analytical framework that is related to crime displacement models and inspired by research on the economic functions of organized crime (Skaperdas 1992, Fiorentini and Peltzman 1997, Skaperdas 2001) and Olson's (1993) *stationary bandit* metaphor.<sup>4</sup> Stationary bandits are akin to mafia groups that tax neighborhoods and industries. They emerge in power vacuums, where the state is absent, to provide protection (Skaperdas 2001). Mafia groups maximize revenues by selling protection to civilians – both against crime they will commit themselves (if not paid coerced taxes) and against crime committed by others. Hence, if one mafia group has the power to monopolize taxation, there will be little ordinary violence. Yet this low-violence, stationary bandit equilibrium is precarious. Groups must find it advantageous to protect rather than harm civilians, and to remain in an area rather than moving to loot other unprotected neighborhoods, or to challenge the areas controlled by competing groups.

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<sup>3</sup> Dube and Vargas (2013) find a positive relationship between oil prices and conflict in oil producing regions of Columbia, but a negative relationship between coffee prices and conflict in coffee producing regions. Brückner and Ciccone (2010) find that decreases in a country's main commodity are associated with increases in the likelihood of civil war, which is consistent with a dominant opportunity cost effect.

<sup>4</sup> Our theoretical reasoning is informed by Mampilly's (2011) research on *Rebel Rulers*. Our reasoning complements Maystadt et. al (2014), who model the incentives of militias to exploit and protect minerals in the DRC using ideas from the crime displacement literature, and Sanchez de la Sierra (2014), who also analogizes armed groups in the DRC to stationary and roving bandits.

Applying this analogy to the eastern DRC, mafia groups are militias, mining villages are neighborhoods, and the relevant industries are 3T and gold mining. As we describe in section 2, the mafia characterization fits our setting, where there is a power vacuum created by the lack of state authority. We refer to surveys of militarized mining sites, which describe how the sites were controlled by “mafia-like” groups who “taxed” labor in somewhat predictable ways, prior to Dodd-Frank. In exchange, the armed groups provided a crude form of protection. This characterization complements recent research by Buonanno et al. (2015), whose findings suggest the Sicilian mafia may have emerged to protect sulphur mining when state control was absent.

For these reasons, our analytical framework in section 3 assumes that militia groups choose between a) stationing near mining sites for the purpose of taxing, b) roving and looting civilians, and c) challenging another militia for control over mineral deposits. Section 1502 lowered the value to militias of stationing near 3T sites, but had less of an effect on the value of taxing labor near gold mining sites. We explain how this asymmetric shock could have triggered a sequence of looting, first against civilians near 3T deposits as militias ceased ‘protection’, and then towards non-mining areas. Logically, the asymmetric regulation of 3T and gold mining would lead to armed group competition for control over gold deposits. We examine these ideas empirically in section 5, and generally find support for the analytical framework. We also discuss factors not modeled in the framework but that may have contributed to the observed patterns of conflict.

## **2. Background**

### *A. Artisanal Mining Prior to Dodd Frank*

Mining has been an important contributor to the DRC economy since colonial times. According to the U.S. Geological Survey, the mining sector’s recorded contribution to GDP was 13.4% in 2009, but the World Bank estimates that it could account for 20-25% if the sector was better managed (World Bank 2008). Mineral deposits are scattered throughout the eleven provinces, but it is in the east where artisanal mining is infused with armed groups, and where the attention on conflict minerals is focused. The eastern provinces usually associated with conflict minerals are North and South Kivu, Maniema, Orientale, and Katanga (map 1) (Bawa 2010, D’Souza 2007, de Koning 2011).

Artisanal miners are not officially employed by mining companies, but instead work independently using their own resources to pan and dig for alluvial, open pit, and hard rock mineral deposits. Artisanal mining is labor intensive and employs minimal technological inputs. Estimates of the number of artisanal miners in the five eastern provinces are rough but ranged from 710,000 to 860,000 in 2007 (D'Souza 2007). The World Bank (2008, 10) estimates that artisans produce 90% of the minerals exported from the country.

The key minerals in the east are tin (from cassiterite), tantalum (from coltan and tantalite), tungsten (from wolframite) and gold. The Enough Project estimates the DRC's contribution to world supply of the 3Ts and gold. For tantalum, the estimate is 15-20%; for tin 6-8%; for tungsten 2-4%; and for gold, less than 1%. However, tin and gold have generated much more local revenue.<sup>5</sup>

Map 2 shows the distribution of mining sites based on interactive maps created by the International Peace Information Service (IPIS) during 2008-2010, before Dodd-Frank. The data were gathered by research teams equipped with GPS devices and questionnaires. The teams identified coordinates of sites and solicited information about minerals, and the presence of militias and their interactions with miners. The IPIS outsourced data collection to local teams in order to best locate and access remote mining sites.<sup>6</sup> In spite of this strategic methodology, the IPIS notes their mapping is “not an exercise in exact science” (Spittaels and Hilgert 2009, 7), because logistical challenges prevented teams from locating and entering every site. Rather than omitting important sites that researchers could not physically visit, the IPIS estimated those locations and included them in the maps.

Map 2 suggests that mining sites follow geological endowments rather than road networks, reducing concern that the distribution of reported mines is biased towards infrastructure. The coincidence of mining sites with rivers suggests alluvial deposits. Of the 659 mines identified, 415 were gold, 214 were tin, 23 were coltan, and 8 were wolframite.

Based on IPIS surveys conducted prior to Dodd Frank, roughly one-half of the mines were controlled by, or visited regularly by, militias (including the Congolese Army), usually for

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<sup>5</sup> [www.enoughproject.org/files/Comprehensive-Approach.pdf](http://www.enoughproject.org/files/Comprehensive-Approach.pdf). The Enough Project also estimates that tantalum has provided armed groups in the eastern Congo with \$12 million in 2008, tungsten with \$7.4 million, tin with \$115 million, and gold with \$50 million.

<sup>6</sup> This allowed teams to “... rely on their own networks and contracts in the region, which allowed them to enter sites that others could not and speak to people who would never answer the same questions asked by a stranger, especially a white man (Spittaels and Hilgert 2009, 6). The drawback is that the use of multiple researchers led to more “...inconsistencies due to the different ‘research habits’ developed by the teams” (p. 6).

the purpose of taxing civilian miners.<sup>7</sup> Examples of interactions between these groups and miners, from Spittaels and Hilgert (2008, 2010), include the following: “The soldiers secure the area and levy taxes”; “12 soldiers are present. Anyone entering the site has to pay them FC 500 to 1000”; and “Receive 500 FC from each miner on Thursdays and Fridays.”

### *B. Dodd-Frank and the DRC Mining Ban*

The United States’ first attempt to regulate conflict minerals was in April 2009 with the proposed Congo Conflict Minerals Act. That legislation failed to pass but a revised version was passed as Section 1502 of the Dodd-Frank Act.<sup>8</sup> Section 1502 is designed to discourage major manufacturing and processing companies from using conflict minerals. It also authorized Congress to produce a map (which it commissioned from the IPIS) of conflict mining zones in the eastern DRC to guide the regulatory process. As we discuss below, this map provides us with a way of identifying the mining areas most directly targeted by Section 1502 regulations.

Section 1502 affects the reporting requirements of perhaps half (at least 6,000) of all publicly traded companies in the United States (KPMG 2011). It directs the Securities Exchange Commission to make disclosure rules for companies manufacturing products containing tin, tungsten, tantalum, or gold. The rules require companies to conduct “due diligence” on the origin of minerals; if the origin is from a DRC conflict mining zone then companies must report on the possibility that warlords have benefitted from the purchases.

The Dodd-Frank Act was signed into law on July 21, 2010. Although the Act did not prohibit mineral purchase from eastern DRC conflict zones, observers contend it acted as a de facto boycott of such purchases (Pöyhönen et. al. 2010, Seay 2012). This is because the easiest way for companies to report being conflict-free is to avoid minerals from the entire conflict zone. The boycotting of eastern DRC minerals has been more explicit since April 1, 2011, when a coalition of large electronics and high-technology companies – the Electronic Industry Citizenship Coalition (EICC) - stopped buying the 3Ts from smelters unable to prove their source minerals did not fund DRC conflict (Wimmer and Hilgert 2011).

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<sup>7</sup> Based on reports from Spittaels and Hilgert (2009) and Spittaels (2010), 214 mines in the Kivus and its hinterlands were controlled by, or visited regularly, by militia groups. The Armed Forces of the Democratic Republic of Congo (FARDC) was present at 142 mines, the Democratic Forces for the Liberation of Rwanda (FDLR) was present at 37, and the other groups at mines included Mayi Mayi militias (an umbrella term for loosely affiliated groups of local militias), the National Congolese Police (PNC), and the Forces Républicaines Fédéralistes (FRF).

<sup>8</sup> See [www.opencongress.org/bill/111-s891/show](http://www.opencongress.org/bill/111-s891/show).

Arguably as a response to Dodd-Frank, the DRC also imposed a governmental ban on artisanal mining on September 11, 2010.<sup>9</sup> The ban covered three provinces - Maniema, North Kivu, and South Kivu. A week after the ban was announced, the Congolese Minister of Mines stated that it concerned extraction of the 3Ts (see de Koning 2010), but observers note confusion about whether or not gold was covered, and most reports include artisanal gold among the banned mines. The ban was lifted on March 10, 2011, shortly before the international EICC boycott took hold on April 1, 2011 (de Koning 2011).

How did Dodd-Frank and the mining ban impact mining output? We do not have production data but official export data, which may be an unreliable measure of output, reveal a large drop in exports of tin, coltan, and wolframite during 2010-2012. Figure 1A shows the decrease in tin exported from the tin producing provinces of the DRC. The volume of reported exports correlated positively with the world price from 2004-2009, but then dropped significantly, despite high world prices. Reported tin exports from Katanga Province increased after Dodd Frank. Mines in this province were exempt from the mining ban, and many were outside of the conflict zone mapped by the U.S. State Department. Figure 1B shows monthly export data of tin from North Kivu. Official exports went to zero during the ban. Stockpiles were exported in March 2011, during a window of time between the end of the ban and the April 1, 2011 deadline that EICC companies had set to stop buying 3T minerals from smelters lacking traceability systems. Chinese companies continued to buy uncertified 3T minerals after the boycott, but at prices discounted of “up to 80 percent compared to world market valuations” (Carisch 2012, 15).

Export data are a less reliable measure of gold output because approximately 98% of gold mined in the eastern DRC is smuggled, presumably because it is valuable in small, easy to conceal quantities (United Nations 2014, de Koning 2011). Figure 1C shows USGS estimates of DRC gold production over 2004-2012. Estimated production decreased slightly during 2011 when the mining ban was in force, but rose during 2012 in spite of Dodd Frank.

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<sup>9</sup> DRC’s President stated the ban’s goal was to weed out “mafia groups” from the mining industry. Some observers think the ban was a response to international pressure to stop trade in conflict minerals (Geenan 2012, Seay 2012). Seay (2012) states: “Neither Kabila’s ban or the MSC’s [EITC boycott] decision to stop buying Congolese minerals would have happened had Dodd-Frank not become law. Both the timing of the actual and *de facto* bans and all rhetoric surrounding them suggests that these were clear responses to the perceived future effects of the legislation. MSC and other international buyers are not purchasing Congolese minerals due to uncertainty about the SEC regulations on Section 1502.”



Why would gold production rise despite its official status as a “conflict mineral” under Dodd Frank? There are two main reasons. First, much of the gold mined in the DRC supplies Middle East and East Asian jewelry markets that are not regulated by Dodd Frank (de Koning 2011). Second, purchases of gold from companies that are regulated by Dodd Frank have been de facto exempt because regulators recognize it is difficult to trace gold and demonstrate it is not from a mine controlled by militias. For 3Ts, tracing is relatively easier because 3Ts are extracted with bulky waste rock that is difficult to completely remove without using smelting facilities absent in the eastern DRC. The extra rock helps distinguish the mine of origin, due to different coloration and texture (Prendergast and Lezhnev 2009). By contrast, gold is relatively easy to melt and separate from waste rock locally, before it reaches official trading locations. These geological differences mean it is possible to co-mingle refined gold from different mines early in the supply chain, making further tracing impossible (Schraeder 2011, Khushrushahi 2011).

### *C. Corroborating Evidence from Satellite Images*

To corroborate and cross-reference the patterns of 3T and gold production implied by figure 1, we analyze annual satellite images of forest cover around each of the IPIS mining sites before and after Dodd Frank. Our approach is inspired by Wimmer and Hilgert (2011, 9) who study satellite images of a large tin mine in North Kivu before and after the mining ban. From those images, they infer that some mine expansion occurred during the ban, meaning the ban was not fully effective. Visual inspection of their images indicates that mine expansion correlated with deforestation: the areas over which the mine expanded were also deforested.

The satellite images that we employ span 2000-2012 and come from Hansen et al. (2013). We assume that accelerating deforestation within a small radius around the centroid of a mine implies accelerating mining. We assume that slowing deforestation implies a decreased rate of mining. Although a change in deforestation rates is an imprecise signal of changes in mining activity, we argue it is a useful signal, as shown in Wimmer and Hilgert’s (2011) case study. Moreover, other research also finds positive relationships between deforestation and artisanal mining in the DRC (Bustic et al. 2015) and in other African locales (Hirons 2011).

We define relative deforestation before and after Dodd Frank for a radius around each mine as follows:

$$\text{deforestation} = \begin{cases} -1 & \text{if } 2011 + 2012 \text{ forest loss} < 2008 + 2009 \text{ forest loss} \\ 0 & \text{if } 2011 + 2012 \text{ forest loss} = 2008 + 2009 \text{ forest loss} \\ +1 & \text{if } 2011 + 2012 \text{ forest loss} > 2008 + 2009 \text{ forest loss} \end{cases}$$

We evaluate the relationship between Dodd Frank and deforestation using

$$(1) \quad \text{deforestation}_m = \alpha + \lambda(3T \text{ indicator})_m + \delta(\text{policy territory indicator})_m + \beta(3T \text{ ind.} \times \text{policy ind.})_m + \eta(\text{forest loss over 2001-2007})_m + \varepsilon_m$$

Here  $m$  indicates the 659 mining sites. The policy territories are displayed in map 3 and are defined in table 2. The 3T indicator equals 1 for 3T mines. The omitted category is gold mines in non-policy territories. We control for aggregate deforestation over 2001 to 2007 to account for differences in forest cover at the beginning of 2008.

Table 1 reports OLS estimates for radius lengths of 100, 200, and 500 meters. The key results are the estimates of  $\beta$ , which are negative and statistically precise, indicating the conditional mean of the dependent variable is smaller around 3T mines in areas targeted by Dodd Frank. These negative coefficients are consistent with 3T export data, and with on-the-ground anecdotes, in that all pieces of information suggest Dodd Frank slowed 3T mining in targeted areas. The positive estimates of  $\lambda$  provide some evidence that 3T mining increased outside of the policy territories, which is consistent with the rise in reported exports from Katanga province after Dodd Frank (see figure 1). The coefficients on  $\delta$  are statistically imprecise and effectively zero, which is consistent with other indications that Dodd Frank did not slow gold mining within the areas targeted by Dodd Frank.

To summarize, disparate data sources suggest that Dodd Frank slowed but did not stop 3T mining, and that it had less of an impact on gold mining within the targeted areas. In the next section we theorize about how this disruption in mining affected armed conflict.

### 3. Analytical Framework

We present a stylized analytical framework to guide the empirical analysis and to highlight some channels through which Dodd Frank could have increased conflict. We impose several simplifying assumptions to facilitate a concise explanation of the channels and, where possible, our assumptions are aligned with facts about the empirical setting described above.

The framework captures the essence of Olson's (1993) stationary bandit idea and theories of organized criminals (Skaperdas 2001). The structure closely resembles crime displacement

models. In order to focus on short-run impacts, we fix the amount of armed capital held by a militia, and describe the conditions under which it will be ‘stationed’ at a mining site for the purpose of taxing mining labor. We then illustrate how the policies could disrupt what we call a “stationary bandit” equilibrium, triggering an increase in looting and battles.

#### *A. Labor and Mineral Extraction*

We model a landscape containing  $N$  homogenous agricultural sites ( $A$ ) and two artisanal mining sites. The mining sites contain one of two different minerals, labeled  $G$  (for gold) and  $C$  (for cassiterite). There are  $L$  homogenous civilian laborers who work in agriculture and the mines, such that  $L = L_A + L_G + L_C$ . We assume agricultural labor is evenly distributed across sites so that  $L_A = Nl_n$ , where  $l_n$  is agricultural labor at site  $n$ . The expected wage in agriculture is fixed at  $w$ , and is effectively a subsistence wage. Random rainfall shocks occur at the different agricultural sites such that the realized wage during a time period is high ( $\bar{w}$ ) or low ( $\underline{w}$ ), where  $\bar{w} > w > \underline{w}$ . Artisanal mining technology is such that the quantity of minerals extracted is increasing in labor at a decreasing rate. To simplify exposition and to generate expressions that are easy to compare, we assume  $Y_i = L_i^{0.5}$  for  $i=G$  and  $C$ , and ignore dynamic depletion of a mine.

#### *B. Militia Groups*

We assume there are two militia groups, where each has a fixed number  $S_k$  of armed soldiers that are not part of the civilian labor force.<sup>10</sup> Each militia deploys its soldiers in one of three mutually exclusive activities: stationing at a site for the purpose of taxing miners, roving across sites for the purpose of looting, or moving to another site to battle for its control. Looting is a surprise event that takes all income from a site’s civilians, whereas taxing is predictable and does not strip civilians of all income. Taxing also differs from looting because it provides security for civilians against looting by the other militia. Battles between militias occur only to the extent the two groups seek control over the same site. If the groups battle, the probabilities of victory are ex ante known and based on relative size, so that, for example,  $P(1 \text{ defeats } 2) = S_1 / (S_1 + S_2)$ .

