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Unintended consequences of economic sanctions for human rights

Conflict minerals and infant mortality in the Democratic Republic of the Congo

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Abstract: Are victims of human rights abuses better off with or without economic sanctions targeted at their perpetrators? We study this question in the context of a US human rights policy, Section 1502 of the 2010 Dodd–Frank Act. By discouraging companies from sourcing 'conflict minerals' from the eastern Democratic Republic of the Congo, the policy has acted as a de facto boycott on mineral purchases that may finance warlords and armed militias. We estimate the policy's impact on mortality outcomes of children born prior to 2013 and find that it increased the probability of infant deaths in villages near the regulated 'conflict mineral' deposits by at least 143 per cent. We find suggestive evidence that the legislation-induced boycott did so by stunting mother consumption of infant health care goods and services. The findings demonstrate how sanctions and certification programmes for human rights can unintentionally harm the vulnerable populations they seek to protect.

Keywords: trade sanctions, infant mortality, civil conflict, conflict minerals, Dodd–Frank Act, Democratic Republic of the Congo, resource certification
JEL classification: F51, I15, O17, Q34

Figures and tables: provided at the end of the paper.

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1 Introduction

To discourage human rights abuses across the world, the United States and other developed countries often turn to a powerful foreign policy tool: economic sanctions. Sanctions against human rights abusers seek to help victims by withholding economic transactions from perpetrators until abuses cease, or other conditions are met (see Hufbauer et al. 2009). Sanction programmes for this purpose may target political regimes of countries (e.g. Iran, Cuba, Sudan), certain companies, individuals such as war criminals, or specific markets such as 'blood diamonds'.[1] But sanctions are blunt tools and their widespread use raises questions about their consequences for the populations they are supposed to help. Is it possible to withhold market transactions from perpetrators without impairing economic and health outcomes for victims? Are human rights victims better off with or without the sanctions?

Research points to cases in which economic sanctions have ostensibly harmed victim populations. For example, evidence shows that child health degraded after US sanctions on Iraq (Zaidi and Fawzi 1995), Haiti (Gibbons and Garfield 1999), Cuba (Barry 2000), and other countries (Peksen 2011). But did the sanctions *cause* impaired health? It is difficult to know because the sanctions were country-wide and hence the researchers had little choice but to rely on coarse time series and cross-sectional comparisons. Most of the current literature lacks a convincing counterfactual for how child health outcomes would have evolved in the absence of sanctions.

In this paper, we assess the causal impact of economic sanctions targeted towards human rights violators on child health by exploiting what we believe to be an excellent quasi-experimental setting. Our study focuses on a recent US human rights policy, Section 1502 of the 2010 Dodd–Frank Wall Street Reform and Consumer Protection Act. This policy discourages major electronics manufacturing companies—such as Apple, Motorola, and Hewlett Packard[2]—from sourcing 'conflict minerals' from the eastern Democratic Republic of the Congo (DRC).[3] The policy resembles a targeted trade sanction because the goal is to cut off revenue to warlords who control aspects of mineral trade and sometimes commit brutal acts of violence against civilians, including women and children.[4] Although Section 1502 is ultimately supposed to help set up a voluntary certification system for 'conflict-free' minerals, it has acted as an 'intended or

[1] In 2016 the US Treasury reports 28 different operational sanction programmes naming 21 different countries and over 6,000 people, as well as constraining the operation of markets and trade in diamonds, gold, uranium, and other nuclear materials, as well as the conflict mineral markets considered here (United States Treasury 2016).

[2] According to Bayer (2015) more than 1,200 different US companies have had to file their compliance with this policy to the Securities and Exchange Commission including both technology firms such as Intel and Xerox as well as media companies such as Walt Disney and apparel manufacturers such as Abercrombie and Fitch. Based on Bayer's numbers from 2014, the total compliance costs to those firms by 2016 is likely to have approached US$1 billion. Bayer (2015) also reports survey evidence that companies' most frequent reservation about the law was that it rendered them 'less competitive due to heavy compliance cost burden[s]'.

[3] The DRC is the world's 11th largest country by area and the 19th most populated. It ranks in the bottom five countries in the world in child mortality rates, and dead last in GDP per capita.

[4] The purpose of Section 1502, in the words of the co-sponsoring Congressman Barney Frank, is to 'cut off funding to people who kill people' (Aronson 2011).

unintended boycott' on purchases of tin, tantalum, and tungsten—the '3Ts'—from the broadly demarcated conflict mineral zone of the eastern DRC (Pöyhönen et. al 2010: 27).[5]

While it is generally accepted that Dodd–Frank caused significant declines in 3T mining, it is less clear whether or not 'Obama's Law', as it is known in local vernacular, has benefited Congolese citizens. Decreases in mineral production were absorbed by an artisanal mining sector that had supported an estimated 785,000 miners prior to Dodd–Frank (D'Souza 2007) with spillovers from their economic activity thought to affect millions. This is why some observers raise concerns that the conflict minerals legislation might have impaired economic conditions for marginalized populations in areas where mining has slowed (Pöyhönen et al. 2010; Aronson 2011; Sematumba 2011; Geenan 2012; Seay 2012).

We investigate these concerns quantitatively, by studying the potentially lasting impacts of Dodd–Frank through its effects on infant mortality. This is an outcome that could conceivably benefit from Dodd–Frank through two channels. First, if the legislation succeeded in achieving its goal of lowering civilian exposure to violent conflict, then child health would presumably improve. Second, if pollution flows from mining impairs child health in nearby villages, then Dodd–Frank could have lowered mortality rates through its reduction in mining activity.

Qualitative reports from health NGOs and research from the eastern DRC, however, suggest Dodd–Frank more likely increased mortality through three channels. First, the law may have reduced income streams to families and communities previously dependent on artisanal mining. Second, the law may have disrupted public health provision and reduced mothers' access to health care facilities and services. Third, contrary to the policy's purpose, Parker and Vadheim (2016) provide evidence that Dodd–Frank's goal of reducing conflict backfired and increased armed conflict, which could have adverse effects on infant mortality.

We test for mortality effects from the Dodd–Frank policy using a data set constructed from three publicly available sources. To measure infant mortality, we employ Demographic and Health Surveys (DHS) data from the most recent 2013 survey wave. The data include geo-coordinates of villages along with detailed information about mother respondents. We construct under-1 infant mortality rates based on recall questions, asked of mothers, to measure if and when children died between 2007 and 2013. To identify village locations most directly exposed to Dodd–Frank 'treatment' we use International Peace Information Services (IPIS) data on the geo-coordinates of artisanal mining during 2009 and 2010, before Dodd–Frank. To identify the location and timing of armed conflict we use the Armed Conflict Location Events Database (ACLED) and also control for rainfall shocks (i.e. flooding and drought) that could affect child mortality.

Our econometric strategy for isolating the effects of Dodd–Frank is based on triple difference comparisons of mortality in 'treated' villages with mortality in two types of comparison villages. The infants in treated villages are in the broadly demarcated policy zone targeted by the boycott, and also near (<20km) a 3T mine that was operating prior to Dodd–Frank.[6] One comparison

[5] Two years after Dodd-Frank was passed, advocacy groups were claiming success, stating 'the passage of conflict minerals legislation … [has] helped lead to a 65 per cent drop in armed groups' profits from trade in tin, tantalum, and tungsten …' (Johnson 2013: 53). A report by Bafilemba et al. (2014: 2) also points to success, pointing out, for example, that 'Armed groups and the Congolese army are no longer present at two-thirds (67 per cent) of tin, tantalum and tungsten mines surveyed…'.

[6] This distance threshold complies with recent literature on the spatial extent of mining impacts (see section 3), but we also employ different thresholds in robustness checks.

group consists of infants in villages near 3T deposits located outside of the targeted zone. The other comparison group consists of children born in villages within the targeted zone, but far (>20km) from a 3T mine. Infant mortality in the treated and comparison villages were following similar trends prior to Dodd–Frank; this supports their validity as counterfactuals for how mortality would have changed absent the conflict mineral policies.

The evidence indicates that Dodd–Frank increased the probability of infant deaths for children who live part of or all of their first year in treated villages under the Dodd–Frank regime. A conservative estimate is that the legislation increased under-1 mortality from a baseline mean of 60 deaths per 1,000 births to 146 deaths per 1,000 births, which represents a 143 per cent increase. While one might be willing to accept higher infant mortality as a necessary cost of defunding illicit armed militias, we also find evidence that mortality rose in policy zone villages near 3T mines that were not controlled by armed groups prior to Dodd–Frank. By contrast, we find no evidence that Dodd–Frank affected mortality in villages near gold mines, which are in similar areas and also small scale and artisanal but were de facto exempt from the Dodd–Frank-induced boycott as explained in section 2. Although some commentators have considered Dodd–Frank a failure in terms of its inability to effectively regulate gold, our findings provide a starkly different perspective.

We investigate some of the channels through which Dodd–Frank might have caused mortality rates to rise near targeted 3T mines and find suggestive evidence that the legislation did so by reducing consumption of health care goods such as disease-preventing bednets. In contrast, infant mortality decreased, and health care consumption increased, in villages near 3T mines outside of the Dodd–Frank-targeted conflict mineral zone. This provides evidence that 3T mineral endowments—which experienced rising world prices during 2010–12—would have in all likelihood helped improved infant health in the eastern DRC absent the US human rights policy.

We also investigate the relative impact of armed conflict near (< 20km) villages on infant mortality and compare this effect to the overall Dodd–Frank effect. The estimates indicate that Dodd–Frank would have to cause an enormous reduction in armed conflict in order to offset the legislation-induced increases in mortality rates through other channels. To reduce the mortality rate enough to offset the estimated 86 extra deaths per 1,000 caused by the policy, Dodd–Frank would have to eliminate 116 armed conflict events around the infant's village in the first 12 months after the infant's birth. A reduction of this magnitude would be possible for only 29 of 7,697 (0.4 per cent) infants in our sample. Overall, the findings suggest that, even if Dodd–Frank had been successful in reducing armed conflict, the cost of reducing conflict would have been larger than the benefit in terms of infant mortality.

Our findings provide quantitative backing to qualitative concerns about the human cost of sanctions and certification programmes in general, as well as quantitative backing for more specific concerns about the unintended consequences of conflict minerals legislation and the welfare costs of conflict-free certification.[7] Our findings are also relevant to the literature on the natural resource curse, particularly the strand investigating the sub-national effects of mining booms on local communities. Like other sub-national case studies of mining in Africa, South America, and elsewhere, our evidence suggests local mining opportunities are a net benefit for local populations even in the presence of poorly functioning institutions.[8] Our setting is unique,

[7] A growing literature raises scepticism that conflict minerals legislation is helping civilians in the eastern DRC. This literature includes Sematumba (2011), Pöyhönen et al. (2010), Seay (2012), Geenan (2012), and Jameson et al. (2015).

[8] Aggregate macro level studies generally find that countries dependent on mining have lower growth and lower incomes than others (see Deacon 2011; van der Ploeg 2011; Edwards 2016). Most sub-national studies of mining

however, because policy makers and activists have long assumed that mines in conflict zones such as the eastern DRC were a curse for local civilians, which is why they garnered the label 'conflict minerals'. Methodologically our study has the distinctive feature that the human rights policy shock created a unique experiment for comparing outcomes in villages near booming and busting mines.