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<sup>10</sup> We abstract from the possibility that soldiers can mine by assuming soldiers earn a higher return on their labor and guns in non-mining activities. There is evidence that most militia members in the Eastern DRC do not regularly mine themselves (see Sanchez de la Sierra 2014) but there are exceptions described in the IPIS mapping surveys.

The maximum value to an armed group of taxing a mining site  $i$  is given by

$$(2) \quad V_i^{TAX} = \sum_{t=0}^T \rho^t [\tau_i^* L_i(p_i, w, \tau_i^*)], \quad i = G, C$$

where  $T$  is the planning horizon,  $\tau_i^*$  is the revenue-maximizing tax on each laborer,  $L_i^*(p_i, w, \tau_i^*)$  is the number of laborers,  $p_i$  is the price per unit of mineral output, and  $\rho$  is the discount factor, with  $0 < \rho < 1$ .<sup>11</sup> A site is secured for taxing if the armed group has stationed its soldiers at the site, and it is not engaged in battle with the other group.<sup>12</sup> Stationing soldiers ‘protects’ labor from being looted by the other group. We assume that neither group has sufficient armed soldiers to station at and secure more than one site. In addition, we assume that militia groups will never station at and tax a specific agricultural site. The random (and seasonal) nature of realized wages across agricultural sites makes those sites relatively unattractive for stationary taxation, because armed groups can plausibly extract more revenue from roving and looting agricultural sites as they experience favorable shocks, rather than stationing at a single site through periods of unfavorable shocks.<sup>13</sup>

### C. Stationary Bandit Equilibrium

The system is in a stationary bandit equilibrium if each militia group is ‘stationed’ at a mining site, taxing labor, and neither group could gain expected revenue by looting civilians, or by challenging the other armed group for control over the other mining site. If  $L$  is large enough to dissipate mining rents, a stationary-bandit equilibrium dictates that labor will enter each mining site until the per miner after-tax income equals the expected agricultural wage, such that

$$(3) \quad \frac{p_i Y_i}{L_i} - \tau_i = p_i L_i^{-0.5} - \tau_i = w, \quad i = G, C.$$

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<sup>11</sup> We model labor taxes rather than output taxes but we recognize that output taxes are also imposed by militia groups, often at road blocks downstream from mining sites (Nest 2011). We focus on labor taxes because the modelling is simpler and for two additional reasons. First, the IPIS data described in section 2 suggest that labor taxes were more typically used at mining sites. Second, because many output taxes are levied downstream from mining sites, away from production, there is a weaker link between those taxes and miner protection.

<sup>12</sup> These conditions are related to the conditions for secure ‘ownership’ of any economic asset (see Barzel 1997). Similar requirements were important in the evolution of property rights to mines in Australia, the United States, and elsewhere (see e.g., Anderson and Hill 1975, Umbeck 1977, La Croix 1992, and Libecap 2007).

<sup>13</sup> We recognize this is a strong assumption that may seem to be at odds with empirical observation of militias in the DRC levying what appear to be predictable and consensual fees on agricultural output at road blocks (Laudati 2013). These downstream roadblock fees do conform with our definition of taxing, but it is important to emphasize that this system of taxation is imposed far from farming sites. We are simply assuming that looting dominates taxation at the scale in which militias can offer protection, which is the  $N$  agricultural sites that we model.

After solving for labor, the equilibrium tax revenue can be expressed as

$$(4) \quad \tau_i^* L_i = \tau_i^* \left( \frac{p_i}{w + \tau_i^*} \right)^2.$$

Maximizing revenue with respect to the tax rate we find  $\tau_i^* = w$ , so that equation (4) becomes

$$(5) \quad \tau_i^* L_i = \frac{p_i^2}{4w}.$$

In a stationary equilibrium, labor is therefore distributed according to

$$(6) \quad L_G = \frac{p_G^2}{4w^2}; \quad L_C = \frac{p_C^2}{4w^2}; \quad L_A = L - \frac{p_G^2 + p_C^2}{4w^2}.$$

All laborers earn a subsistence wage in expectation, and there is taxing, but not looting.

#### *D. Incentives to Loot*

We now consider the payouts to an armed group from looting, assuming the stationary-bandit equilibrium as a starting point. To be more concrete, we assume Group 1 initially controls and taxes the cassiterite (tin) site and that Group 2 initially controls and taxes the gold site. We focus on Group 1's incentives, and consider Group 2 only in the context of how it responds to the actions of Group 1.

Figure 2 illustrates Group 1's payoffs in a one-period model. If Group 1 loots tin miners' earnings, it appropriates the one-time revenue earned at the tin mining site of

$$(7) \quad p_C Y_C = p_C \sqrt{L_C(p_C, w, \tau_C^*)} = \frac{p_C^2}{2w}.$$

This revenue is twice the per-period revenue Group 1 could extract by taxing (equation 5). Hence, it would never be optimal for Group 1 to tax in a one-period planning horizon.

If the planning period is two periods, there is a cost to looting the mining site, due to lost future tax revenue. Specifically, we assume a mining site is unproductive in the period after it is looted, because the looting event harms or displaces the labor force. Figure 3 illustrates the payouts to Group 1 under a two-period planning horizon. If Group 1 taxes in  $t=0$ , then its soldiers remain stationed at the tin mining site through  $t=1$ .<sup>14</sup> If Group 1 loots the tin site during

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<sup>14</sup> It will always be optimal for Group 1 to use those stationed soldiers to loot the tin mine during  $t=1$ , because it is the final period. The planning horizon resets after  $t=0$ , however, so the final period of looting would never actually be reached.

$t=0$ , then its soldiers are free to rove, and will either loot an agricultural site experiencing a favorable rainfall shock, or battle for the right to loot the gold site in  $t=1$ .

Note from figure 3 that the ‘stationary path’ payout from taxing the mine in  $t=0$  is  $(1+2\rho)p_C^2/4w$ , and the payout from the ‘looting path’ is  $p_C^2/2w + \rho\bar{w}l_j$ . The term  $\bar{w}l_j$  represents agricultural revenue at any site where a favorable shock has occurred. To simplify, we assume labor is not mobile in or out of sites during the two-period planning horizon, so that  $l_j$  is fixed across the two periods.

Given these assumptions, a sufficient (but not necessary) condition for Group 1 to loot the tin site in  $t=0$  is  $(1+2\rho)p_C/4w < \rho\bar{w}l_j$ . Assuming  $\bar{w} = \alpha w$ , with  $\alpha > 1$  indicating the degree to which the weather shock raises wages above expectation, we rewrite the expression as

$$(8) \quad \rho \left( 2 - \alpha l_j \frac{4w^2}{p_C^2} \right) < 1.$$

Comparative statics of (8) indicate that looting is more likely with an increase in  $w$  or  $\alpha$ , or with a decrease in  $p_C$  or  $\rho$ .

The attractiveness of the ‘loot and battle’ path to Group 1 depends on how Group 2 would respond to Group 1’s looting in  $t=0$ . If Group 2 loots the gold site in  $t=0$ , then Group 1 would have no incentive to battle for the gold site, because it was rendered unproductive by the previous period looting. If Group 2 remains and defends the gold mine in  $t=1$ , then the expected revenue to Group 1 of the ‘loot and battle’ path is  $p_C^2/2w + \rho\pi p_G^2/2w$ , where  $\pi$  is the probability that Group 1 defeats Group 2 in battle. Conditional on Group 2 fighting, Group 1 prefers the ‘loot and battle’ path to the ‘stationary path’ when  $(1+2\rho)p_C^2/4w < p_C^2/2w + \rho\pi p_G^2/2w$ , which we rewrite as

$$(9) \quad \pi \frac{p_G^2}{p_C^2} + \frac{1}{2\rho} > 1.$$

Comparative statics of (9) indicate the ‘loot and battle’ path becomes more attractive to Group 1 as the relative price of gold rises, and the discount factor falls. Similarly, conditional on Group 2 fighting, Group 1 prefers the ‘loot and battle’ path to the ‘looting path’ when  $p_C^2/2w + \rho\pi p_G^2/2w > p_C^2/2w + \rho\alpha w l_j$ , which we rewrite as

$$(10) \quad \pi p_G^2 > 2\alpha w^2 l_j.$$

Comparative statics of (10) indicate the ‘loot and battle’ path becomes more attractive to Group 1, relative to the ‘looting path’, as the price of gold rises.

The ‘loot and battle’ path is feasible only if Group 2 remains stationed at the gold site during  $t=1$  to defend it, rather than looting the gold site in  $t=0$ . Group 2 will remain stationed if  $(1 + 2\rho(1 - \pi))p_G^2 / 4w \geq p_G^2 / 2w + \rho\alpha w l_j$ . Rewriting, Group 2 will remain and tax in  $t=0$  if

$$(11) \quad \pi \leq 1 - \frac{1}{2\rho} - \frac{2\alpha w^2 l_j}{p_G^2}.$$

Comparing (9), (10), and (11) indicates that, although a larger  $\pi$  raises the expected payout to Group 1 of the ‘loot and battle’ path – conditional on Group 2 staying to fight – a larger  $\pi$  simultaneously lowers the probability that Group 2 will stay to fight. Rewriting, we note that battle over the gold site will occur if the following conditions hold:

$$(12) \quad \left(1 - \frac{1}{2\rho}\right) \left(\frac{p_C}{p_G}\right)^2 < \pi \leq 1 - \frac{1}{2\rho} - \frac{2\alpha w^2 l_j}{p_G^2}$$

$$(13) \quad \frac{2\alpha w^2 l_j}{p_G^2} < \pi \leq 1 - \frac{1}{2\rho} - \frac{2\alpha w^2 l_j}{p_G^2}.$$

An increase in the price of gold increases the probability that conditions (12) and (13) will hold. A decrease in the price of tin raises the probability that (12) will hold, and has no effect on (13). Therefore, if there are large asymmetries in the two militia groups, then battles will be induced only by large changes in mineral prices. Asymmetry in military power helps bind the stationary-bandit equilibrium.<sup>15</sup>

### *E. Effects of the Mining Policies*

We hypothesize that Dodd Frank broke down a stationary bandit equilibrium. First, the policy sharply lowered the local price of minerals, particularly tin, tantalum and tungsten (see section 2). In our theory, a decrease in  $p_C$  makes looting relatively more attractive than taxing tin and thus raises the probability of looting (see equation 8). Second, the policies caused the local price of the 3Ts to fall more dramatically than the local price of gold. In our theory, a decrease in the relative price,  $p_C / p_G$ , raises the probability of battle over gold mines in  $t=1$  (see

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<sup>15</sup> This result complements a literature suggesting the probability of conflict in the absence of clear property rights is higher when power is relatively symmetric (see Umbeck 1981, Hirshleifer 1991, Skepardas 1991, Anderson and McChesney 1994, Fearon 1995, and Ralston 2012).

equation 12). Third, the policies made the future of artisanal 3T mining more uncertain, plausibly shortening militia planning horizons. In our theory, a planning horizon of one-period rather than two periods guarantees looting.

Adapting our stylized theory to the real setting in the eastern DRC with multiple armed groups and multiple mining sites motivates the following testable hypotheses:

- H1. The conflict mineral policies increased the probability of battles between armed groups over gold mining sites, but not over 3T mines.
- H2. The conflict mineral policies caused some armed groups that were stationed at 3T mines to abandon those mines and loot civilians, first near the 3T mining sites and then in surrounding non-mining areas.
- H3. The conflict mineral policies caused some armed groups that were stationed at gold mines to abandon those mines and loot civilians, first near the gold mining sites (in order to avoid battles with armed groups seeking gold site control), and then in surrounding non-mining areas.

In addition, the theory implies that looting is more likely in agricultural areas receiving favorable weather shocks, which is testable in the data.<sup>16</sup>

#### **4. Data for Empirical Analysis**

To assess the effects of the mineral policies, we employ data on armed civil conflict, the location of artisanal mining sites (described in section 2), and on world mineral prices.

##### *A. Conflict Data*

The conflict data come from the Armed Conflict Location and Event Dataset (ACLED). This dataset provides information on internal conflict disaggregated by date, location, and by actor or actors for several unstable African countries, including the DRC.<sup>17</sup>

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<sup>16</sup> The framework also generates hypotheses that we are unable to test. For example, we expect the policies to have caused a decline in the number of mines at which militias were stationed and taxing, and we expect the policies to have caused an increase in militia mobility.

<sup>17</sup> The ACLED data are available at [www.acleddata.com](http://www.acleddata.com) and are described in Raleigh et al. (2010). The data are employed in several economics and political science studies (see, e.g., Minoiu and Sehmyakina 2014) and we are aware of two other economic studies that employ DRC, ACLED data in empirical analysis. Maystadt et al. (2014) study the relationship between conflict and mineral prices during 1997-2007, and Pellillo (2011) uses the data to study the impact of conflicts on household assets. Maystadt et al. (2014) uncover a complex relationship between mining starts and violence that depends on the spatial scale considered. Pellillo (2011) finds negative effects of violence near villages on the accumulation of assets in villages.



The ACLED data currently cover 1997-2013 for the DRC, but our analysis focuses on 2004-2012. We begin the analysis in 2004, rather than earlier, to exclude the 1997 to 2003 period of the Second Congo War (see Stearns 2011). We end the analysis in 2012, rather than later, because we are interested in measuring the short-run impacts of the conflict mineral policies.

The unit of analysis in ACLED is an event occurring on a specific date at a specific location (longitude and latitude). The ACLED data are based on media reports and there is almost certainly measurement error in reported conflicts. As we argue below, the error would have to occur in a very particular way, both spatially and temporally, to undermine our overall conclusions. Conflicts lasting multiple days are recorded as separate “atomic” incidents. This fact is important to consider when interpreting the data.

From the ACLED data we construct two dependent variables that coincide with outcomes in the theory. We construct the first, “looting”, from verbal descriptions of each conflict. The looting variable equals one if an armed militia group’s actions are described by the words ‘loot’, ‘pillage’, ‘plunder’, ‘rob’, ‘steal’, ‘ransack’, ‘sack’, or ‘seize.’ There are 367 of 4218 non-protest/riot DRC events over 2004-2012 described with these words. The second dependent variable, “battles” comes directly from ACLED coding. Battle events are between militia groups, including the Congolese army. Forty-six percent of the DRC events are coded as battles.<sup>18</sup>

In addition to looting and battles, we employ two auxiliary dependent variables to help interpret the main findings and to examine possible channels. These are “violence against civilians” and “militia recruiting.” Violence against civilians events come directly from ACLED coding and are perpetrated by militia groups. Based on text descriptions, violence against civilian events are sometimes coupled with looting, but often they are simply described as “attacks”, “kidnappings”, and “rapes.” Forty-one percent of the 4218 ACLED DRC events are coded as violence against civilians.<sup>19</sup>

Because the raw ACLED data are available in a highly disaggregated form, we must choose if and how to aggregate the data spatially and temporally prior to analysis. In all cases we are focusing on the eastern DRC, rather than the full country. Spatially, our choice is to pick

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<sup>18</sup> Examples of “looting” events include: “Mayi Mayi Militia (DRC) attacked the mining quarry at Bibolo to loot”, “FDLR attacked and looted the Mikweti village in the Walikale locality”, and “Mayi Mayi Militia (DR Congo) looted villages in the Beni area, taking food, goats etc.” Examples of battle events include “Mayi Mayi Militia clashed with FARDC in Kisele”, and “FARDC launched an offensive against FDLR in Nduma.”

<sup>19</sup> The ACLED data provide some information on the number of fatalities per event, but the true number of fatalities is often unknown or unreported, rendering the quantitative information unreliable. For this reason, we analyze the number of conflict events and indicator variables for conflict rather than analyzing fatalities.

between using administrative units (e.g., provinces, districts, territories, etc.) or imposing a spatial grid using GIS software and conducting analysis of cells within that grid. Out of these two options, we choose to analyze administrative units rather than cells of arbitrary size. Using administrative units seems preferable because these units demarcate topographical boundaries that could segregate mining and agricultural sites, and to follow precedent in the empirical literature on DRC conflict (Maystadt et al 2014).

The administrative unit choices are the 5 provinces, the 12 districts, the 70 territories, or an unknown number of villages. Village level analysis is not feasible for our purposes because, to our knowledge, there is not a comprehensive mapping of villages in the DRC. Of the feasible options, we chose the smallest sized administrative unit, which is the territory. Territory-level aggregation matches well the scale of our rainfall data, which are provided in gridded units that are similar in size to the average sized territory.<sup>20</sup> Because territories are not an obvious size match with the agricultural and mining sites considered in the theory, we provide robustness checks using much smaller gridded cells and find similar results (see online appendix 4).

Temporally, we aggregate to the monthly level, because our mineral price data can be observed at the monthly (but not daily or weekly) level, and because other researchers have also employed monthly observations (Maystadt et al. 2014). Our main econometric models employ monthly, territory-level conflict data over 2004-2012, which are summarized in Table 2.

### *B. Policy Variables*

In the main analysis, we employ a single ‘policy indicator’ variable that assigns ‘treatment’ over time and across the territories most directly affected by the mining policies. Although the use of a single indicator variable forgoes details about the timing of different policies (e.g., the passage of Dodd Frank in July 2010, the mining ban in September 2010, the EITC boycott in April 2011), this choice has advantages. Importantly, it may be inappropriate to consider the policies following-up on Dodd Frank as separate, independent events when the passage of Dodd Frank possibly triggered the subsequent policies (see section 2). With this caveat in mind, we test for separate policy effects in some specifications.

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<sup>20</sup> The mean size of territories in the eastern DRC is 17,912 km<sup>2</sup> (6,916 miles<sup>2</sup>), which is similar in area to the smallest U.S. states or the largest U.S. counties.

We choose July 2010 as the time in which Dodd Frank ‘treatment’ began, although formal regulatory authority of Section 1502 was not exercised until later. Our choice of July 2010 is reasonable for two reasons. First, Section 1502 may have shortened planning horizons over mine control immediately, causing armed groups to react before specific regulations were written.<sup>21</sup> Second, observers argued that Dodd Frank was causing a de facto boycott of 3Ts shortly after it was passed, before the more official boycott began in April 2011 (Pöyhönen et. al 2010, Seay 2012). For the spatial dimension of policy treatment, we designate as ‘treated’ the union of territories for which mining was banned (i.e., all territories in Maniema, North Kivu, and South Kivu) and territories containing at least one mine with geo-coordinates within the Section 1502 map of conflict mining zones discussed above.

There are 27 ‘policy territories’, which are illustrated as the shaded territories in maps 3 and 4. Seven of these territories had one or fewer mines, according to where the IPIS longitude and latitude coordinates for each mine fall within our shapefiles of territory boundaries.<sup>22</sup> Map 4 codes these territories as “non-mining” territories for descriptive purposes. Twenty policy territories had more than one mine. For descriptive purposes, Map 4 labels five of these territories as “3Ts dominated” and seven as “gold dominated.” The criteria for being “3Ts dominated” is twofold: the territory must have more than the mean number of 3T mines found in policy territories (which is 7.44), and the number of 3T mines must be at least three times the number of gold mines. Similarly, a territory is gold dominated if it has more than the mean number of gold mines (which is 10.44) and the number of gold mines is at least three times greater than the number of 3T mines. These labeling criteria are subjective, and do not account for variation in the size of mines because systematic data on size are not available.

Given our temporal and spatial designation of treatment, the policy indicator takes a non-zero value for 810 observations in our main regressions. (There are 27 policy territories times 30 months from July 2010-December 2012). The policy indicator variable equals 0.33 during July 2010, because Dodd Frank was not passed until July 21, 2010.

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<sup>21</sup> Section 1502 was added to Dodd-Frank late in the legislation process, in late May 2010, so a treatment date preceding July 2010 by more than a month or two would not be appropriate (see Woody 2012).

<sup>22</sup> Our shape files of territory boundaries come from [www.gadm.org/about](http://www.gadm.org/about). In some cases, the boundaries mapped by IPIS differ from boundaries in maps 3 and 4 (see [www.ipisresearch.be/maps/MiMiKi/Areas/web/index.htm](http://www.ipisresearch.be/maps/MiMiKi/Areas/web/index.htm)). We resolve discrepancies by locating the mine’s territory based on the shapefiles from [www.gadm.org/about](http://www.gadm.org/about).

### *C. Mineral Prices*

The mineral price data come from MetalPrices.com, which requires a subscription for historical data. From their website we have downloaded data on the world prices of gold, tin, tantalum, and tungsten. The gold prices are reported in dollars per troy ounce. The prices of 3Ts are reported in dollars per pound. Online appendix 1 shows the monthly averages for 2004 through 2012, which are CPI adjusted and reported in U.S. dollars.<sup>23</sup>

### *D. Rainfall Seasons and Shocks*

In the theory, positive wage shocks in agriculture raise the incentives for armed groups to loot. To proxy exogenous shocks, we use two sets of rainfall measures. First, we follow Maystadt et al. (2014) by constructing a measure of territory-level rainfall anomalies. Like Maystadt et al., we assume that abnormally high – but not excessively high – quantities of rainfall act as positive shocks in the unirrigated agricultural regions of the eastern DRC. To construct the rainfall measure, we first downloaded precipitation data from Global Precipitation Climatology Center’s (GPCC) website and then converted the monthly, spatially gridded data to territory-level averages using a process we describe in online appendix 2. Next, we calculated rainfall anomalies for each territory-month observation. Anomalies are the difference between rainfall during the specific month and the territory’s 1951-2012 mean for that month. We divided the difference by the standard deviation in rainfall over 1951-2012 to standardize. The resulting variable has a mean of 0.035 and ranges from -2.69 to 3.61 (see Table 2).

To account for the possibility that rainfall seasons are also important determinants of conflict, we constructed indicator variables for wet and dry season patterns within each territory. We identify the driest and wettest three months in each territory based on long run precipitation averages. The ‘wet season indicator’ equals one for territory  $i$  in a particular month if the long-run average precipitation for that month ranks among the highest three. Similarly, the ‘dry season indicator’ is equal to one for territory  $i$  in a particular month if the long-run average precipitation for that month ranks among the lowest three.

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<sup>23</sup> We lack detailed data on the time variation in prices received locally, which differ from world prices, especially after Dodd Frank according to on-the-ground anecdotes (see, e.g., Carish 2012).

## 5. Empirical Analysis

Our theory hypothesizes the mining policies triggered an increase in looting within the policy territories, both near mines and in the surrounding non-mining, agricultural territories. We also hypothesize the mining policies triggered battles near gold sites but not near 3T sites. We provide statistical tests in this section, after first presenting graphical evidence.

### A. *Graphical Evidence*

Figure 4 compares patterns of conflict inside versus outside the policy territories depicted in map 3. Focusing first on Panel A, we see that looting events in the 27 policy territories began to rise sharply at the end of 2010, after the passage of Dodd Frank, and stayed high through much of 2011 and 2012. As Panel B indicates, there was not a commensurate rise in looting in the 43 non-policy territories, despite similar pre-Dodd Frank trends across the two regions.<sup>24</sup> In Panel D, we see that the number of battles increased in the policy territories after Dodd Frank was passed, at least relative to the number of battles in the non-policy territories post-Dodd Frank. The incidence of violence against civilians also increased in the policy regions after Dodd Frank, and there was not a commensurate rise in the non-policy regions (Panels E and F).

Figure 5 shows visual evidence on the location and timing of conflict events that is consistent with theoretical predictions. The plots show conflicts per territory over a narrower time window, from 2008-2011, in order to give better perspective on the timing of conflict. Panels C and D separately plot conflict in the mining and non-mining territories defined in map 4. Panel C indicates that mining and non-mining territories had similar levels of looting prior to Dodd Frank and looting was following similar - albeit erratic - trajectories during 2008 to July 2010 in both sets of territories. Looting increased after the passage of Dodd Frank in both sets of territories, and the increase in the mining territories preceded the increase in the non-mining territories. Panel E shows that looting increased in territories specializing in both gold and 3Ts. Panel F shows that battles were infrequent in both types of territories during mid-2009 until early 2011, when battles increased sharply in the gold mining but not 3T mining territories.

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<sup>24</sup> If Dodd Frank policies caused spatial spillover of looting into the non-policy territories, then these comparisons are biased downward and understate the true effect of the policies. We address this issue in a robustness check.

### B. Econometric Model

To implement formal tests, we employ panel regression analysis. Our least restrictive model is of the form in (14), but we begin by estimating a baseline regression in which all coefficients except  $\delta_i$ ,  $\mu_t$ ,  $\beta_1$ ,  $\beta_2$ ,  $\beta_3$  are zero. We sequentially relax the restrictions and allow the other coefficients to be non-zero.

$$\begin{aligned}
 & conflict_{itk} = \delta_i + \mu_t + \omega_i t + \beta_1 policy_{it} + \beta_2 (policy_{it} \times 3T_i) + \beta_3 (policy_{it} \times gold_i) \\
 (14) \quad & + \sum_{m=1}^4 \lambda_m (mine_{i,m} \times price_{t,m}) + \sum_{k=1}^2 \gamma_k season_i + \sum_{x=0}^2 \eta_x rain_{i,t-x} + \sum_{x=1}^3 \alpha_x conflict_{i,t-x} \\
 & + \sum_{q=0}^1 \phi_q adj. conflict_{i,t-q} + \varepsilon_{itk}
 \end{aligned}$$

Here  $i$  denotes the 70 territories,  $t$  denotes the 108 months spanning 2004-2012, and  $k$  denotes the season (dry, wet, or neither). The notation  $\delta_i$  represents the 70 territory fixed effects and  $\mu_t$  represents the 108 time effects. The term  $\omega_i t$  denotes individual linear time trends for each territory. The territory fixed effects control for factors that are relatively time invariant, and known to be important determinants of conflict, such as ethnic composition, fractionalization, and geography (Esteban et al. 2012). The time period effects control for eastern DRC-wide factors that may cause changes in conflict, such as presidential elections or changes in national inflation. The territory-specific time trends control for the possibility that conflict in a territory was already trending up or down prior to Dodd Frank.

The coefficients of interest are  $\beta_1$ ,  $\beta_2$ , and  $\beta_3$ . The coefficient  $\beta_1$  measures the effects of the policy in territories lacking mines, and  $\beta_2$  and  $\beta_3$  measure the effects of the policy interacted with the number of 3T and gold mines respectively (see map 3). Our theoretical framework implies  $\beta_1 > 0$  in the looting regressions, and  $\beta_3 > 0$  in the battle regressions.<sup>25</sup>

The coefficients  $\lambda_m$  measure the relationships between conflict and monthly world mineral prices for  $m$  indexing tin, tungsten, tantalum, and gold, each interacted with indicators for the presence of each type of mine in a territory. The coefficients  $\gamma_k$  measure seasonal

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<sup>25</sup> In making these predictions, we are assuming the number of mines in a territory prior to Dodd Frank appropriately measures mineral endowments. We are also assuming that conflict linearly relates to the number of mines. In table 4, we show the results are robust to interacting the policy indicator with the log of the number of mines.

patterns in conflict with territory specific indicators. The  $\eta_x$  coefficients measure the effects of contemporaneous and lagged rainfall anomalies on conflict.<sup>26</sup>

Finally, to check the robustness of the  $\beta$  coefficients to controls for the local persistence of conflict, we include lags for the number of conflicts in previous months in some specifications.<sup>27</sup> We also control for conflicts in adjacent territories, measured as the aggregate number of conflicts in all adjacent territories, to control for potential spatial spillover. In the context of regression equation (14), we allow  $\alpha_x$  and  $\phi_q$  to be non-zero. We include lags until they are no longer statistically significant in any specification that we have tried, which occurs at three months for within-territory conflict and one month lags for adjacent territory conflict.<sup>28</sup> We follow precedent from other panel data studies of conflict by using linear fixed effects estimators. In all estimates, we cluster standard errors at the territory level to account for possible serial correlation within territories (Bertrand et al. 2004).

### C. Main Results

Table 3 provides the main set of estimates. The dependent variable in columns 1-6 is the looting indicator, and the dependent variable in columns 7-12 is the battle indicator. The even numbered columns include territory-specific time trends. Columns 1-2 and 7-8 are the baseline results with no additional covariates. Columns 3-4 and 9-10 add the mineral price and rainfall variables. Columns 5-6 and 11-12 introduce the lagged and adjacent conflict variables.

In the looting regressions in table 3, the policy coefficient estimates,  $\hat{\beta}_1$ , are all positive, statistically significant, and fairly stable in magnitude across specifications. The coefficients on

<sup>26</sup> Many papers have uncovered relationships between resource prices, climate shocks, and conflict (see Collier and Hoeffler 2004, Miguel et al. 2004, Miguel 2005, Angrist and Kugler 2008, Brückner and Ciccone 2010, Bohlken and Sergenti 2011, Hsiang et al. 2011, Dube and Vargas 2013, Maystadt et al. 2014, Sanchez de la Sierra 2014).

<sup>27</sup> These lags measure conflict of all types in the ACLED data to allow for the possibility that a past battle may affect future looting and vice versa. The lagged conflict may be particularly important when the dependent variable is an ACLED monthly count of conflict episodes because extended conflicts are measured as separate incidents for each day they persist. This kind of structural autocorrelation is less of a problem when the dependent variable is an indicator variable for whether or not a conflict occurred during a given month.

<sup>28</sup> We introduce the dynamic and spatial lags mainly to test for robustness of the  $\beta$  coefficients, rather than due to inherent interest in the  $\alpha$  and  $\phi$ , but we recognize the potential biases that adding these variables create. The dynamic lags introduce the Nickell Bias (Nickell 1981). With positive serial correlation, our estimates of  $\alpha$  understate the true persistence of conflict by a factor that decreases with  $T$ , the number of time periods. Because  $T$  is relatively large in our case, at 108, this bias is likely small. Including the adjacent territory variable potentially introduces a simultaneity or reflection problem (Anselin 2002). If conflict among neighbors is positively correlated, our estimates will overstate the true  $\phi$  coefficients. Inclusion of the adjacent territory variable could also bias downward our estimates of the  $\beta$  coefficients if the policy indicator is positively correlated with adjacent conflict.

the interaction terms,  $\hat{\beta}_2$  and  $\hat{\beta}_3$ , are not statistically different from zero. Hence, the evidence suggests that Dodd-Frank increased the probability of looting in the policy territories, and that this increase was not statistically different across mining and non-mining territories. This result is in line with our theoretical framework, which implies that looting would increase in 3T and gold regions and spill into agricultural regions. For perspective on magnitudes, the mean probability of looting in the policy territories prior to Dodd Frank was 0.030. Hence, the column 3 estimate of 0.053 implies the probability of looting in policy territories increased by 176% after Dodd Frank.

In the table 3 battle regressions, the  $\hat{\beta}_1$  and  $\hat{\beta}_2$  coefficients are not statistically distinct from zero, but the  $\hat{\beta}_3$  estimates are all positive and statistically significant. These coefficients indicate that battle probabilities did not rise in general within the policy territories, but battle probabilities did increase with the number of gold mines in a territory after the passage of Dodd Frank. This result is in line with our theoretical framework, which predicts that Dodd Frank would encourage battles over gold, because the policy raised its value relative to 3T minerals. For perspective on the magnitudes of the  $\hat{\beta}_3$  estimates, the mean probability of a battle prior to Dodd Frank in a policy territory with at least one gold mine was 0.126. The number of gold mines in the policy territories ranged from 0 to 69 with a mean of 15.7, conditional on having at least one gold mine. Hence, the column 10 coefficient of 0.003 means the probability of battle increased by  $15.7 \times 0.003 = 0.0471$  in a policy territory with the mean number of gold mines. This is a 37% increase relative to the pre-Dodd Frank mean of 0.126.

Turning to the covariates in table 3, which are not our focus, we note the following patterns. First, the coefficients on the price-mine interaction terms,  $\hat{\lambda}_m$ , are generally insignificant, and they are sensitive to the inclusion of time trends. This sensitivity may be due to the fact that mineral prices, especially gold, trended in systematic ways during much of 2004-2012, making it difficult to separately identify time trends from price effects. In any case, we are reluctant to draw conclusions from the price coefficients, because local prices reportedly deviated sharply from the world prices during our period of study.

Second, the lagged rainfall anomalies positively relate to the probability of looting in the specifications with time trends, which we believe are the most rigorous and reliable. These results are arguably consistent with the theoretical framework, which assumes that agricultural



surpluses are lootable and increase with rainfall.<sup>29</sup> Assuming the surpluses are harvested in time periods after favorable rain, we expect looting to increase in the aftermath, as the lagged rainfall coefficients indicate.

To summarize the results in tables 3, Dodd-Frank appears to have caused a rise in the probability of looting across the entire targeted policy area. Dodd Frank also appears to have caused a rise in the probability of battles in gold mining territories.

#### *D. Robustness of Main Results*

Table 4 shows a series robustness checks. Panel B includes the same set of regression specifications, but here the dependent variables measure the number of looting and battle incidents. We do not focus on these outcomes in the main results because the theory describes the probability of conflict, rather than the intensity and duration of conflict. Nonetheless, we see that, in the looting regressions,  $\hat{\beta}_1$ ,  $\hat{\beta}_2$ , and  $\hat{\beta}_3$  follow the same pattern as those in table 3. For perspective on magnitudes, the mean number of looting incidents in the policy territories was 0.047 prior to Dodd Frank. Hence, the column 6 coefficient of 0.156 means that looting increased by 332%. The battle coefficients of  $\hat{\beta}_1$  and  $\hat{\beta}_3$  in columns 7-12 follow the same general pattern as those in table 3, but are less robust. The column 11 coefficient, for example, suggests that battles increased by  $15.7 \times 0.007 = 0.109$  in the policy territory with the mean number of gold mines. This is a 29% increase relative to the pre-Dodd Frank mean of 0.374 in the territories with gold mines. Comparing the battle coefficients of  $\hat{\beta}_3$  across panel A and B suggests Dodd Frank had a more systematic effect on battle probabilities in gold mining territories, when compared to its effect on the number of battles.

Panels C through G show the results are generally robust to changes in the sample territories and to changes in covariates employed. Panel C drops urban territories from the analysis because of possible urban reporting bias in the ACLED data (Eck 2012). Panel D omits from the sample the 17 territories that are outside the policy region but directly adjacent to at least one policy territory. Omitting these policy-border territories controls for the possibility that

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<sup>29</sup> The marginal effect of rainfall anomalies on looting is declining as the magnitude of rainfall anomalies increase, as indicated by the negative sign on the rainfall anomaly squared variable. This is not surprising. Like Maystadt et al. (2014), we suspect that above average rainfall increases agricultural earnings in the eastern DRC but flooding does the opposite.

our main estimates are confounded by the spillover of conflict into the neighboring non-policy areas. Panel E omits observations from 2012. The panel E specifications focus on the very short-run impacts of Dodd Frank, and control for the 2012 M23 rebellion that might confound the estimates if M23 activity concentrated in policy territories. Panel F interacts the policy indicator with the logged number of mines, to account for the possibility that policy effects are non-linearly related to mines.<sup>30</sup> Panel G includes lags of the dependent variable on the right hand side rather than lags for the number of conflicts of all types. As table 4 indicates, our main findings generally hold across these robustness checks.

Panel H of table 4 tests for robustness to an alternative econometric model. The alternative adds two interaction variables to (14):  $\zeta_1(post-policy_i \times 3T_i \times non-policy\ territory_i)$  and  $\zeta_2(post-policy_i \times gold_i \times non-policy\ territory_i)$ . With this expansion of the model, the  $\beta_2$  and  $\beta_3$  policy coefficients of interest are identified from differences in conflict relative to non-policy territories with similar numbers of mines. The drawback of this model is that heavily endowed territories outside the policy targeted zone are not clean counterfactuals for heavily endowed territories inside the zone, if Dodd Frank caused battles for their control. This is plausible for 3Ts because Dodd Frank made 3Ts from outside the zone relatively more valuable. And, although Dodd Frank probably did not raise the price of gold from outside the zone relative to gold from inside, it is still plausible that Dodd Frank caused battles for gold outside the zone. If Dodd Frank increased within-zone competition for gold, as our theory implies, this competition may have pushed militias towards heavily endowed gold territories outside the zone.