The rest of the paper proceeds as follows. In section 2, we provide background on artisanal mining in the eastern DRC and Section 1502 of Dodd–Frank and related policies towards mining. In section 3, we review related literature. In section 4 we describe the data sets and in section 5 we provide the main empirical tests. Section 6 investigates channels and section 7 disaggregates effects from the intended and unintended boycott, followed by a conclusion.

2 Background: mining and conflict minerals legislation[9]

2.1 Artisanal mining prior to Dodd–Frank

The DRC contains large deposits of tin, tungsten, and tantalum (the '3Ts') that supply surging world demand for their use in mobile phones and other modern electronic devices. Armed militia groups and warlords have controlled some production of 3T minerals, along with gold, in eastern DRC and they have profited from surging global demand by taxing and extorting miners. The eastern provinces usually associated with these 'conflict minerals' are North and South Kivu, Maniema, Orientale, and Katanga (Figure 1) (D'Souza 2007; Bawa 2010; de Koning 2011).

The majority of the mines in the eastern DRC are worked by artisanal miners. These 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.[10] Artisanal mining is labour 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 produced 90 per cent of the minerals exported from the country prior to Dodd–Frank.

The left panel of Figure 2 shows the distribution of artisanal mining sites based on interactive maps created by the International Peace Information Service (IPIS) during 2008-2010, before passage of Dodd–Frank. Teams of researchers equipped with GPS devices and questionnaires identified the geographic coordinates of sites and solicited information about mineral resources. Rather than omitting important mining sites that its researchers could not physically visit, the IPIS estimated those mining locations and included them in the maps. From the IPIS maps, we know that gold and tin mines dominated the landscape and tantalum (coltan) and tungsten (wolframite) sites were relatively rare. Approximately one-half of the mines were controlled by, or visited regularly by, armed militias (including the Congolese Army), usually for the purpose of taxing and extorting civilian miners.

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booms, however, find evidence that mineral booms have improved at least short-run outcomes in South America (see Aragón and Rud 2013; Loayza et al. 2013) and Africa (Kotsadam and Tolenen 2016). For a literature review of local mining booms, see Aragon et al. (2015).

[9] This section builds partially on Parker's work co-authored by B. Vadheim, entitled 'Resource Cursed or Policy Cursed? US Regulation of Conflict Minerals and Violence in the Congo (2016).

[10] According to the 2002 DRC Mining Code, artisanal mining is 'any activity by means of which a person of Congolese nationality carries out the extraction and concentration of mineral substances using artisanal tools, methods and processes, within an artisanal exploitation area limited in terms of surface'.

2.2 Passage of 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. Congress passed and the President signed a revised version as Section 1502 of the Dodd–Frank Act on July 21, 2010 (Figure 3).[11] Section 1502 is designed to discourage major manufacturing and processing companies from purchasing conflict minerals. Section 1502 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 the DRC conflict mining zone then companies must report on the possibility that warlords have benefited from the purchases. It also authorized Congress to produce a map (which it commissioned from the IPIS) of the conflict mining zone to guide the regulatory process. As we discuss below, 3T mines located within this mapped zone were most directly subjected to intended and unintended boycotts.

Although Section 1502 of the Dodd–Frank Act did not prohibit the purchase of minerals from eastern DRC conflict zone, many observers say it acted as a swift de facto boycott of 3T minerals (Pöyhönen et al. 2010; Seay 2012). Rather than trying to discriminate mine origin and defend purchases from within the zone as not financing armed militias, many US companies avoided 3T purchases from the entire conflict zone. The boycotting of eastern DRC minerals became more explicit after 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). This formal boycott was likely a direct response to the Dodd–Frank legislation.

Probably also as a response to Dodd–Frank, the DRC imposed a governmental ban on artisanal mining in September, 2010. The ban covered three provinces - Maniema, North Kivu, and South Kivu (see Figure 2).[12] A week after the ban was announced, the Congolese Minister of Mines stated that it concerned only extraction of the 3Ts (see de Koning 2010) but observers note considerable confusion about whether or not gold was covered. The DRC governmental ban was lifted in March, 2011, shortly before the international EICC boycott took hold, effectively replacing it (de Koning 2011). We thus see the passage of the Dodd–Frank act in July 2010 as the beginning of a mining disruption and boycott that lasted through 2013, when our study period concludes.

2.3 Impact on mining activity

How did Dodd–Frank and the associated mining ban and EICC boycott impact mining activity? Official data reveal a large drop in exports of 3Ts during 2011-2012. Figure 4a shows the decrease in tin exported from North Kivu, South Kivu, and Katanga, the tin producing provinces of the DRC. The volume of official exports tracked the world price from 2004-2009,

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[11] See www.opencongress.org/bill/111-s891/show.

[12] 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 Kabilia'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'.

but then dropped significantly during 2010-2011 as the world price continued to rise. Some of the export decrease from the mining zone targeted by Dodd–Frank was offset by increased exports from Katanga Province, which was exempt from the mining ban and largely outside of the conflict territory mapped by the US State Department. We also know that while official exports went to zero during the ban, some 3T mining did continue in the policy targeted zone. A number of Chinese companies continued to buy 3T minerals from eastern Congo but at prices discounted of "up to 80 per cent compared to world market valuations" (Carisch 2012: 15, see also Johnson 2013).

Export data provide a less reliable indicator of gold production because approximately 98 per cent of gold mined in the eastern DRC was sold through unofficial channels (smuggled), both before and after the Dodd–Frank Act (de Koning 2011; United Nations 2014).[13] Figure 4b shows estimates of gold production over 2004–12. Production generally rose, with a slight decrease during 2011 when the mining ban was in force, but overall there is little evidence that Dodd–Frank reduced gold production.