Turning to the results, as a comparison of panels A and H indicates, there is suggestive evidence that Dodd Frank did cause battles to increase in heavily endowed mining territories outside the policy targeted zone. In panel H, the coefficients on the interaction between 3T mines and the policy indicator ( $\hat{\beta}_2$ ) are negative and statistically significant, whereas they are effectively zero in panel A. Mechanically, specifying a counterfactual with rising battles (e.g., non-policy territories with several 3T mines) would negatively affect the size of  $\hat{\beta}_2$ . For the gold mine interactions, the estimates of  $\hat{\beta}_3$  in panel H are sensitive to the inclusion of territory-specific time trends. For our preferred models with time trends, the  $\hat{\beta}_3$  coefficients are smaller in

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<sup>30</sup> For territories with zero mines, we replace the zeroes with 0.1 prior to taking logs.

panel H when compared to panel A. This finding is consistent with battles rising with the number of gold mines in the non-policy territories, because such a rise would negatively affect  $\hat{\beta}_3$ .

To summarize, it is useful to synthesize the findings from the panel H model with those of the main, Panel A model. Overall, the results suggest that Dodd Frank shifted battles away from regulated minerals (i.e., 3Ts in policy territories) and towards unregulated minerals. The unregulated minerals include gold in the policy territories, 3Ts in the non-policy territories, and gold in the non-policy territories.

Online appendices 3 and 4 show two additional robustness checks. Table A1 in appendix 3 shows the findings pass placebo tests that assign false policy passage dates, in July 2009 and July 2008, rather than the actual passage of Dodd Frank in July 2010. We also assign a January 2009 placebo date to correspond with the beginning of the Kimia II offensive, which was a coalition between competing armed groups in the eastern DRC (see Sanchez de la Sierra 2014). These null placebo results increase our confidence that the key results are not simply driven by pre-existing trends in territory-specific conflict that are perhaps not well accounted for by the linear, territory specific time trends.

In appendix 4, we analyze conflict across gridded cells that are 700 km<sup>2</sup> (which is much smaller than the averaged sized territory). In general, the disaggregated analysis produces results consistent with the territory level analysis. The consistent results are important because the theoretical model is silent about how far militias will travel to obtain revenue.

#### E. *Separate Effects of Dodd Frank and the Mining Ban*

In this section we present a version of the model that allows the effects of Dodd Frank to differ from the effects of the mining ban. The econometric model is given below. Our primary interest is in  $\nu_1$  and  $\nu_2$ . To simplify interpretation, and to allow adequate statistical power, here we do not interact the policy variables with the number of mines.

$$\begin{aligned}
 \text{conflict}_{itk} = & \delta_i + \mu_t + \omega_i t + \nu_1 \text{Policy}_{it} + \nu_2 \text{Ban}_{it} + \sum_{m=1}^4 \lambda_m (\text{mine}_{i,m} \times \text{price}_{t,m}) \\
 (15) \quad & + \sum_{k=1}^2 \gamma_k \text{season}_i + \sum_{x=0}^2 \eta_x \text{rain}_{i,t-x} + \sum_{x=1}^3 \alpha_x \text{conflict}_{i,t-x} + \sum_{q=0}^1 \phi_q \text{adj. conflict}_{i,t-q} + \varepsilon_{itk}
 \end{aligned}$$

In this model,  $\nu_1$  is the average effect of the policy indicator, which includes the time period during which the mining ban was in effect. Hence,  $\nu_2$  tests whether or not patterns of conflict differed during the ban time period.<sup>31</sup> An estimate of zero implies no difference.

Estimates of (15) provide tests of the theoretical model because Dodd Frank and the mining ban had different effects on the incentives of armed groups to engage in looting or battles. The ban declared artisanal mining illegal, whether gold or 3Ts, whereas Dodd Frank acted as a de facto boycott on 3Ts but not on gold. Because Dodd Frank sharply changed the relative value of minerals, we expect it to have increased incentives to battle in the policy targeted territories. Hence, the theory suggests  $\nu_1 > 0$  in the battle regressions. By contrast, the theory suggests an outright ban on mining would have, if anything, diminished the incentives to battle for minerals in policy territories, whether 3Ts or gold. Hence, the theory implies  $\nu_2 < 0$  in the battle regressions.

Table 5 shows results. For the battle regression results in columns 7-12, the  $\hat{\nu}_1$  coefficients are positive and the  $\hat{\nu}_2$  coefficients are negative. These findings suggest the mining ban actually dampened incentives to battle, because it reduced the value of both gold and 3Ts. Once the ban was lifted, the relative value of gold increased due to Dodd Frank's asymmetric regulation of gold and 3Ts, and so did battles in the policy territories.

For the looting regressions in columns 1-6, the policy coefficients,  $\hat{\nu}_1$ , are all positive and significant. The mining ban coefficients,  $\hat{\nu}_2$ , are effectively zero. These results suggest that looting increased by a similar magnitude during and after the ban. One interpretation is as follows: both the mining ban and the de facto boycott (Dodd Frank) displaced militia groups and caused them replace lost revenue by looting. If the policies are considered independent, Dodd Frank appears to have caused an increase in the probability of looting across the policy region ranging from 0.043 to 0.074. Recalling the pre-Dodd Frank mean probability of looting of 0.030, these estimates imply an increase of 143% to 247% in the probability of looting.

Our theory focuses on how the mining policies affected militia decisions to loot and battle, but the policies may have also indirectly increased violence against civilians, if such

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<sup>31</sup> The ban indicator differs from the policy indicator mainly in the temporal dimension, with the ban indicator having a non-zero value during September 2010 to March 2011. However, the ban variable also differs somewhat from policy indicator in spatial coverage, as the policy indicator includes all of the territories for which at least one mine had coordinates within the U.S. State Department's section 1502 map described in section 2.

violence is a byproduct of looting and battles. We conclude this section by assessing violence against civilians, which is an outcome directly coded by ACLED. Table 6 presents results for estimates of the  $\beta$  coefficients in (14), and for the  $\nu$  coefficients in (15). In general, the pattern of estimates mimics the patterns in the looting regressions. The point estimate range of 0.052 to 0.156 in columns 1-6 of panel C is relative to a pre-Dodd Frank mean of 0.118. Hence, the probability of a violence against civilians event increased by 44 to 132%. One interpretation of these findings is that Dodd Frank caused increases in violence against civilians primarily because it generated incentives for militias to loot.

#### *F. Limitations and Alternative Interpretations*

To summarize the results in tables 3-6, Dodd-Frank appears to have caused a general rise in looting and violence against civilians that was not specific to territories dominated by mining prior to Dodd Frank, and a targeted rise in battles over gold mining territories. These results are consistent with the theoretical framework suggesting the policies caused militia groups to loot and rove, and to battle for the relatively more valuable gold mining sites.

Some of the results are potentially consistent with alternative theories which include (but are not limited to) those emphasizing a) information asymmetries among militia groups (e.g., Fearon 1995); b) the opportunity cost of militia participation (e.g., Becker 1968, Grossman 1999, Chassangy and Miquel 2009); and c) crime displacement (e.g., Repetto 1976, Barr and Pease 1990, Hesseling 1994, Sherman and Weisburd 1995, Braga 2001, Guerette and Bowers 2009, Draca et al. 2010, Dell 2014, Maystadt 2014, and Kalyvas 2015). We relate our findings to these alternatives in online appendix 5. We have chosen to emphasize the stationary bandits framework because it relies on a clear and simple economic objective on the part of militias (revenue maximization), and because it fits well with popular descriptions of militia groups in the eastern DRC. With that said, we recognize the causes of violence in the eastern DRC are more complex than the theory we present (see, e.g., Austesserre 2012), and we concede that ACLED data are not up to the task of precisely testing each alternative theory against one another. Hence, we make no claim that our analysis rules out all other explanations for the empirical patterns.

Finally, we emphasize two data limitations before concluding. First, there is almost certainly measurement error in the ACLED conflict data. Such errors would have to occur in a

particular way - i.e., improved measurement of battles in gold territories after Dodd Frank and improved measurement of looting in policy regions after Dodd Frank – to undermine our overall conclusions. In general, we would be most concerned about measurement error if we thought ACLED data underreported conflict during 2004 to July 2010, but fully reported conflicts after Dodd Frank. Conflict data collected by other researchers working in the eastern DRC, however, suggests the pattern of ACLED measurement error works against our finding of greater conflict after Dodd Frank.<sup>32</sup> Second, the composition of the control and treatment areas – i.e., the policy and non-policy areas – unlikely remained the same because miners likely relocated across territories for income opportunities. We cannot directly control for this factor because we lack territory-level data on populations. If the policies shifted populations away from 3T areas towards gold and agricultural areas, as we expect, then our coefficients underestimate the per-capita increases of conflict in 3T territories.

## **7. Conclusions**

The top-down decision to regulate “conflict minerals” from the DRC did not reduce violence committed by militias during our period of study. The long term effects remain to be seen, but in the short term, the policy appears to have backfired. Instead of reducing violence, the evidence indicates the policies increased the incidents in which armed groups looted civilians and committed violence against them.

Our results and approach contribute to the literature on the interaction between natural resource prices, endowments, and violence. We join others in concluding that the resource curse, as it pertains to violent conflict, is a complex phenomenon that is unlikely solved by international trade embargoes, boycotts, or certification programs that reduce the value of a country’s resource endowment.<sup>33</sup> Moreover, our findings call into question the widespread use of new forms of resource governance interventions that rely on strong assumptions about how natural resources are linked to conflict motivations (see Cuvelier et al. 2014). Our findings are also a cautionary tale about what can go wrong if an international intervention cuts revenue from natural

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<sup>32</sup> Sanchez de la Sierra (2014) compares ACLED conflict data, aggregated to the annual level, with villager recollections of conflict in the eastern DRC. For the sample of villages surveyed, the ACLED data track villager recollections very closely for 2008 and 2009. For 2010-2011, however, the ACLED data underreport conflicts relative to villager recollections.

<sup>33</sup> Several recent studies cast doubt on the generalization that resources are the cause of violence in conflict ridden countries (see, e.g., Brunnschweiler and Bulte 2009, Arezki et al. 2015).

resources, without anticipating the effects on the incentives of armed groups, and their preferred interactions with civilians.

Methodologically, we highlight the importance of within-country analysis in detecting policy-induced shifts in violence that are difficult to identify with country-wide data (see Dell 2015). In our case, for example, the resource-value shock induced by Dodd Frank shifted battles from territories containing regulated minerals, to those containing unregulated minerals; these within-country patterns are important, but not detectable at more aggregate levels of analysis.

As a secondary contribution, we also offer a theoretical premise – that of stationary and roving bandits - from which to analyze armed civil conflict that we view as complementing other models focused on rapacity, opportunity cost, information asymmetries, and crime displacement. Under the bandit premise, civilians pay taxes to militias in exchange for crude protection. This informal institutional arrangement is not first best, of course, but it may be safer and more economically productive than anarchy (Hirshleifer 1995, Skaperdas 2001, Olson 1993). Policy-makers in rich nations should be aware of this possibility when considering whether or not to intervene in ways that affect informal property rights and warlord incentives in foreign lands.

We recognize the causes of violence in the eastern DRC are more complex than the theory we present (see Austesserre 2012, Stearns 2014), and we hope that future research can better pinpoint the role of mineral trade. We analyze conflict at a spatial scale smaller than cross-country analysis – the territories in the eastern DRC – but a more disaggregated analysis could reveal additional detail about militia activity and movement. We have also focused on the short-run impacts of Dodd Frank, but an examination of the longer run impacts could be fruitful. These issues are beyond the scope of our paper, but we hope they are studied in future research.

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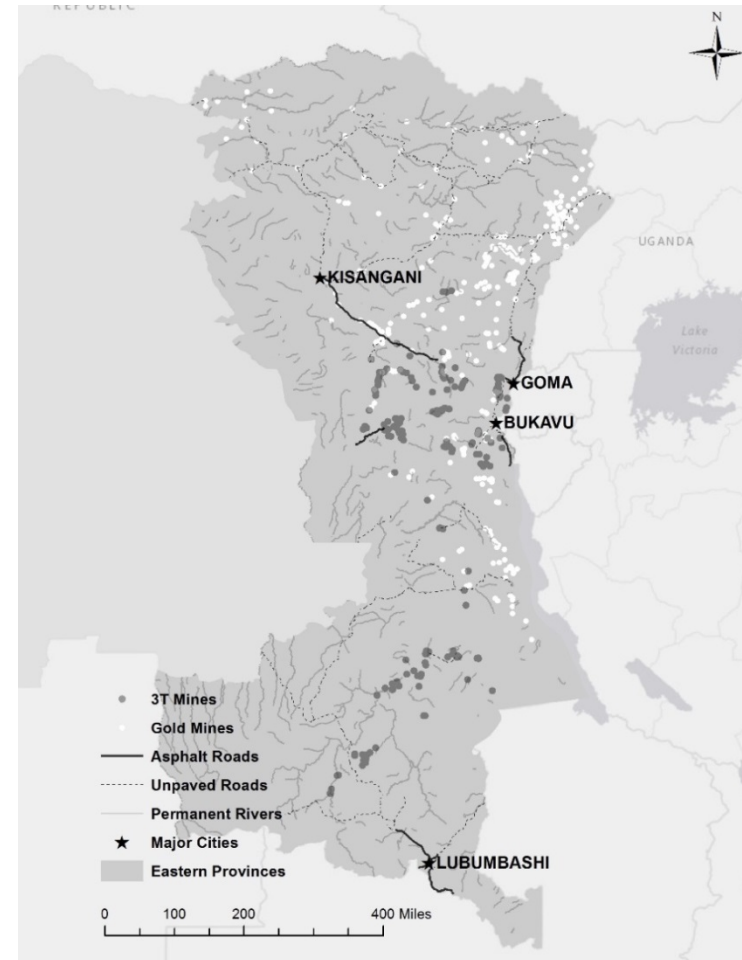
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**Map 1: The Five Eastern Provinces of the DRC**

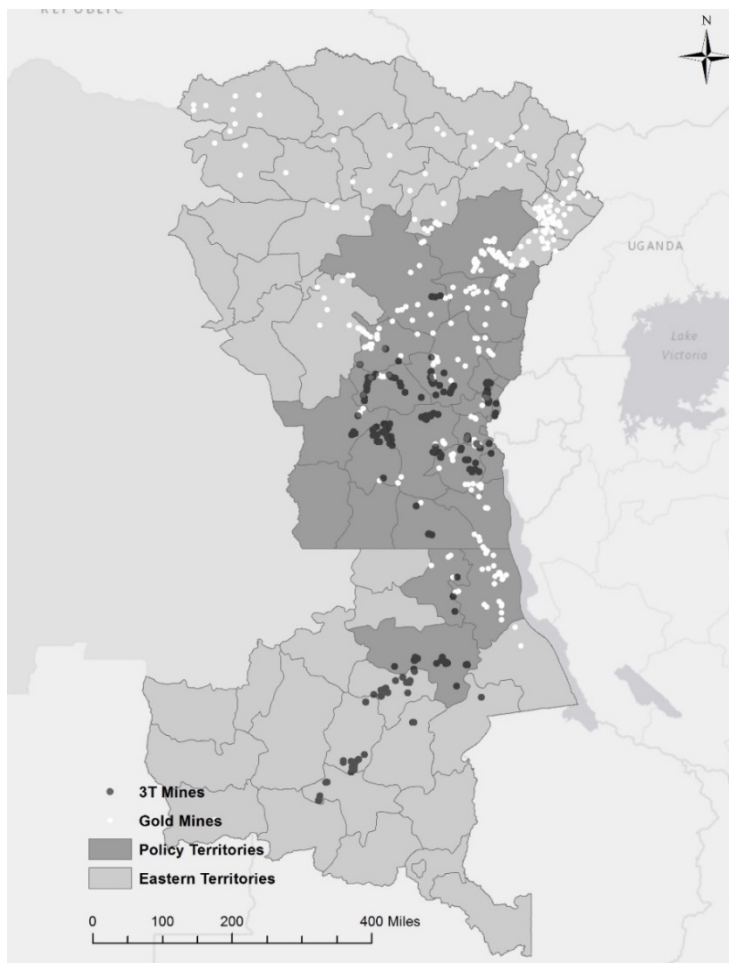


**Map 2: IPIS Mines and Infrastructure in the Eastern DRC**

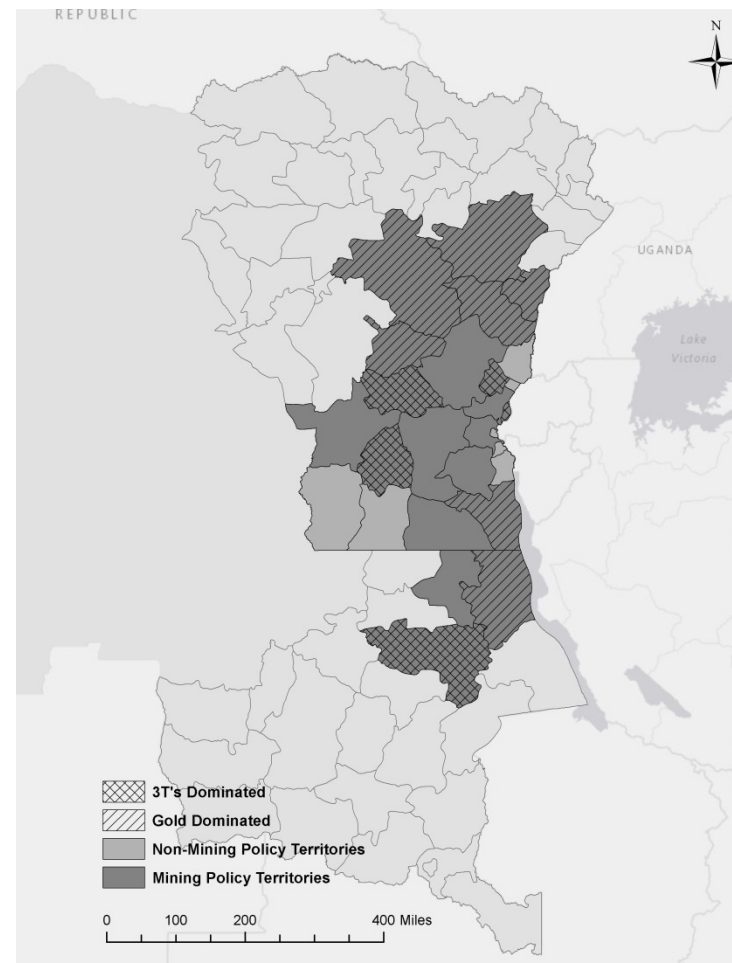


**Notes:** The shapefiles for province boundaries come from [www.gadm.org/about](http://www.gadm.org/about). The GIS data on roads, rivers and major cities come from the USAID GIST Data Repository, available at: <https://gistdata.its.uga.edu/>. The data on mine locations come from a series of International Peace Information Service (IPIS) interactive maps described in detail by Spittaels and Hilgert (2008), Spittaels and Hilgert (2009), Spittaels (2010), and Spittaels and Hilgert (2010). These maps inventoried mining sites over 2008-2010, prior to Dodd Frank. The mines are listed by primary mineral but, in some cases, more than one mineral is mined from a site.