The indicators just described suggest that Dodd–Frank and the mining ban were effective in slowing 3T mining within the targeted conflict mining zone, but much less effective in slowing gold mining. Parker and Vadheim (2016) corroborate these indications by reviewing satellite images of changes in forest cover around IPIS mining centroids before and after Dodd–Frank. Using deforestation rates as a proxy for the level of mining activity at particular mining sites, they report evidence consistent with the other data on Dodd–Frank's effects on mining activity. Deforestation rates systematically slowed around 3T mine coordinates within the targeted conflict zone after the passage of Dodd–Frank; they did not systematically slow around gold mine coordinates, nor did deforestation rates slow around 3T mine coordinates outside of the policy targeted zone. The findings in Parker and Vadheim (2016) from deforestation corroborate the evidence that the combination of the Dodd–Frank law, DRC governmental ban, and EICC boycott has reduced 3T mining in the mapped conflict mineral zone while having less or no effect on gold mining.

There are two main reasons why gold production rose despite its official status as a 'conflict mineral' under Dodd–Frank. First, DRC gold mainly goes to the Middle East and East Asia to supply jewellery markets, whereas 3Ts are primarily consumed by companies that are members of the EICC or regulated by Dodd–Frank (de Koning 2011). Second, while it is technologically feasible to track the origin of 3Ts and demonstrate whether or not their origin differs from those controlled by armed groups, this is less easily accomplished for gold. This is because gold is more easily smelted (i.e. separated from waste rock) on site or earlier in the supply chain (see Lezhnev and Prendergast 2009; Schraeder 2011).

To summarize, we emphasize the following: a) artisanal mining was an important economic sector in the eastern DRC prior to Dodd–Frank; b) artisanal mining was dramatically disrupted by a boycott of purchases that was triggered by Section 1502; and c) the boycott and certification policy slowed 3T mining within the Dodd–Frank targeted conflict mineral zone but 3T mining outside the zone did not slow and there is little evidence that gold mining slowed inside or outside the zone. In the sections that follow, we discuss and then estimate the impacts of these mining policies on infant mortality in locations near artisanal mining sites.

[13] The United Nations (2014) reports that networks engaged in smuggling gold from the DRC neighbouring countries are more than 20 years old, and deeply entrenched. Gold is easier and more profitable to smuggle than the 3Ts because it is much more valuable by weight.

3 Related literature

The Dodd–Frank sanctions and certification policy could potentially impact child mortality in the eastern DRC via four main channels. These are 1) conflict and violence; 2) family income and employment; 3) access to health care services; and 4) child and mother exposure to mining pollution. To inform our empirical analysis, we briefly describe literatures related to these channels. Prior to describing the literature on these channels, we first provide background on child mortality in Africa.

3.1 Child mortality in Africa

A large literature spanning many decades investigates the determinants of child health generally, and infant mortality specifically, in Sub-Saharan Africa. The global infant (under-1) mortality rate in 2012 was 35 per 1,000 live births compared to 63 per 1,000 live births in 1990, an improvement of over 40 per cent in two decades (UNICEF 2013). Although countries in Sub-Saharan Africa still endure high rates of infant mortality, Sub-Saharan Africa as a region has also experienced a drastic decrease in infant mortality rate. The highest rates of infant mortality, however, are still found in Sub-Saharan Africa: at 64 per 1,000 live births (1 in 16), it is more than 12 times the average in developed regions. Furthermore, the high fertility rate in Sub-Saharan Africa implies that by 2050, almost 40 per cent of all births will take place in Sub-Saharan Africa, so the actual worldwide number of infant deaths in this region may actually go up. Even with an almost 40 per cent reduction in infant mortality rate in Sub-Saharan Africa, the actual number of infant deaths decreased by less than 10 per cent.

As of 2013, the DRC had the sixth highest infant mortality rate in the world, at 78 per 1,000 live births.[14] Following global and African trends in infant mortality, the DRC's national rate has also fallen steadily over time, including during our study period of infants born during 2007–12. Across our study region, infant mortality decreased from 87 per 1,000 in 2007 to 55 per 1,000 in 2009 to 72 per 1,000 in 2012. As we describe in more detail below, however, infant mortality rates varied widely across villages in our study region of the eastern DRC (Figure 1).

There is an extensive public health literature indicating that infant mortality is affected by: 1) an infant's gender, birth order, birth interval; 2) parents' education level, age, mother's health, family income, and mother's intrahousehold bargaining power; and 3) household residency, wealth, water sources, and number of children (see e.g. Thomas et al. 1990). Kudamatsu et al. (2012) and Han and Foltz (2015) further demonstrate the importance of climate and weather variables in child mortality outcomes.

3.2 Civil conflict and child health

The ongoing conflict in the eastern DRC has elements of civil war, terrorism, and gang-related violence (Mampilly 2011; Austesserre 2012; Stearns 2014).[15] Literatures related to our study therefore include those focused on the effects of these types of conflicts on child health. Recent studies of civil war in Burundi (Bundervoet et al. 2009), Nigeria (Akresh, Bhalotra et al. 2012), and Ethiopia (Akresh et al. 2012), report evidence that a mother and child's exposure to conflict early in life causes long-term negative health impacts on surviving children. Recent studies of

[14] This ranking is based on data accessed at the World Bank's website at http://data.worldbank.org/indicator/SP.DYN.IMRT.IN.

[15] A common but debated assumption is that this violence has been motivated and funded by global demand for the region's mineral endowments (Cuvelier et al. 2014).

terrorism in Columbia (Camacho 2008) and Palestine and Israel (Mansour and Rees 2012) find that neonatal (in utero) exposure to terrorism incidents reduce child health, measured by birth weight.[16] With respect to gang violence, a recent study by Koppensteiner and Manacorda (2016) finds that increased incidents of homicides in a mother's neighbourhood led to lower birth weights in Brazil.

There is also a small literature on conflict and infant mortality in the DRC, including a recent working paper by Dagnelie et al. (2015), which focuses on the civil war period of 1997–2004. They study the relationship between armed conflict at the district level (there are 38 districts in the DRC and 12 in the eastern DRC) and under-1 mortality rates and find evidence that conflict increased under-1 child mortality for females but not for males. Earlier studies also consistently find higher rates of child mortality in conflict areas of Africa using different econometric techniques (Guha-Sapir et al. 2005; Guha-Sapir and D'Aoust 2010).

Based on the literature just cited, and intuitive reasoning, we conclude that Dodd–Frank could have reduced infant mortality if it succeeded in lowering mother and infant exposure to conflict. There is evidence, however, that Dodd–Frank has not reduced conflict (see Parker and Vadheim 2016). These findings cast doubt on the likelihood that Dodd–Frank successfully reduced child mortality through the conflict channel.[17]

3.3 Mining and child health

Dodd–Frank could have also affected child health through its effects on family income and on mother and child exposure to mining pollution. Aggregate macro-level studies generally find that countries dependent on mining have lower growth and lower incomes than others (see van der Ploeg 2011; Deacon 2011; Edwards 2016). These results, however, rarely hold in micro studies, which generally associate mining booms with increases in local incomes (see Aragón et al. 2015 for an overview). At the local level, mining can generate positive income effects through fiscal and employment channels. Fiscal channels, in which there are spending benefits from government mining revenues (see Caselli and Michaels 2013), require strong functioning institutions, which are not present in the eastern DRC, at least formally.[18] Positive employment effects, in which earning opportunities are generated in the mining industry with spillover

[16] Camacho (2008) does not focus on infant mortality but Mansour and Rees (2012) find weak evidence, at best, that infant mortality was directly impacted by a mother's exposure to terrorist attacks. Low birth weights, however, are a predictor of higher rates of infant mortality (Almond et al. 2005).

[17] Parker and Vadheim (2016) find evidence that the legislation increased looting and violence against civilians in the territories targeted by the boycott. Battle probabilities also increased in territories endowed with gold mines, presumably because gold was de facto exempt and hence became relatively more valuable to control. Instead of improving law and order, Parker and Vadheim (2016) explain how the legislation may have broken down a fragile low-conflict equilibrium in which militia groups were acting as mafia groups and providing security at and around mining sites. Their reasoning complements other studies of the DRC, which also suggest that valuable minerals endowments could promote peace in the unstable country (see Maystadt et al. 2014; Sanchez de la Sierra 2015a, 2015b). This reasoning analogizes the militia groups in the eastern DRC to organized criminals that ultimately prefer order and stability in the industries they control (see Fiorentini and Peltzman 1997; Skaperdas 2001; Buonanno et al. 2015).

[18] Although local mining populations are unlikely to benefit from fiscal channels through formal government, they may benefit from informal governance and security provided by armed militia groups (see Sanchez de la Sierra 2015a, 2015b; Parker and Vadheim 2016).

benefits to non-tradable sectors (see Aragón and Rud 2013; de Haas and Poelhekke 2016), are likely present and important in the eastern DRC.[19]

The literature also provides evidence on the spatial extent of income benefits, and on the differential impact of mining on women, particularly in Africa. De Haas and Poelhekke (2016) and von der Goltz and Barnwal (2014), for example, suggest the effects are concentrated within a 20km distance from the mine. Tolenen (2014) finds evidence that a gold-mining boom in Ghana improved employment outcomes near the mines while von der Goltz and Barnwal (2014) find significant positive effects on villager asset accumulation near mines in developing countries in general. In a cross-country study using DHS data spanning Africa, Kotsadam and Tolonen (2016) find that mining booms induce men to move into skilled manual labour jobs while women move from agriculture into service industries and become more likely to have yearly rather than seasonal cash incomes. When mines close, men return to agriculture while women are more likely to exit the labour force. The Kotsadam and Tolonen (2016) study implies that mining declines in African countries have the strongest negative effects on women's labour outcomes.

While we expect the positive income effects of mining booms to reduce child mortality, these benefits could potentially be offset by the impacts of mining pollution on child health. Aragón and Rud (2015) find reductions in agricultural productivity in the vicinity of gold mines in Ghana. They attribute the lower productivity to both air pollution reducing labour productivity, as well as pollution of the soil, water, and air that might affect agriculture directly. The effects are strongest within 20km of the mining site, and decline with distance from the mine beyond that threshold. The same pollutants that affect labour productivity could have health effects with potential impacts on child survival.

Research by von der Goltz and Barnwal (2014) on the health effects of mining across 44 countries finds that mining is associated with increases in stunting (i.e. malnutrition) of children and anaemia in girls, but that those effects are localized within 5 km of the mine, and specifically linked to mining that generates lead pollution. They conclude that while mining communities enjoy substantial increases in wealth, these increases do not protect them from the pollution aspects of mining stemming from lead pollution. Romero and Saavedra (2015), studying gold mining in Colombia, find that newborns with mothers living within 10km of a mine are healthier (higher APGAR scores). They attribute the higher APGAR scores near mines to higher incomes and potentially better water and sewer systems.

Perhaps most relevant for our research is recent work by Chunan-Pole et al. (2015), which focuses on the health and income effects of large gold-mining operations in Ghana. That work shows large decreases in infant mortality at both the local and district level in and around mining operations. They attribute this health benefit to increased access to prenatal care as well as lower incidence levels of diarrhoea among households indigenous to the mining area. They also find robust positive local income effects due to gold mining coming from increased employment opportunities and cash earnings. Tolonen (2014) also finds decreasing child mortality especially for girls in gold-mining areas, likely due to women's better access to market opportunities and health care facilities.