**Map 3: Conflict Mineral Policy Territories and Mines**



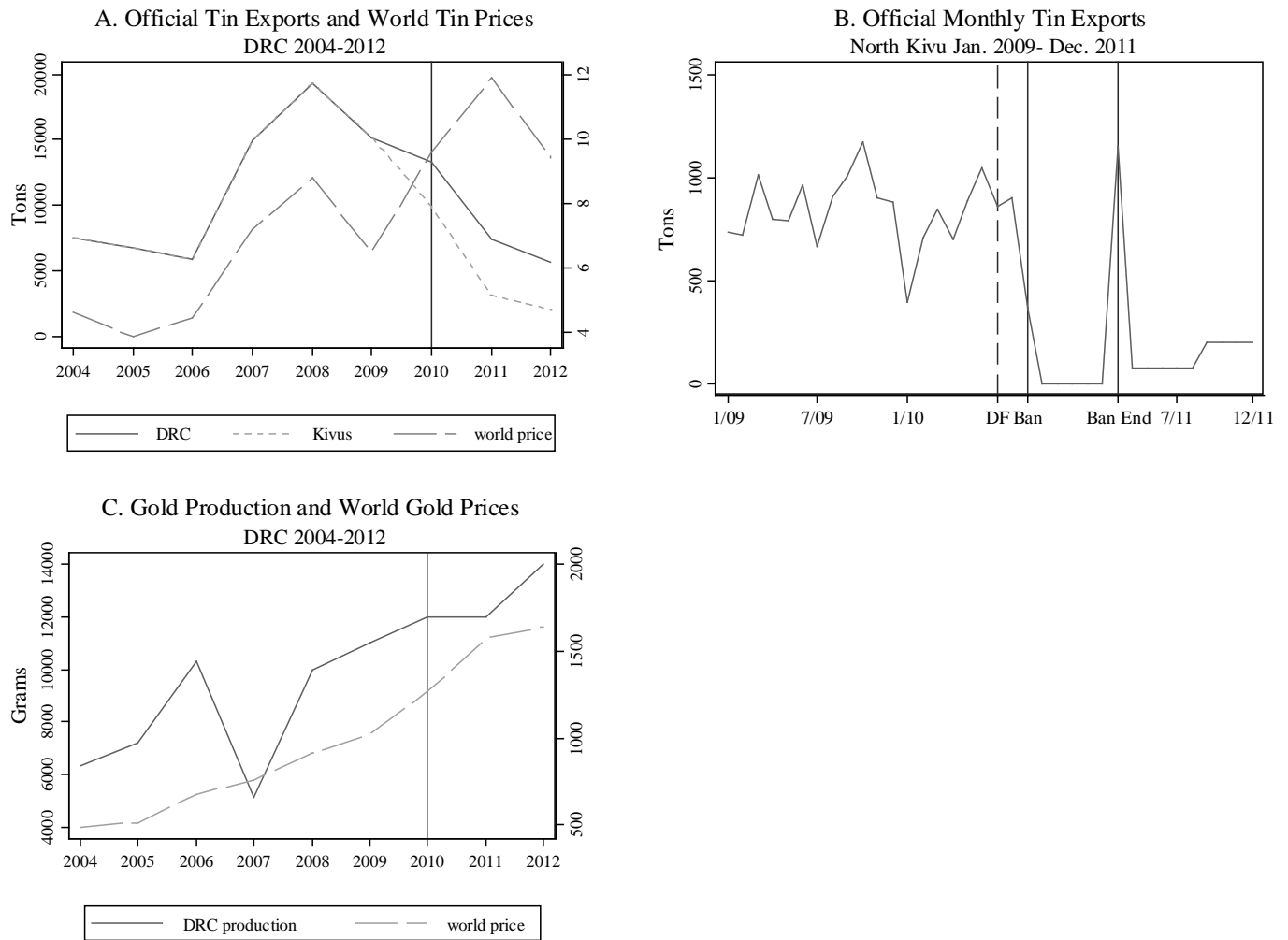
**Map 4: Categories of Policy Territories**



**Notes:** The “Policy Territories” comprise the union of territories in which mining was banned (i.e., all territories in Maniema, North Kivu, and South Kivu) and the territories containing at least one mine with geo-coordinates falling within the U.S. State Department’s Section 1502 map of conflict mining zones. The map is available at: [https://hiu.state.gov/Products/DRC\\_MineralExploitation\\_2011June14\\_HIU\\_U357.pdf](https://hiu.state.gov/Products/DRC_MineralExploitation_2011June14_HIU_U357.pdf). The shapefiles for territory boundaries come from [www.gadm.org/about](http://www.gadm.org/about) and sometimes differ from spatial definitions of territory boundaries found in the IPIS maps. Here the map 4 categorizations are as follows. The seven non-mining policy territories had one or fewer IPIS mines. The 20 mining policy territories had more than one mine, ranging from a minimum of 6 mines to a maximum of 69. A territory is “gold dominated” if the number of gold mines exceeded the mean number of 10.44, and if the number of gold mines is more than triple the number of 3Ts mines. A territory is “3T dominated” if the number of 3Ts mines exceeded the mean number of 7.44, and if the number of 3T mines is more than triple the number of gold mines. There are 7 gold dominated territories and 5 3Ts dominated territories by these definitions.

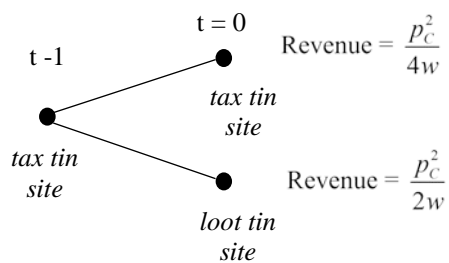


**Figure 1: Official Exports of Tin and Estimated Production of Gold**

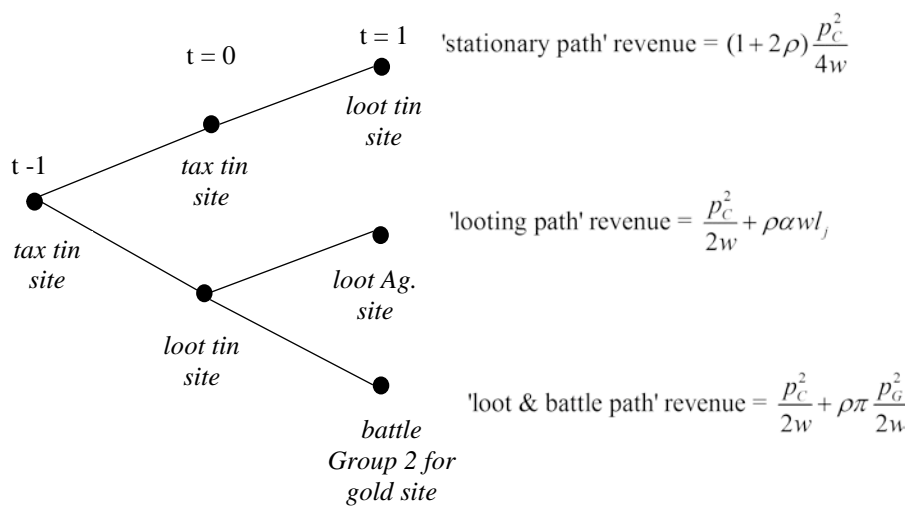


**Notes:** The export and production data in panels A and C come from USGS Mineral Yearbooks Reports, 2008-2012, which are available online at <http://minerals.usgs.gov/minerals/pubs/country/2012/myb3-2012-cg.pdf>. The data for mineral prices come from MetalPrices.com, online subscription. The gold price represents the monthly average of the PM spot prices on the London Market Exchange. The tin price is the monthly average of the cash official price paid by buyers on the London Market Exchange. The data for panel B come from Johnson (2013) and from data posted by Goma's Pole Institute at [www.pole-institute.org/site%20web/echos/echo134.htm](http://www.pole-institute.org/site%20web/echos/echo134.htm), and [www.pole-institute.org/site%20web/echos/echo147.htm](http://www.pole-institute.org/site%20web/echos/echo147.htm), last visited in July 2014.

**Figure 2**  
**Decision Tree for Group 1 with a One-Period Planning Horizon**

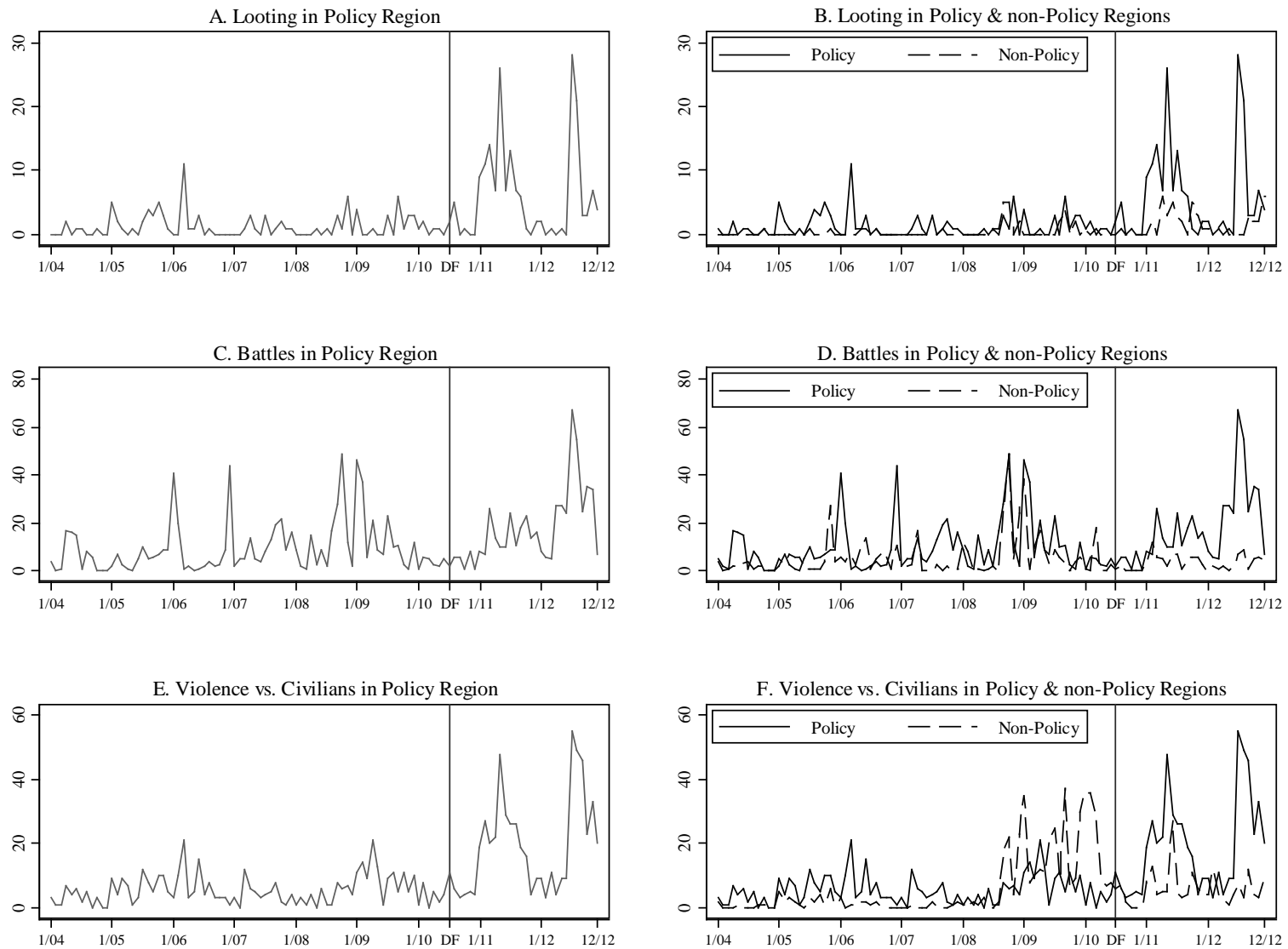


**Figure 3**  
**Decision Tree for Group 1 with a Two Period Planning Horizon**



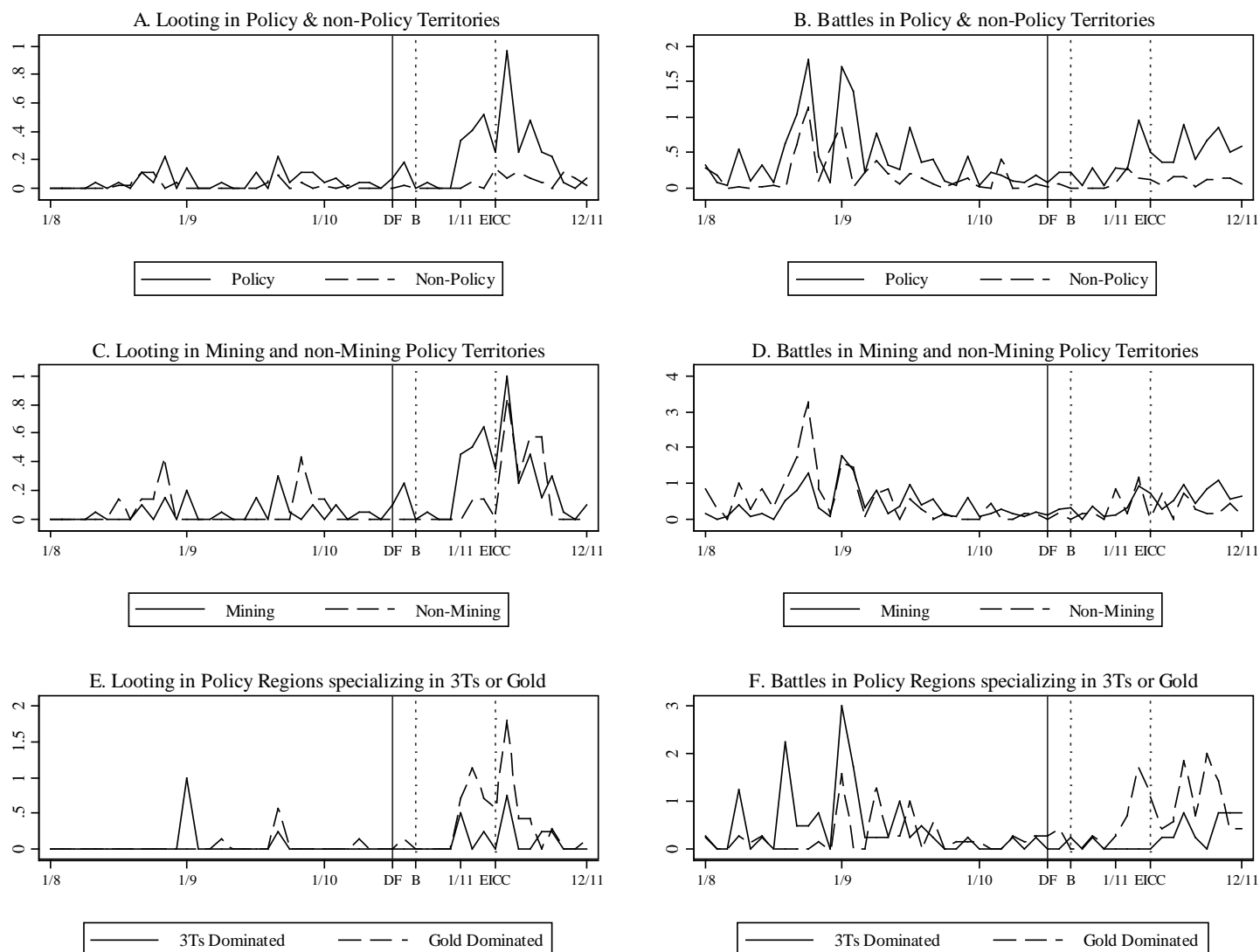


**Figure 4: Monthly Conflict Incidents in the Eastern DRC, 2004-2012**



**Notes:** The source is the ACLED database (Raleigh et al. 2010). The policy regions and outcome variables are defined in section 4. “DF” indicates the passage of Dodd-Frank.

**Figure 5: Per Territory Means of Monthly Conflict Incidents in the Eastern DRC, 2008-2011**



**Notes:** The source is ACLED (Raleigh et al. 2010). The policy regions and outcome variables are defined in section 4. “DF” indicates the passage of Dodd-Frank, “B” indicates the mining ban, and “EICC” indicates the boycott on tin, tungsten, and tantalum from smelters lacking traceability systems. Map 4 shows the gold and 3Ts dominated territories.