[19] The positive benefits of a mining boom could in theory be offset by the crowding out of other local sectors providing tradable goods (e.g. manufacturing, commercial agriculture) but this is unlikely to be a problem in the eastern DRC which produces few tradable goods other than natural resources.

Overall the health and mining literature in Africa suggests that mining, at least when large scale and formal, is likely to improve child health outcomes in the mine-dependent communities through a combination of better access to health infrastructure and higher incomes for their parents, especially their mothers. The closing of a mine is likely to produce significantly worse outcomes for infant children than for other members of the family because women, their primary caregivers, are likely to experience large employment and income shocks.

4 Data for main empirical analysis

To test for the effects of Dodd–Frank on child mortality, we create a data set from three publicly available sources. In this section we describe the data and key variables.

4.1 Outcome variable: infant mortality

Data on infant mortality come from the Demographic and Health Survey (DHS) in DRC, of which we only use data from the five eastern Congo provinces. We employ the 'births recode' data set from the 2013–14 survey wave. In this wave, survey teams asked women to recall their complete birth history as well as the dates (month and year) of their children's deaths. The DHS data set also includes information on birth order, child gender, household size, and mother's education and marital status at the time of the interview. The survey teams conducted multiple interviews within each enumeration area (village). The data include geo-coordinates of the enumeration sites, but those geo-coordinates have been randomly altered within a 10 km radius to preserve the anonymity of survey respondents.

To mitigate potential error in mother's recall, we focus on recent history by creating a mortality measure spanning births occurring from 2007 through 2012. We focus on under-1 mortality because this infant mortality measure does not require us to make assumptions about the future mortality rate of children still alive at the end of the survey period. For example, we know definitively if a child born in 2012 reached one year of age by the end of 2013, but we do not know whether or not a child born in 2012 will reach five years of age. Each observation i, is a child for whom we construct a binary under-1 mortality variable, equal to 1 if the child died before his or her first birthday and 0 otherwise.

Due to the structure of the DHS recall data, the births during 2007–12 are linked spatially to the village of mother's residence when she was interviewed during 2013–14. The data set does not identify whether or not the mother moved from a different village during the period of our analysis, 2007–12. This feature of the recall data, while important to consider when interpreting data patterns, should not bias our estimates of Dodd–Frank when we include mother fixed effects, as we explain in Appendix 2.

Table 1 summarizes the under-1 mortality variable and the other relevant DHS variables used in the analysis. For all variables, the unit of analysis is the under-1 child. We observe the additional covariates at the level of the mother, and we observe the coordinates of the enumeration area (village) with random error. The mean of the dependent variable is 0.072, which implies an infant mortality rate of 72 deaths per 1,000 births.

4.2 Treated and control village groups

We employ a single 'policy' variable that assigns 'treatment' over time and across villages most directly affected by the conflict mining policies (see Figure 2). 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 simple choice has advantages. Importantly, it is likely inappropriate to consider the policies following up on Dodd–Frank as separate and independent events when the passage of Dodd–Frank likely triggered the subsequent policies as discussed in section 2.

We chose July 2010 as the time in which Dodd–Frank 'treatment' begins, although formal regulatory authority of Section 1502 was not exercised until later. For children born earlier than July 2010 in the affected villages, the treatment variable takes a fractional value ranging from 1/12 to 1 based on the proportion of a child's first year under the Dodd–Frank regime. Our choice to define treatment as beginning in July 2010 is based on the work of researchers who have argued that Dodd–Frank caused a de facto boycott of 3Ts shortly after it was passed, long before the more official boycott began in April 2011 (see e.g. Pöyhönen et al 2010; Seay 2012). In robustness checks, shown below, we demonstrate the main results are similar when we drop 2010 births from the sample.

We define a village as 'treated' if two conditions hold. First, the treated village must have geo-coordinates within the spatial 'policy zone' targeted by the conflict mineral policies. The policy zone is the union of space appearing on the US State Department's Dodd–Frank Section 1502 map of conflict mines and the three provinces subject to the mining ban.[20] The second condition is that treated DHS villages must be close in distance to at least one 3T mine that was operating prior to Dodd–Frank, as identified by the IPIS 2009 and 2010 mining location surveys. This second condition assumes that the de facto boycott and the related mining ban have disproportionately affected villages closest to 3T mines.[21]

To operationalize the 'close-in-distance' condition, we choose a distance threshold of 20km, which follows the distance used in the literature on mining impacts (e.g. Aragón and Rud 2015; von der Goltz and Barnwal 2014; de Haas and Poelhekke 2016). In our case, using a shorter distance would be inappropriate, given the 10km radius random error in DHS enumeration coordinates. Using a further distance would dilute the potency of the policy treatment and potentially open the regressions up to more unobservable sources of bias. In robustness checks, presented below, we show the main findings are similar at threshold distances of 10km and 30km, while effects diminish out to 40km and 50km.

Figure 2 illustrates the 'treatment' designation of all 201 DHS villages in the eastern DRC, with the treated villages depicted by dark circles. Figure 2 also shows the two sets of villages that comprise our counterfactual, 'untreated' villages. One counterfactual group of villages—depicted by dark squares—consists of those within 20km of a 3T mine, but outside the policy zone.3 Another counterfactual group of villages—depicted by light circles—consists of those within the policy zone, but further from 20km to a 3T mine. We employ all village types in our econometric analysis as described below.