**Table 1: OLS Regression Estimates of Deforestation within Radius of Mines Before and After Dodd-Frank**

	100m. radius	200m. radius	500m. radius
Constant	-0.005 (0.869)	-0.077 (0.151)	-0.024 (0.743)
3Ts mine indicator	0.103* (0.098)	0.142 (0.180)	0.127 (0.404)
Indicator for policy territory	0.020 (0.640)	0.078 (0.246)	0.044 (0.626)
Policy territory indicator $\times$ 3Ts mine indicator	-0.209*** (0.006)	-0.292** (0.016)	-0.304* (0.074)
Percent forest loss over 2001-2007	-0.0007 (0.717)	-0.0001 (0.974)	-0.0122*** (0.008)
Observations (mines)	659	659	659
Obs. with y = - 1 (slowing deforestation)	74	163	259
Obs. with y = +1 (accelerating deforestation)	58	120	230
F-Stat	2.78	2.08	3.55
R <sup>2</sup>	0.015	0.012	0.020

**Notes:** \* p<0.10; \*\* p<0.05; \*\*\* p<0.01. *P* values are shown in parentheses. The dependent variable is equal to -1 if deforestation within the radius around the centroid of the mine declined during 2011-2012, relative to 2008-2009. The dependent variable is equal to 1 if deforestation increased. The dependent variable is equal to 0 if there was not a change in the rate of deforestation. The data come from a time-series analysis of satellite images that characterize global forest extent detailed in Hansen et al. (2013). Land area is divided into grid squares of approximately 30 meters on a side at the equator. Forest loss in each grid square is recorded as a binary event for each year, with the initial satellite image from 2000 taken as the base year. Grid boxes that fall partially within the relevant mine radius are considered to be entirely contained.

**Table 2: Summary Statistics**  
(month-territory observations for 2004-2012 in territories of five eastern provinces)

<i>Variable</i>	<i>Mean</i>	<i>Std. Dev.</i>	<i>Min</i>	<i>Max</i>	<i>Description</i>
<i>Time Variant</i>					
Looting <sup>a</sup>	0.048	0.400	0	13	# of events described with text of loot, pillage, plunder, rob, steal, ransack, or seize
Battles <sup>a</sup>	0.242	1.351	0	36	# of battles between armed groups events
Civilians <sup>a</sup>	0.217	1.133	0	28	# of violence against civilians events
Looting indicator <sup>a</sup>	0.027	0.163	0	1	=1 if there was at least one looting event, otherwise =0
Battle indicator <sup>a</sup>	0.087	0.282	0	1	=1 if there was at least one battle event, otherwise =0
Civilians indicator <sup>a</sup>	0.092	0.289	0	1	=1 if there was at least one violence against civilian event, otherwise =0
Policy indicator <sup>b</sup>	0.105	0.305	0	1	=1 starting in July 2010, for union of Section 1502 and mining ban territories,=0 otherwise
Mining ban <sup>c</sup>	0.017	0.126	0	1	=1 for territories in N. Kivu, S. Kivu, and Maniema during Sept. 2010-March 2011, =0 otherwise
Gold price <sup>d</sup>	1.950	0.815	0.91	3.49	World price of gold, normalized at 1 based on the January 2004 price, U.S. CPI adjusted
Tin price <sup>d</sup>	2.054	0.793	0.89	4.12	World price of tin, normalized at 1 based on the January 2004 price, U.S. CPI adjusted
Tantalum price <sup>d</sup>	1.422	0.699	0.81	2.83	World price of tantalum, normalized at 1 based on the January 2004 price, U.S. CPI adj.
Tungsten price <sup>d</sup>	3.475	0.953	0.99	5.13	World price of tungsten, normalized at 1 based on the January 2004 price, U.S. CPI adj.
Rainfall anomalies <sup>e</sup>	0.035	1.00	-2.69	3.61	Difference in rainfall and 1951-2012 average for month, divided by st. deviation
Adj. conflicts <sup>a</sup>	2.671	7.585	0	139	The sum of the # of conflict events (of any type excluding riots/protests) in all adjacent territories
<i>Time Invariant</i>					
Dry Season <sup>e</sup>	0.250	0.433	0	1	=1 for the three driest months in each territory, based on 1951-2012 precipitation averages
Wet Season <sup>e</sup>	0.250	0.433	0	1	=1 for the three wettest months in each territory, based on 1951-2012 precipitation averages
Gold mines <sup>f</sup>	5.786	11.03	0	69	# of gold mining sites or deposits
Cassiterite mines <sup>f</sup>	2.914	6.372	0	33	# of cassiterite mining sites or deposits
Coltan mines <sup>f</sup>	0.314	1.214	0	9	# of coltan (tantalum) mining sites or deposits
Wolframite mines <sup>f</sup>	0.114	0.622	0	5	# of wolframite (tungsten) mining sites or deposits

**Notes:** N=7560 for all variables, with 8 years, 12 months and 70 territories of observations. a) The source is the ACLED database. b) Takes a fractional value of 1/3 for July, 2010 because Dodd Frank was passed on July 21. For purposes here, 'Section 1502 territories' refers to territories on the U.S. State Department's Section 1502 map, which has been declassified and is available at: [https://hiu.state.gov/Products/DRC\\_MineralExploitation\\_2011June14\\_HIU\\_U357.pdf](https://hiu.state.gov/Products/DRC_MineralExploitation_2011June14_HIU_U357.pdf). c) takes value of 2/3 for September 2010 and 1/3 for March 2011. d) The source is MetalPrices.com. e) The source is GPCC at <http://kunden.dwd.de/GPCC/Visualizer>. f) The source is the IPIS maps described in Spittaels and Hilgert (2008), Spittaels and Hilgert (2009), Spittaels (2010), and Spittaels and Hilgert (2010).

**Table 3: Fixed Effects Estimates of Monthly Conflict Indicators**  
in territories of Five Eastern Provinces (2004-2012)

	<i>Y= Looting Indicator</i>						<i>Y= Battle Indicator</i>					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Policy indicator	0.050 <sup>*</sup> (0.063)	0.071 <sup>**</sup> (0.017)	0.053 <sup>**</sup> (0.043)	0.073 <sup>**</sup> (0.017)	0.041 <sup>*</sup> (0.089)	0.064 <sup>**</sup> (0.027)	0.058 (0.109)	0.036 (0.270)	0.052 (0.176)	0.041 (0.194)	0.022 (0.542)	0.013 (0.617)
Policy ind. $\times$ No. of 3T mines	-0.000 (0.928)	-0.002 (0.279)	-0.000 (0.874)	-0.002 (0.263)	-0.000 (0.848)	-0.002 (0.248)	0.000 (0.892)	-0.002 (0.548)	0.000 (0.963)	-0.002 (0.383)	0.000 (0.975)	-0.002 (0.310)
Policy ind. $\times$ No. of gold mines	0.001 (0.305)	0.001 (0.547)	0.001 (0.477)	0.001 (0.575)	0.001 (0.507)	0.001 (0.651)	0.004 <sup>***</sup> (0.009)	0.002 <sup>*</sup> (0.083)	0.004 <sup>***</sup> (0.005)	0.003 <sup>**</sup> (0.033)	0.004 <sup>***</sup> (0.001)	0.002 <sup>**</sup> (0.038)
Gold price $\times$ gold indicator			0.019 <sup>**</sup>	0.002	0.017 <sup>**</sup>	0.014			-0.000	-0.064	-0.007	-0.029
Tin price $\times$ cassiterite indicator			-0.003	-0.015	-0.001	-0.012			-0.003	-0.034	0.003	-0.027
Tant. price $\times$ coltan indicator			-0.006	0.003	-0.007	0.003			0.022	0.046	0.019	0.046
Tung. price $\times$ wolf. indicator			0.001	0.003	-0.001	0.002			-0.013	-0.034	-0.011	-0.030
Dry season indicator			0.002	0.002	0.000	0.000			0.006	0.007	0.002	0.004
Wet season indicator			-0.003	-0.003	-0.003	-0.003			0.006	0.007	0.006	0.007
Rainfall anomalies			0.000	0.002	-0.001	0.001			-0.004	-0.005	-0.004	-0.006
1 month lag rainfall anomalies			0.004	0.005 <sup>*</sup>	0.003	0.005			-0.000	-0.002	-0.002	-0.003
2 month lag rainfall anomalies			0.003	0.005 <sup>**</sup>	0.003	0.005 <sup>**</sup>			0.007	0.006	0.007	0.005
Rainfall anomalies <sup>2</sup>			0.000	-0.000	0.001	0.000			-0.002	-0.002	-0.002	-0.001
1 month lag rainfall anomalies <sup>2</sup>			-0.003 <sup>**</sup>	-0.003 <sup>***</sup>	-0.003 <sup>*</sup>	-0.003 <sup>**</sup>			-0.000	-0.000	0.001	0.001
2 month lag rainfall anomalies <sup>2</sup>			-0.003	-0.003 <sup>*</sup>	-0.002	-0.003 <sup>*</sup>			-0.003	-0.003	-0.002	-0.002
1 month lagged conflict					0.005 <sup>**</sup>	0.005					0.015 <sup>***</sup>	0.013 <sup>***</sup>
2 month lagged conflict					0.002	0.001					0.007 <sup>***</sup>	0.005 <sup>*</sup>
3 month lagged conflict					0.003	0.002					0.005 <sup>*</sup>	0.002
Adjacent territory conflict					0.002 <sup>**</sup>	0.002 <sup>**</sup>					0.003 <sup>**</sup>	0.003 <sup>**</sup>
1 month lagged adj. terr conflict					-0.000	-0.000					0.000	0.001
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R <sup>2</sup> (within)	0.052	0.083	0.055	0.084	0.067	0.092	0.053	0.099	0.054	0.102	0.086	0.121
Observations	7560	7560	7420	7420	7350	7350	7560	7560	7420	7420	7350	7350

**Notes:** <sup>\*</sup> p<0.10; <sup>\*\*</sup> p<0.05; <sup>\*\*\*</sup> p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

**Table 4: Robustness of Main Empirical Results**  
in territories of Five Eastern Provinces (2004-2012)

	<i>Y= Looting Indicator</i>						<i>Y= Battle Indicator</i>					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<b>A. Baseline</b>												
Policy indicator	0.050*	0.071**	0.053**	0.073**	0.041*	0.064**	0.058	0.036	0.052	0.041	0.022	0.013
Policy ind. $\times$ No. of 3T mines	-0.000	-0.002	-0.000	-0.002	-0.000	-0.002	0.000	-0.002	0.000	-0.002	0.000	-0.002
Policy ind. $\times$ No. of gold mines	0.001	0.001	0.001	0.001	0.001	0.001	0.004***	0.002*	0.004***	0.003**	0.004***	0.002**
R <sup>2</sup> (within)	0.052	0.083	0.055	0.084	0.067	0.092	0.053	0.099	0.054	0.102	0.086	0.121
Observations	7560	7560	7420	7420	7350	7350	7560	7560	7420	7420	7350	7350
<b>B. Y = Number of Incidents</b>												
Policy indicator	0.156**	0.174***	0.176**	0.187***	0.143**	0.156**	0.251**	0.153	0.248*	0.184	0.052	-0.009
Policy ind. $\times$ No. of 3T mines	-0.002	-0.004	-0.001	-0.003	-0.001	-0.003	0.003	-0.006	0.004	-0.001	0.005	-0.000
Policy ind. $\times$ No. of gold mines	0.001	0.001	0.000	0.001	-0.000	0.001	0.009	0.007	0.009	0.011	0.007**	0.007*
R <sup>2</sup> (within)	0.044	0.068	0.047	0.071	0.062	0.081	0.042	0.067	0.043	0.070	0.118	0.128
Observations	7560	7560	7420	7420	7350	7350	7560	7560	7420	7420	7350	7350
<b>C. Omits Urban Territories</b>												
Policy indicator	0.060**	0.070**	0.064**	0.072**	0.049**	0.061**	0.084**	0.045	0.082**	0.046	0.044	0.014
Policy ind. $\times$ No. of 3T mines	-0.000	-0.001	-0.000	-0.002	-0.000	-0.002	-0.001	-0.002	-0.000	-0.002	-0.000	-0.002
Policy ind. $\times$ No. of gold mines	0.001	0.001	0.001	0.001	0.000	0.001	0.004**	0.002*	0.004***	0.003**	0.004***	0.002**
R <sup>2</sup> (within)	0.053	0.083	0.057	0.085	0.069	0.092	0.059	0.105	0.060	0.107	0.093	0.127
Observations	7128	7128	6996	6996	6930	6930	7128	7128	6996	6996	6930	6930
<b>D. Omits Policy Neighbors</b>												
Policy indicator	0.056**	0.075**	0.058**	0.076**	0.045*	0.063**	0.060	0.043	0.061	0.047	0.027	0.015
Policy ind. $\times$ No. of 3T mines	-0.000	-0.002	-0.000	-0.002	-0.000	-0.002	0.000	-0.001	0.001	-0.002	0.000	-0.002
Policy ind. $\times$ No. of gold mines	0.001	0.001	0.001	0.001	0.001	0.001	0.004***	0.002*	0.004***	0.003**	0.004***	0.002**
R <sup>2</sup> (within)	0.062	0.089	0.065	0.091	0.081	0.100	0.064	0.112	0.065	0.115	0.101	0.136
Observations	5724	5724	5618	5618	5565	5565	5724	5724	5618	5618	5565	5565
Territory fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged and adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes

**Table 4: Robustness of Main Empirical Results - Continued**  
in territories of Five Eastern Provinces (2004-2012)

	<i>Y= Looting Indicator</i>						<i>Y= Battle Indicator</i>					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<b>E. Omits 2012</b>												
Policy indicator	0.072**	0.085**	0.081**	0.095**	0.077**	0.094**	0.040	0.023	0.034	0.033	0.011	0.009
Policy ind. x No. of 3T mines	-0.001	-0.002	-0.000	-0.001	-0.000	-0.001	0.001	-0.001	0.000	-0.001	0.000	-0.001
Policy ind. x No. of gold mines	0.001	0.000	-0.000	0.000	-0.000	0.000	0.003**	0.002	0.003**	0.002	0.003**	0.001*
R <sup>2</sup> (within)	0.054	0.091	0.058	0.093	0.063	0.095	0.042	0.086	0.043	0.089	0.073	0.108
Observations	6720	6720	6580	6580	6510	6510	6720	6720	6580	6580	6510	6510
<b>F. Policy Indicator x Log(Mines)</b>												
Policy indicator	0.053***	0.064***	0.053***	0.066***	0.041**	0.055**	0.086***	0.038	0.081***	0.042*	0.048**	0.013
Policy ind. x No. of 3T mines	-0.005	-0.013	-0.005	-0.013	-0.004	-0.013	-0.009	-0.015	-0.011	-0.017*	-0.010	-0.017*
Policy ind. x No. of gold mines	0.014*	0.013	0.011	0.014	0.009	0.012	0.029**	0.023**	0.033**	0.030***	0.029**	0.022**
R <sup>2</sup> (within)	0.055	0.084	0.056	0.086	0.068	0.093	0.054	0.100	0.055	0.103	0.086	0.122
Observations	7560	7560	7420	7420	7350	7350	7560	7560	7420	7420	7350	7350
<b>G. Lagged Dep. Variables</b>												
Policy indicator					0.038*	0.060**					0.024	0.025
Policy ind. x No. of 3T mines					-0.000	-0.002					-0.000	-0.002
Policy ind. x No. of gold mines					0.001	0.001					0.003**	0.002**
R <sup>2</sup> (within)					0.075	0.095					0.093	0.120
Observations					7350	7350					7350	7350
<b>H. Alternative Empirical Model</b>												
Policy indicator	0.060**	0.071**	0.062**	0.074**	0.050**	0.064**	0.050	0.038	0.048	0.045	0.017	0.019
Policy ind. x No. of 3T mines	-0.001	-0.001	-0.002	-0.001	-0.001	-0.000	-0.003	-0.009**	-0.004*	-0.010**	-0.004*	-0.008**
Policy ind. x No. of gold mines	-0.003**	0.000	-0.002**	0.001	-0.003**	0.000	0.008***	0.002	0.008***	0.002	0.007***	0.001
R <sup>2</sup> (within)	0.055	0.083	0.057	0.084	0.070	0.092	0.055	0.100	0.056	0.102	0.087	0.121
Observations	7560	7560	7420	7420	7350	7350	7560	7560	7420	7420	7350	7350
Territory fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged and adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes

**Notes:** \* p<0.10; \*\* p<0.05; \*\*\* p<0.01. Standard errors (not shown) are clustered at the territory level. Panel B analyzes the number of looting and battle incidents rather than indicators for whether or not there was an incident. Panel C omits the four territories comprising the following large cities: Lubumbashi in Katanga (population 1,786,397), Goma in North Kivu (population of 1,000,000), Kisangani in Orientale (population 925,977) and Bukavu in South Kivu (806,940). Panel D omits 17 territories that are adjacent to at least one policy territory. Panel E omits all observations from the year 2012. Panel F interacts the policy indicator with the natural log of the number of mines. In cases in which this number is zero, we force the value to be 0.1 prior to logging. Panel G employs lags of the dependent variable, rather than the lagged number of conflicts of all types, which is the measure employed in the baseline. Panel H shows results from an alternative econometric model that allows conflict to differ linearly with the number of mines in the non-policy territories after Dodd Frank. In this alternative model, the policy ind. x the no. of mines interaction effects are identified off of differences in conflict relative to non-policy territories with similar numbers of mines.