We also identify villages within 20km of a gold mine inside the targeted policy zone and separately compare their mortality outcomes before and after Dodd–Frank with the mortality outcomes of villages within 20km of a gold mine. We do not anticipate any Dodd–Frank effect,

[20] This definition of the policy zone is consistent with the Parker and Vadheim's (2016) definition of the area most directly affected by the conflict mineral policies. The US State Department map is available at: https://hiu.state.gov/Products/DRC_MineralExploitation_2011June14_HIU_U357.pdf.

[21] To test for heterogeneity in the effects we also disaggregate our measure by whether the mine near a village was operated by an armed group or not. This provides a measure that is robust to whether the mine is intended to be treated or is treated by the boycott spillover effect.

however, because gold has been de facto exempt from the Dodd–Frank induced boycott as discussed in section 2. Figure 2 shows the spatial distribution of gold mines and Table 1 indicates that 31 per cent of the villages were within 20km of at least one gold mine.[22]

4.3 Rainfall seasons and shocks

Previous literature suggests that weather seasons and precipitation can affect child health and mortality in underdeveloped countries through increased disease burdens during wet periods (e.g. malaria exposure) and from increased or decreased agricultural production affecting nutrition (see e.g. Kudamatsu et al. 2012; Han and Foltz, 2015). To measure abnormal precipitation, we follow Maystadt et al. (2014) by constructing a standardized measure of rainfall for each child in the following way. First, we construct a three-month rolling average of precipitation at each DHS enumeration site using CHIRPS data.[23] Then we take the three-month average before the child's birth, subtract the mean of the three-month averages, and divide by the standard deviation of the three-month averages. This gives a standardized measure of relative rainfall before the child's birth. We employ an equivalent process to construct a standardized measure of rainfall during the three months after the child's birth. The resulting variables, shown in Table 1, have means of zero (by construction) and ranges from -2.54 to 4.50 for after birth and -2.62 to 4.02 for before birth.

To account for the possibility that rainfall seasons are also important determinants of child mortality, we have constructed indicator variables for wet and dry season patterns near each enumeration village. We identify the driest and wettest three months in each territory based on long-run precipitation averages. The 'wet season indicator' equals one if the child is born in a month for which the long-run average precipitation for that month ranks among the highest three. Similarly, the 'dry season indicator' is equal to one if the child is born in a month for which the long-run average precipitation for that month ranks among the lowest three. Table 1 shows summary statistics of these measures.

4.4 Control variables

We also control for factors, observed at the child and mother level that may affect mortality outcomes. At the child level we include an indicator for child gender and the variable Birth Order. Birth order measures the sequencing of the child within the family with a lower number indicating a later (younger) child, as a way of capturing sibling rivalry effects as found for example by Morduch (2000) in African countries. Household Size is measured at time of the interview as is the mother's years of education and her marital and literacy status.

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[22] If we were certain that Dodd–Frank had no effect on gold mining, we could consider the estimates of Dodd–Frank on gold-mining villages in the policy zone as placebo tests of its effects. However, it is likely that Dodd–Frank had at least some impact, albeit small, on gold mining and the DRC's mining ban may have also temporarily disrupted gold mining (see section 2).

[23] We use the Climate Hazard group InfrRed Precipitation with Station (CHIRPS) data archive to map each DHS cluster coordinate with daily and monthly precipitation estimations during the study period. We have selected CHIRPS dataset because it uses both new resources of satellite observations, such as gridded satellite-based estimations from NASA and NOAA, and also in situ precipitation gauge observations from ground stations to build a high resolution (0.05^0) estimation model (Funk et al. 2014). Second, we use CRU TS3.10 for daily and monthly average, min/max temperature estimation for 0.5^0 resolution. CRU TS3.10 updates previous CRU TS3.00 with observations at meteorological stations across the world's land areas up to December 2009. Station anomalies were interpolated into 0.5° latitude/longitude grid cells covering the global land surface (excluding Antarctica).

5 Main empirical analysis

In this section, we examine the reduced form impact of the conflict mineral policy on infant mortality. We begin with graphical evidence and then present econometric estimates.

5.1 Graphical evidence

Figure 5 compares trends in mortality rates for villages within 20km of at least one 3T mining site, with the solid line representing the mortality rate in the 'treated' villages depicted by dark circles in Figure 2. The dashed line represents the mortality rate in counterfactual villages within 20km of at least one 3T mine but outside the policy zone, depicted by the dark squares in Figure 2. Comparing the two lines, we see that mean mortality rates in both village groups were following the same parallel trends through 2009, the final year before the conflict mineral policies. Prior to 2010, mean levels of infant mortality were higher in the counterfactual villages. Beginning in 2010, the situation reversed such that mortality rates were higher in the treated villages, in the aftermath of Dodd–Frank. This figure provides visual indication that Dodd–Frank and the mining ban caused local increases in mortality, for treated villages relative to other villages near mines not targeted by the policy.

There are two other features of Figure 5 to highlight. First, a comparison of the dashed line mortality patterns in villages outside of the policy zone with the world price of tin (see Figure 4) suggests a strong, negative correlation between the two during 2007–12. The Pearson correlation coefficient is -0.80. This correlation suggests that infant mortality rates in villages near tin mines would have declined with exogenous increases in the world price, absent the Dodd–Frank induced boycott.[24] This is evidence that mining booms improve infant health. The second feature of Figure 5 is that infant mortality rates were lower in villages near 3T mines inside the policy zone, prior to Dodd–Frank. This may suggest that income benefits and/or living conditions near mines were in fact greater in the conflict mining zone.