**Table 5: Fixed Effects Estimates of Monthly Conflict Indicators, Allowing for Separate Policy Effects**  
in territories of Five Eastern Provinces (2004-2012)

	<i>Y = Looting</i>						<i>Y = Battles</i>					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Policy Indicator	0.061*** (0.004)	0.069*** (0.003)	0.059*** (0.013)	0.074*** (0.005)	0.043** (0.041)	0.059** (0.018)	0.127*** (0.001)	0.080** (0.035)	0.123*** (0.003)	0.089** (0.011)	0.082** (0.022)	0.047 (0.114)
Mining Ban Indicator	0.004 (0.874)	-0.007 (0.778)	0.004 (0.870)	-0.004 (0.875)	0.019 (0.408)	0.010 (0.667)	-0.138*** (0.008)	-0.135** (0.012)	-0.136** (0.007)	-0.124** (0.018)	-0.100** (0.018)	-0.087* (0.073)
R <sup>2</sup> (within)	0.051	0.082	0.055	0.084	0.067	0.091	0.051	0.101	0.052	0.103	0.083	0.121
Observations	7560	7560	7420	7420	7350	7350	7560	7560	7420	7420	7350	7350
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged & adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes

**Notes:** \* p<0.10; \*\* p<0.05; \*\*\* p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.



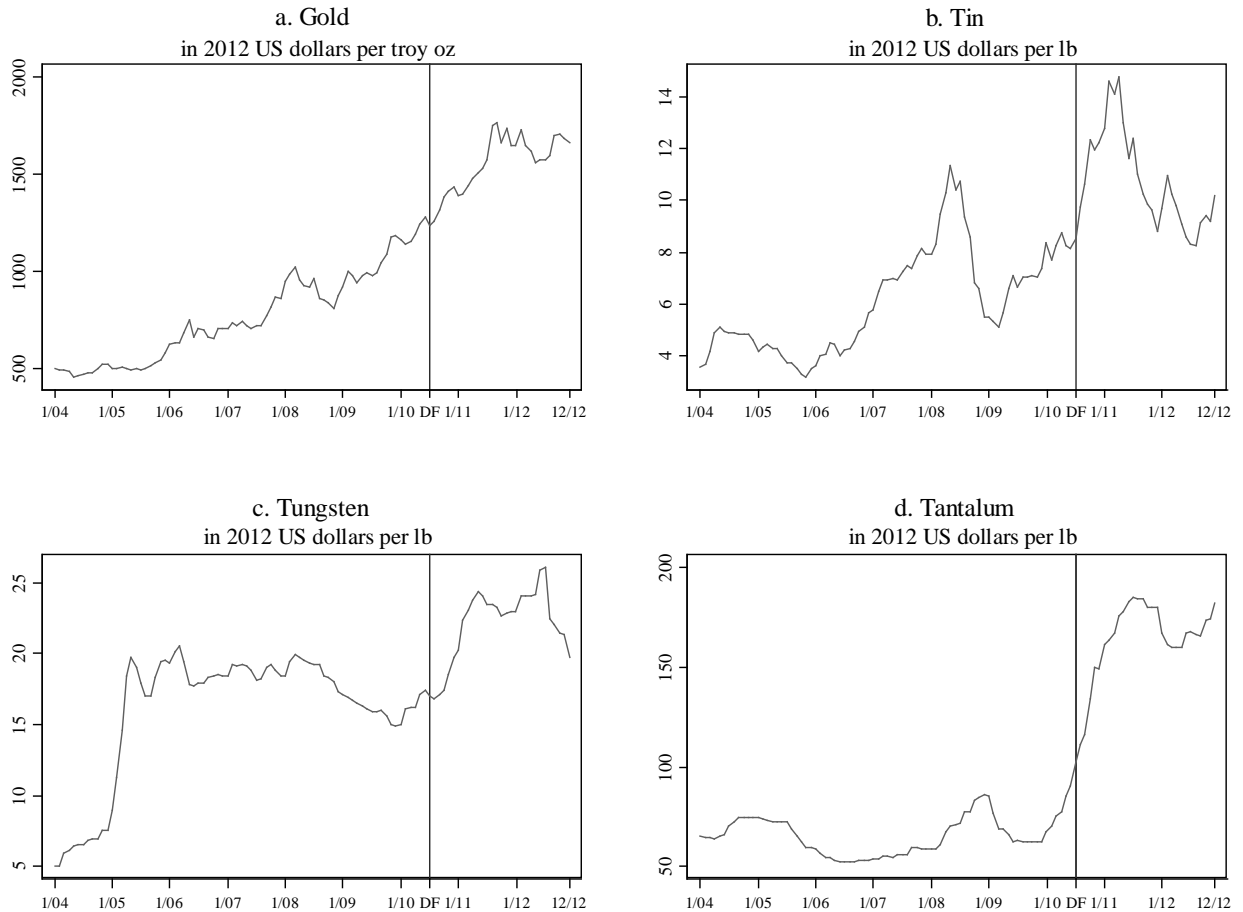
**Table 6: Fixed Effects Estimates of Monthly Violence against Civilians Indicator**  
in territories of Five Eastern Provinces (2004-2012)

	<i>Y = Violence Against Civilian Indicator</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A:</b>						
<b><i>Policy Interacted with Mines</i></b>						
Policy Indicator	0.052 (0.227)	0.144*** (0.001)	0.070* (0.099)	0.144*** (0.002)	0.029 (0.371)	0.108*** (0.001)
Policy ind. x No. of 3T mines	0.001 (0.768)	-0.001 (0.495)	0.000 (0.907)	-0.000 (0.803)	0.000 (0.947)	-0.000 (0.727)
Policy ind. x No. of gold mines	0.004* (0.082)	0.001 (0.486)	0.002 (0.164)	0.001 (0.349)	0.002 (0.124)	0.001 (0.522)
R <sup>2</sup> (within)	0.062	0.120	0.071	0.124	0.120	0.155
<b>Panel B:</b>						
<b><i>Separate Policy Indicators</i></b>						
Policy Indicator	0.105*** (0.007)	0.158*** (0.000)	0.100** (0.015)	0.167*** (0.001)	0.045 (0.153)	0.110*** (0.001)
Mining Ban Indicator	-0.027 (0.494)	-0.055 (0.181)	-0.013 (0.733)	-0.043 (0.276)	0.039 (0.230)	0.009 (0.791)
R <sup>2</sup> (within)	0.058	0.120	0.069	0.124	0.119	0.155
<b>Panel C:</b>						
<b><i>Single Policy Indicator</i></b>						
Policy Indicator	0.100*** (0.007)	0.145*** (0.000)	0.098** (0.014)	0.156*** (0.000)	0.052* (0.098)	0.113** (0.000)
R <sup>2</sup> (within)	0.058	0.120	0.069	0.124	0.119	0.155
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes
Lagged & adj. conflict controls	No	No	No	No	Yes	Yes
Observations	7560	7560	7420	7420	7350	7350

**Notes:** \* p<0.10; \*\* p<0.05; \*\*\* p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

## ONLINE APPENDICES

### APPENDIX 1: MONTHLY WORLD PRICES OF CONFLICT MINERALS, 2004-2012



**Notes:** The source is MetalPrices.com, online subscription. The gold price represents the monthly average of the PM spot prices on the London Market Exchange. The tin price is the monthly average of the cash official price paid by buyers on the London Market Exchange. The tungsten price represents the monthly average of the ferro tungsten alloy price. The tantalum price represents the monthly average price paid for tantalum scrap by U.S. Vacuum Processors. “DF” indicates the passage of Dodd-Frank.

## **APPENDIX 2: CONSTRUCTING THE TERRITORY LEVEL RAINFALL VARIABLES**

We estimated monthly precipitation for the 70 territories of the eastern DRC using the following process. First, we downloaded precipitation normals from GPCC Visualizer (<http://kunden.dwd.de/GPCC/Visualizer>) as ascii ArcView GRID files, and then converted to rasters using the ArcGIS ascii to raster tool. Next we downloaded error corrected monthly precipitation data as a NetCDF file. This file contained monthly precipitation data from 1901-2010 and had to be unpackaged to obtain the years relevant for our empirical analysis. The process to unpackage the file and convert individual months to ArcGIS raster files was done with the help of code written in Python. We next used the zonal statistics tool in ArcGIS 10.2 to calculate average precipitation values for each of the 70 territories. We resampled the precipitation data to 0.1 degree resolution for this purpose, because in some cases the one degree pixels of precipitation data were larger than the territories. After resampling, we were able to calculate average precipitation values for all territories.

The precipitation data for 2004-2010 were already error corrected in GPCC raw data files, but we needed to correct the 2011 and 2012 data for systematic gauge errors prior to use, so that it would correspond to the 2004-2010 data. We executed the correction using the GPCC 1 degree relative systematic gauge error product, available for every month during 2011-2012 period from GPCC Visualizer. We converted the percent error to a multiplication factor which we applied to each month of the 2011-2012 precipitation grids. After this correction was achieved, we resampled the data to 0.1 degrees using the same procedure described above.

### APPENDIX 3: PLACEBO TESTS ON TIMING OF MINING POLICIES

Table A1 presents placebo tests that assign false policy passage dates, in July 2009 and July 2008, rather than the actual passage of Dodd Frank in July 2010. We also assign a January 2009 placebo date to correspond with the beginning of the Kimia II offensive, which was a coalition between competing armed groups in the eastern DRC (see Sanchez de la Sierra 2014). We employ these placebo tests to account for the possibility that our territory-specific linear time trends do not adequately control for conflict that may have been trending prior to the conflict mineral policies. Statistically significant placebo tests would therefore raise concerns that the main results are confounded by pre-existing trends.

Table A1 shows the findings pass the placebo tests. Of the 36 placebo coefficients for looting, only one is significantly correlated with looting probabilities or looting incidents. In this case, the  $\hat{\beta}_1$  placebo estimate is negative, which is opposite to our main looting finding (which is  $\hat{\beta}_1 > 0$ ) and hence does undermine its credibility. Of the 36 placebo coefficients for battles, only two coefficients are significantly different than zero, with the  $\hat{\beta}_1$  placebo estimate being positive in those cases. We do not think this finding undermines the strength of our main battle finding (which is  $\hat{\beta}_3 > 0$ ) for two reasons. First, the positive placebo estimates are on  $\hat{\beta}_1$  rather than  $\hat{\beta}_3$ . Second, the positive placebo  $\hat{\beta}_1$  estimates for the January 2009 and July 2009 placebo tests are insignificant, suggesting the rise in battles is attributable to a surge during July 2008 to January 2009, well before the passage of Dodd Frank in July 2010.

**Table A1: Placebo Tests of False Policy Treatment Dates Prior to Dodd Frank**  
in territories of Five Eastern Provinces (January 2004- June 2010)

	July 2009 – June 2010 Treatment Placebo				January 2009 – June 2010 Treatment Placebo (to correspond with Kimia II)				July 2008 - June 2010 Treatment Placebo			
	Loot	Loot	Battle	Battle	Loot	Loot	Battle	Battle	Loot	Loot	Battle	Battle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<b>Panel A: Y = Conflict Indicator</b>												
Policy indicator placebo	-0.011 (0.344)	-0.011 (0.332)	-0.015 (0.580)	-0.002 (0.926)	-0.017* (0.099)	-0.017* (0.088)	0.004 (0.870)	0.013 (0.503)	-0.008 (0.463)	-0.009 (0.388)	0.054** (0.041)	0.057** (0.013)
Placebo x No. of 3T mines	0.001 (0.352)	0.001 (0.339)	0.003 (0.355)	0.003 (0.338)	0.001 (0.307)	0.001 (0.294)	0.002 (0.330)	0.002 (0.307)	0.000 (0.593)	0.000 (0.562)	0.001 (0.626)	0.001 (0.613)
Placebo x No. of gold mines	0.000 (0.765)	0.000 (0.693)	0.002 (0.315)	0.002 (0.245)	0.000 (0.556)	0.000 (0.445)	0.001 (0.388)	0.001 (0.269)	-0.000 (0.525)	-0.000 (0.637)	0.000 (1.000)	0.000 (0.751)
R <sup>2</sup> (within)	0.021	0.025	0.035	0.064	0.021	0.025	0.034	0.064	0.021	0.025	0.036	0.065
Observations	5320	5250	5320	5250	5320	5250	5320	5250	5320	5250	5320	5250
<b>Panel B: Y = Conflict Incidents</b>												
Policy indicator placebo	-0.014 (0.530)	-0.012 (0.552)	-0.060 (0.659)	0.015 (0.822)	-0.022 (0.197)	-0.021 (0.181)	-0.054 (0.644)	0.003 (0.962)	-0.009 (0.568)	-0.010 (0.469)	0.126 (0.370)	0.141 (0.134)
Placebo x No. of 3T mines	0.001 (0.381)	0.001 (0.370)	0.008 (0.452)	0.006 (0.395)	0.002 (0.250)	0.002 (0.235)	0.005 (0.512)	0.004 (0.455)	0.001 (0.501)	0.001 (0.479)	0.005 (0.602)	0.005 (0.475)
Placebo x No. of gold mines	0.000 (0.817)	0.000 (0.772)	0.004 (0.504)	0.004 (0.311)	0.000 (0.616)	0.000 (0.514)	0.003 (0.555)	0.004 (0.270)	-0.000 (0.698)	-0.000 (0.866)	-0.002 (0.594)	-0.000 (0.960)
R <sup>2</sup> (within)	0.019	0.038	0.038	0.106	0.020	0.038	0.038	0.106	0.019	0.038	0.038	0.107
Observations	5320	5250	5320	5250	5320	5250	5320	5250	5320	5250	5320	5250
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 72)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
World mineral price controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rainfall controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged and adj. conflict controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

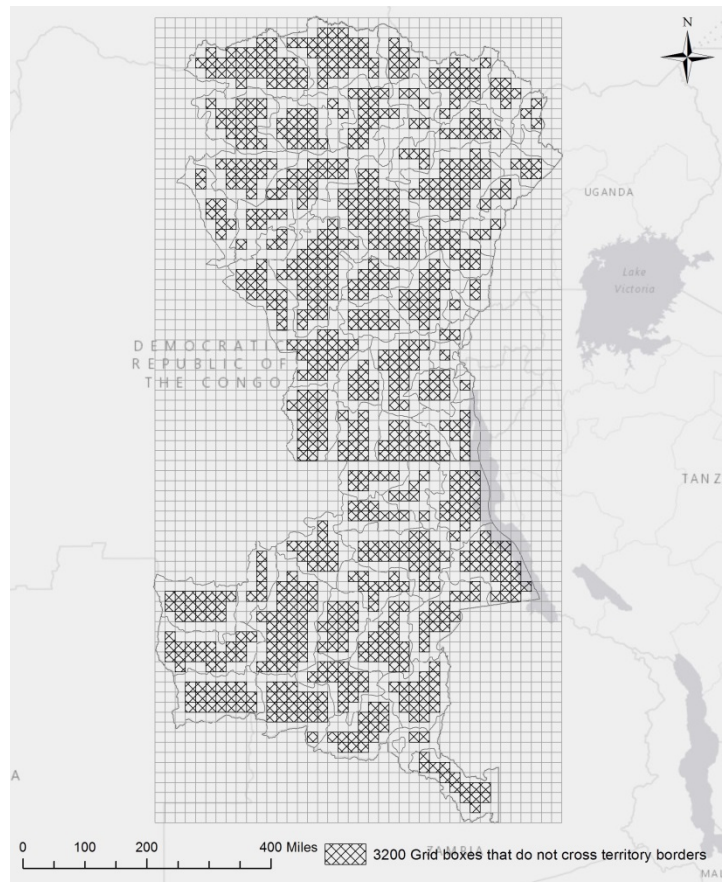
**Notes:** \* p<0.10; \*\* p<0.05; \*\*\* p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

#### APPENDIX 4: DISAGGREGATED EMPIRICAL ANALYSIS

In our theoretical model, militias move to loot agricultural sites and to engage in battles over gold mining sites. The theoretical model is silent about how far the militias will move, but our empirical tests employ territory-level data. As spatial units, territories are arguably too large to match our theoretical sites. In this section, we check the robustness of the results to spatial disaggregation.

To perform the disaggregation, we have imposed a spatial grid on the eastern DRC (see below map). Each cell is approximately 700 km<sup>2</sup> (270 miles<sup>2</sup>), with variation due to the Earth's curvature. For reference, the averaged sized territory is 17,912 km<sup>2</sup>. The 700 km<sup>2</sup> cell size is discretionary but not completely arbitrary. In choosing size, we attempted to balance the drawbacks of small and large cells. Cells that are too small fail to correct for spatial measurement error in IPIS mine and ACLED conflict location. Cells that are too large overlap multiple territory boundaries, making within-territory analysis impossible.

The map of gridded cells demonstrates that many of our 700 km<sup>2</sup> cells overlap territory boundaries. We exclude overlapping cells in the analysis that follows because we are interested in measuring how *within-territory* changes in conflict after Dodd Frank related spatially to the location of mines. We estimate within-territory changes by interacting an indicator variable for the territory containing a cell with an indicator variable for the post-Dodd Frank time period. These simple interactions drop all cells that overlap territory boundaries, leaving 879 cells that are fully contained in a single territory.



The cell-level panel contains 94,824 observations, with 108 time periods and 879 cells. The “policy cells” are contained in a policy territory as defined in table 2.

Unfortunately, excluding cells that overlap territory boundaries requires us to drop areas containing nearly half of the IPIS mines, which happen to concentrate along territory boundaries. Within the 879 cells, the number of IPIS tin mines prior to Dodd Frank ranged from 0 to 8 and the number of gold mines ranged from 0 to 12. The cell-level data set does not include rainfall variables. This is because our rainfall data source reports rainfall estimates at a spatial level much more aggregated than the 700 km<sup>2</sup> gridded cells mapped above.

Table A2 shows results from regression analysis of the cell-level panel. The columns 1-4 results are most comparable to the results in table 3, because those specifications do not control for territory specific policy effects. Hence, the  $\beta$  coefficients (from equation 14) are identified in columns 1-4 off of across and within territory variation in conflict, before and after Dodd Frank. Turning to the  $\hat{\beta}$  estimates in panel A, we see that looting probabilities increased across all of the policy cells post Dodd Frank. In terms of the magnitudes, the column 4 coefficient of  $\hat{\beta} = 0.0037$  is relative to a pre-Dodd Frank mean probability of 0.0013 across the policy cells. Hence, the probability of looting increased by 285%. This increase was not robustly different in cells with 3T and gold mines. In columns 1-4 of panel B, we see that battle probabilities did not in general increase across the policy cells. Battle probabilities did increase with the number of gold mines in the policy cells, however.

To summarize, the results in columns 1-4 are similar to the main results in table 3 in that both suggest that Dodd Frank increased the probability of looting across the entire policy region. The results in columns 1-4 indicate that Dodd Frank did not in general increase the probability of battles unless the policy cell contained gold mines.

Columns 5-8 of table A2 include 63 indicator variables that are interactions between a cell's territory and an indicator for the post Dodd Frank time period. Although there are 70 territories in the eastern DRC, seven territories are dropped because they do not fully contain at least two 700 km<sup>2</sup> cells. With these interaction variables included, the  $\beta_2$  coefficient from equation (14) measure cell-level relationships between the number of 3T mines and post-Dodd Frank conflict while holding constant the average territory level increase in conflict. Hence,  $\beta_2 > 0$  means that conflict increased with the number of 3T mines, *in addition to the territory level rise in conflict* induced by Dodd Frank.  $\beta_3 > 0$  has a similar interpretation with respect to the number of gold mines.

**Table A2: Fixed Effects Estimates of Monthly Conflict Indicators using Gridded Cells**  
in territories of Five Eastern Provinces (2004-2012)

	<i>Across and Within Territories</i>				<i>Within Territories</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A: Y = Loot Indicator</b>								
Policy indicator	0.0036** (0.0343)	0.0037** (0.0401)	0.0028* (0.0555)	0.0037** (0.0433)				
Policy ind. x No. of 3T mines	0.0015** (0.0216)	0.0013** (0.0377)	0.0009 (0.3250)	0.0007 (0.2552)	0.017** (0.0170)	0.0018*** (0.0061)	0.0011 (0.3124)	0.0012** (0.0223)
Policy ind. x No. of gold mines	0.0002 (0.6856)	-0.0000 (0.9729)	-0.0002 (0.6527)	-0.0003 (0.5606)	0.0001 (0.8683)	-0.0004 (0.6619)	-0.0001 (0.8221)	-0.0005 (0.5347)
R <sup>2</sup> (within)	0.003	0.060	0.030	0.070	0.012	0.064	0.033	0.073
Observations	94824	94824	92190	92190	94824	94824	92190	92190
<b>Panel B: Y = Battle Indicator</b>								
Policy indicator	0.0076** (0.0269)	0.0025 (0.3558)	0.0058** (0.0253)	0.0020 (0.4031)				
Policy ind. x No. of 3T mines	0.0006 (0.6726)	0.0000 (0.9719)	0.0019 (0.2629)	0.0011 (0.2624)	0.0008 (0.6397)	0.0006 (0.4798)	0.0019 (0.3468)	0.0016 (0.1980)
Policy ind. x No. of gold mines	0.0006 (0.6588)	0.0018** (0.0872)	0.0022* (0.0889)	0.0021** (0.0278)	0.0002 (0.9045)	0.0018 (0.1207)	0.0017 (0.2407)	0.0023** (0.0281)
R <sup>2</sup> (within)	0.004	0.060	0.030	0.070	0.012	0.064	0.033	0.073
Observations	94824	94824	92190	92190	94824	94824	92190	92190
Gridded cell fixed effects (i = 879)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gridded cell specific time trends	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	No	No	Yes	Yes
Rainfall controls	No	No	No	No	No	No	No	No
Lagged and adj. conflict controls	No	No	Yes	Yes	No	No	Yes	Yes
Territory specific policy effects (i=63)	No	No	No	No	Yes	Yes	Yes	Yes

**Notes:** \* p<0.10; \*\* p<0.05; \*\*\* p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses. The spatial unit of observation is a gridded cell, approximately 700 km<sup>2</sup> in size. The specifications in columns 1-4 are similar to those in table 5 except that here we do not include rainfall controls, because rainfall data are presented in our data source an aggregate level much larger than these gridded units. The specifications in columns 5-8 allow conflict to differ for each territory in the post-Dodd Frank time period by including 63 territory indicators by a post-Dodd Frank time period indicator. Hence, the coefficients in columns 5-8 are identified by within-territory changes in conflict in mining cells after Dodd Frank, relative to overall conflict changes in the territory after Dodd Frank.

Turning to the columns 5-8 results, we see that  $\hat{\beta}_2 > 0$  in the looting regressions of panel A. This result means that, within territories with 3T mines, the increase in looting was greatest in cells containing more 3T mines. This result is consistent with the following interpretation: militia groups that were stationed near 3T sites decided to loot those sites after the policies lowered the net return from stationing. How does this finding and interpretation reconcile with the columns 1-4 (and table 3) finding that across-territory looting probabilities were not disproportionately concentrated in 3T mining cells? One explanation that is consistent with our theory is that militia groups eventually moved out of 3T territories during 2010-2012 in search of revenue.



The columns 5-8 results also indicate that  $\hat{\beta}_3 > 0$  in the battle regressions of panel B, although these coefficients are precisely estimated only in the specification that includes time trends and the full set of covariates. This result means that, within territories with gold mines, the increase in battles was greatest in cells containing more gold mines. This result is consistent with the columns 1-4 results above (and table 3). The finding that within-territory estimates of  $\hat{\beta}_3$  are less statistically precise may imply that battles for gold control do not occur in the immediate vicinity of the mines: possibly the battles occur at out-of-cell locations that are more strategic to attack and/or defend. Moreover, if strategic battle locations tend to be near territory boundaries where topography changes are sharpest, then the analysis of gridded cells omits important areas where battles concentrate.

To summarize, analysis of gridded cells that are much smaller than territories generates results that are generally consistent with territory-level analysis. The differences that do arise raise detailed questions about precisely where within a territory battles over minerals will occur; these are questions that we do not address and leave for future research.

## APPENDIX 5: ALTERNATIVE THEORETICAL EXPLANATIONS FOR EMPIRICAL FINDINGS

### A. *Crime Displacement*

Theories in the literature on crime displacement are closely related to our stylized ‘bandits’ theory and potentially explain patterns of increased conflict that we observe in the data. Crime displacement refers to the indirect effects of police interventions, or related policies against crime, that are focused on a particular illicit industry or on a particular neighborhood. Displacement occurs when a reduction of crime in the targeted area is offset by an increase in crime in other industries or neighborhoods. Displacement is caused by the behavioral responses of criminals to changes in the relative net return of criminal activity. For example, a police crackdown on crime in one neighborhood will raise the net return of committing crime in an adjacent neighborhood and rational criminals may relocate their activities. More generally, displacement has been referred to as the foreclosure of one type of criminal activity shifting the incidence of crime to different forms, times, and locales (Repetto 1976).

According to Draca et al. (2010), the issue of crime displacement has been considered much more in criminology than in economics<sup>1</sup> and our observation is that crime displacement reasoning has only sparsely been used to explain civil conflict over natural resources in the economics literature.<sup>2</sup> An exception is Maystadt et al. (2014), who use crime displacement reasoning to explain empirical relationships between new mining concessions in the DRC during 1997-2007 and violent conflict. They find that conflict occurred at the periphery of mining sites rather than near the mines themselves. They theorize that this empirical pattern occurred because armed groups sought to ensure that mineral production was not disturbed by nearby violence, intentionally displacing violence away from economically productive areas.

The relevance of crime displacement to our analysis depends on whether or not taxing, looting, and battles are aptly analyzed as criminal events, and whether or not the Dodd Frank boycott is properly conceived as a crime enforcement effort or crackdown. If one accepts the crime terminology as descriptive of the setting, then our theory can be re-casted in the following way: the mining policies caused criminals (armed militia groups) to substitute

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<sup>1</sup> Presentations of the issues in the criminology literature include Repetto (1976), Barr and Pease (1990), Guerette and Bowers (2009), Braga (2001), Hesselning (1994), and Sherman and Weisburd (1995). A recent crime displacement analysis in economics is Dell (2014), who studies crackdowns on Mexican drug trafficking.

<sup>2</sup> In political science, Kalyvas (2015) draws parallels between civil war and organized crime, and he discusses the role of natural resources.

one type of criminal activity (concentrated taxation of miners) with another (dispersed looting) and geographically displaced another crime (battles) to other industries and territories (gold).

We view this characterization as a close but imperfect match with the setting and consequences of Dodd Frank. First, Dodd Frank was a targeted boycott rather than an enforcement crackdown. Second, militia taxing at mining sites resembled a legitimate and productivity enhancing governmental function whereas looting resembles the actions of common criminals. Taxing was often regular, predictable, and consensual whereas looting was not. In return for paying taxes, miners received protection and sometimes miners directly solicited an armed group presence for that purpose.<sup>3</sup> Empirically, only six DRC conflict events are described in ACLED with “tax” whereas “loot”, “ransack”, “steal”, etc. is a common description. To us, these are indications – albeit coarse ones – that militia taxing in this environment is not simply criminal. If it’s not, then the possibility that a Dodd Frank boycott led groups to abandon legitimate government functions in favor of common and violent criminal activity may be somewhat obscured if the Dodd Frank chain of events is viewed through the lens of standard crime displacement.

#### *B. Opportunity Cost of Militia Participation*

By holding constant the size of militia groups, our theoretical framework focuses on how the policies affected the spatial allocation of a fixed amount of armed soldiers. Our framework does not allow for soldier entry or exit, which we cannot directly observe in the ALGED data. By reducing mining opportunities and local prices for 3Ts, however, the policies presumably lowered the opportunity cost of militia participation and could have augmented militia sizes.<sup>4</sup> This mechanism could be a contributing factor to the increase in looting and violence against civilians that we observe in tables 3-6. Such a mechanism would be consistent with a large literature suggesting that negative shocks to resource values increases the opportunity cost of fighting, drawing labor into conflict activities (see Becker 1968, Grossman 1999, Chassangy and Miquel 2009). It would also be consistent with some

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<sup>3</sup> Sanchez de la Sierra (2014) provides several examples of this process. In addition, Carisch (2012, pp. 27) describes a situation in which an armed group (the FRF) received logistical and general support from mining communities in return for protection. The FRF raised revenues from taxing local markets and gold traders.

<sup>4</sup> Moreover, if the policy caused more entry into agriculture or other industries, and the wage in those industries fell as a result, then the policies could have further incentivized militia entry through the opportunity cost channel.

commentators in the eastern DRC who worried that Dodd Frank could cause young men to join militias (Pöyhönen et. al. 2010).

It is worth noting that the argument that Dodd Frank would lead to larger militias is inconsistent with the assumptions of the policy makers who sought to reduce the size and strength of militia groups by cutting off revenue sources. With reduced revenues, the militia groups would have difficulty supporting new entrants with supplies, weaponry, and food and this would presumably reduce incentives to join militias (and could cause exit).<sup>5</sup>

We cannot observe enrollment in militia groups, but we attempt to shed light on the effects of Dodd Frank on militia entry by analyzing a “militia recruitment” indicator variable that we create from the ACLED data. We construct this variable by flagging events described with the words “recruit”, “enlist”, or “draft.” There are 64 events over 2004-2012 described with these words. Examples include: “Mayi Mayi Militia (Yakutumba) carry out a recruitment drive around Fizi”; “Reports that Former CNDP militia have attempted to recruit child soldiers in the area”; and “4 FARDC officers stationed at Goma begin a recruitment drive for young soldiers.”<sup>6</sup>

Table A3 presents regression estimates of the recruitment variable that follow the same specification sequence as the estimates for violence against civilians shown in table 6. Columns 1-6 employ the full 2004-2012 sample. Columns 7-12 employ a sub-sample that is cut off at the end of 2011. Cutting the sample after 2011 is useful for two reasons. First, it focuses attention on a time period in which armed group weaponry had probably not yet been significantly diminished by Dodd Frank. Second, it also focuses attention on a time period before unemployed miners may have relocated from one territory to another for income opportunities.

Two table A5 sets of results from the 2004-2011 sample arguably imply that recruiting episodes became more likely when and where the opportunity cost of entry was driven down by the mining policies. First, the panel A coefficients on the interaction between the policy indicator and the number of 3T mines is positive and bordering on statistical significance with *P* values ranging from 0.109 to 0.228. This result provides weak evidence that recruiting increased in territories most directly targeted by the mining policies. Second, the positive panel B coefficients on the mining ban indicator, and the negative coefficients on

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<sup>5</sup> The table 3 finding that looting probabilities increase in the aftermath of favorable rainfall shocks also seems inconsistent with an opportunity cost theory. Favorable rainfall shocks should raise the return from agriculture, and this would seemingly raise the opportunity cost of looting.

<sup>6</sup> We recognize the ACLED data may dramatically understate recruiting efforts and that this weakens the usefulness of the following empirical exercise.

the policy indicator, indicate that recruiting episodes were more likely during the mining ban but less likely in the 2011 months that followed. These results are linked to opportunity cost because the all-out ban on artisanal mining presumably lowered the opportunity cost of militia entry to a greater extent than the Dodd Frank boycott that did not extend to gold.

In summary, our investigation of recruitment is limited but it arguably provides weak evidence that the conflict mineral policies had a short-run positive impact on recruitment (and possibly entry). The spatial pattern of recruitment does not match the pattern of looting, battles, or violence against civilians across the policy territories, however. Whereas recruitment increases were disproportionally confined to 3T areas during the mining ban, the increase in looting was widespread and the increase in battles was disproportionally concentrated in gold territories. This coarse analysis implies that the policy-driven decline in opportunity cost possibly contributed to militia strength, but it does not appear to explain where the conflict occurred. We recognize that this analysis is insufficient to rule out the opportunity cost channel, and we hope that future research can address this issue in more depth.

**Table A5: OLS Fixed Effects Estimates of Monthly Militia Recruiting Episodes**  
in territories of Five Eastern Provinces

	<i>Y = Recruiting Indicator, Sample is 2004-2012</i>						<i>Y = Recruiting Indicator, Sample is 2004-2011</i>					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<b>Panel A:</b>												
<b>Policy Interacted with Mines</b>												
Policy Indicator	0.013 (0.215)	0.005 (0.412)	0.011 (0.336)	0.004 (0.554)	0.007 (0.446)	-0.000 (0.967)	-0.001 (0.864)	-0.003 (0.543)	-0.004 (0.351)	-0.004 (0.509)	-0.005 (0.309)	-0.004 (0.529)
Policy ind. $\times$ No. of 3T mines	0.000 (0.643)	0.000 (0.294)	0.000 (0.858)	0.001 (0.341)	0.000 (0.853)	0.001 (0.332)	0.001 (0.138)	0.001 (0.109)	0.001 (0.225)	0.001 (0.152)	0.001 (0.227)	0.001 (0.157)
Policy ind. $\times$ No. of gold mines	-0.000 (0.635)	0.000 (0.844)	-0.000 (0.627)	0.000 (0.729)	-0.000 (0.538)	0.000 (0.952)	0.000 (0.517)	0.000 (0.355)	0.000 (0.307)	0.000 (0.332)	0.000 (0.299)	0.000 (0.342)
R <sup>2</sup> (within)	0.026	0.047	0.026	0.048	0.036	0.055	0.020	0.027	0.022	0.029	0.023	0.031
<b>Panel B:</b>												
<b>Separate Policy Indicators</b>												
Policy Indicator	0.011 (0.127)	0.005 (0.346)	0.007 (0.436)	0.004 (0.504)	0.002 (0.795)	-0.002 (0.744)	-0.001 (0.624)	-0.004 (0.184)	-0.009** (0.048)	-0.007 (0.149)	-0.010** (0.034)	-0.008 (0.138)
Mining Ban Indicator	0.015 (0.419)	0.014 (0.456)	0.016 (0.395)	0.013 (0.503)	0.021 (0.232)	0.018 (0.302)	0.028* (0.095)	0.029* (0.091)	0.029* (0.089)	0.028* (0.089)	0.030* (0.083)	0.029* (0.082)
R <sup>2</sup> (within)	0.026	0.047	0.027	0.048	0.037	0.056	0.021	0.029	0.024	0.031	0.025	0.032
<b>Panel C:</b>												
<b>Single Policy Indicator</b>												
Policy Indicator	0.014** (0.043)	0.009* (0.094)	0.010 (0.232)	0.007 (0.275)	0.005 (0.410)	0.003 (0.711)	0.007 (0.174)	0.005 (0.366)	0.000 (0.989)	0.002 (0.739)	-0.000 (0.923)	0.002 (0.777)
R <sup>2</sup> (within)	0.025	0.047	0.026	0.048	0.036	0.055	0.018	0.026	0.021	0.029	0.022	0.030
Territory fixed effects (i = 70)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time period effects (t = 108)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory specific time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
World mineral price controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Rainfall controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Lagged & adj. conflict controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Observations	7560	7560	7420	7420	7350	7350	6720	6720	6580	6580	6510	6510

**Notes:** \* p<0.10; \*\* p<0.05; \*\*\* p<0.01. Standard errors are clustered at the territory level. *P* values are shown in parentheses.

### *C. Power Endowments, Information Asymmetries, and Commitment*

The theoretical framework considers the relative size of militia groups as a determinant of battle probabilities but we do not model the role of information asymmetries or commitment problems. These “rationalist” explanations for war could affect the decision to wage battle in general (see Fearon 1995), and potentially in the eastern DRC setting. In terms of commitment problems, Dodd Frank may have made peace alliances between militia groups more tenuous due to uncertainty over the future value of territory control and uncertainty over the future strength of various militia groups. This could have in principle triggered battles if the competing militia groups could not formulate incentive compatible cease fire agreements or alliances in the less certain environment. The Dodd Frank policy presumably also changed the relative military strength of groups and created new information asymmetries about that strength. This is possible, for example, if one militia had better access to gold than another or if there was asymmetric information about the extent to which the groups had access to new revenue sources. We cannot rule out these channels and we speculate that each played a role in triggering battles. These channels would not necessarily concentrate battles disproportionately in gold mining territories, however. It is also not clear to us how these channels would affect looting, which is not an outcome of focus in rationalist theories of war.

A related issue is that Dodd Frank could have attracted into policy territories certain militia groups that specialize in illegal trade. If these groups are more inclined to loot, then this kind of militia sorting could account for some of the rise in looting observed in the data. We feel that sorting is a complementary, rather than alternative, explanation for the empirical patterns. Militia sorting is implicit in our model, which focuses on the motivation for militia movement, and on the connection between militia movements and conflict.