Figure 6 compares trends in mortality rates for the treated villages with trends across a different set of counterfactual villages: those within the policy zone, but more than 20km from a 3T mine as depicted by the light circles in Figure 2. These counterfactual villages should be subject to the same regional trends in mortality, but differentially impacted by the conflict mineral policies. Comparing the two lines, we see that mean mortality rates in both village groups were following roughly similar trends through 2009. Prior to 2010, mean levels of infant mortality were higher in villages away from the 3T mines, which is consistent with infant mortality rates benefiting from being near an active mining industry. Beginning in 2010, the situation reversed such that mortality rates became higher in the villages close to a 3T mine. This figure provides visual indication that conflict mineral policies caused increases in mortality in villages close to 3T mining sites.

To summarize, Figures 5 and 6 indicate that mortality in the 'treated' villages increased after Dodd–Frank relative to both sets of counterfactual villages. The figures also show a declining pre-policy trend in infant mortality in the treated villages that was roughly comparable to pre-policy trends in the two possible counterfactual control village groups.

[24] The world prices of tungsten and tantalum also increased in the post-Dodd–Frank period relative to the pre-Dodd–Frank period. We focus on tin only because the majority of the 3T mines in the IPIS data were tin mines.

5.2 Econometric model and sources of identification

To implement formal tests, we estimate a linear probability triple difference-in-difference regression model of the causes of infant mortality in eastern DRC. For each child, *i*, born to mother, *m*, in village, *v*, located in territory, *t*, in province, *p,* in month, *k*, during year, *y*, we estimate the probability that the child dies in his/her first year. We estimate this probability of mortality as a function of whether the child's birth is in a village that is 'treated' by the Dodd–Frank policy by being less than 20km from a 3T mine, $3TInd_v$, within the Dodd–Frank policy zone, $PolicyZone_v$, after its passage in 2010, $postDF_{yk}$. Because the law also potentially affects gold mines, we also include an indicator for whether there is a gold mine near the village of a child's birth, $GoldInd_v$, which receives the same difference-in-difference treatment as the 3T mining village indicator.

In order to control for potentially confounding effects we include a number of covariates as well as fixed effects. The covariates include mother level variables, Xm, which include age, literacy and marital status; child level variables on birth order, Orderi and gender Malei; and two village and month of birth measures of weather and climate denoted by the variable Rainvk. In terms of fixed effects we include indicators for birth month, ω_k, to control for possible seasonal patterns in mortality, and use either mother, ϕ_m, or village level, α_v, fixed effects to control for time-invariant mother and location effects.[25] We also include variables in all regressions to capture time-dependent effects. Depending on the regression the included time effects are either: year dummies, μ_y, which capture area wide year effects for infants born during 2007–12; provinces specific year effects, $\delta_{y\times p}$, to allow time effects to adjust flexibly to regional patterns; or territory specific linear time trends for each of the 70 territories, $\sigma_t y$.[26]

Putting all of those variables and effects into an equation in their most general form produces equation (1) below:

$$\begin{aligned} mortality_{imtpyk} &= (\alpha_v \text{ or } \phi_m) + \omega_k + (\mu_y \text{ or } \delta_{y\times p} \text{ or } \sigma_t y) + \pi Order_i + \upsilon Male_i + \eta Rain_{vk} + \gamma X_m + \\ &\beta_1(postDF_{yk} \times PolicyZone_v) + \beta_2(postDF_{yk} \times 3TInd_v) + \beta_3(postDF_{yk} \times 3TInd_v \times PolicyZone_v) \\ &+\beta_4(postDF_{yk} \times GoldInd_v) + \beta_5(postDF_{yk} \times GoldInd_v \times PolicyZone_v) + \varepsilon_{imtpyk}. \end{aligned} \tag{1}$$

where *i* = child, *m* = mother, *v* = village, *t*= territory, *p*= province, *y* = year of birth, and *k* = month of birth.

The β coefficients are of primary interest, particularly β_3, which is the triple difference estimate of the policy treatment effect, where $\hat{\beta}_3 > 0$ implies Dodd–Frank increased mortality rates in 3T mining villages within the policy zone. The coefficient β_1 measures the difference in mortality

[25] Due to multicollinearity issues, we are not able to use both mother and village level effects in the same regression. When we use mother level effects, α_v, we also drop the mother level controls, X_m.

[26] We have also estimated a more flexible version of the model, to allow each of the 70 territories to have unique year effects. Although those models generate similar results, we do not present them here because the identification of the policy effects in that model only comes from village-level variation in Dodd–Frank treatment in a few territories (see Figure 2, right-hand panel).

before and after Dodd–Frank at a broad and diffuse 'policy-zone' geographic level; we expect $\hat{\beta}_1$ =0 because the policy is unlikely to have had a measurable impact on mortality outside of mining villages. The coefficient β_2 measures the pre and post-Dodd–Frank difference in mortality for all villages within 20km of 3T mines. We have no a priori expectation of the sign of $\hat{\beta}_2$; the graphical evidence described above suggests the high world prices of tin during 2010–12 decreased infant mortality in 3T mining villages outside of the policy zone, but Dodd–Frank likely suppressed the benefits for mining villages inside the policy zone. The coefficient β_3 measures any additional pre and post-Dodd–Frank difference in mortality for the subset of 'treated' 3T villages within the policy zone. Finally, the coefficient β_5 represents the policy effects on villages near gold mines within the policy zone. Because these gold mines were effectively exempt from Dodd–Frank, we expect $\hat{\beta}_5$ =0.

The variation used to identify policy effects in our model depends on which set of fixed effects we employ. The most demanding specifications include both mother fixed effects and province specific year effects or territory specific trends. These specifications, however, rely on rather narrow, within mother and within province variation in treatment for identification of the key parameter, β_3. By contrast, the specifications with village fixed effects and only area wide year effects are less demanding but rely on broader variation in village outcomes—i.e. variation across villages throughout the entire study area—for identification of β_3. Rather than choosing a favoured specification, we present the full range of results, but we generally interpret results based on broadest sources of variation.

5.3 Main results

Table 2 shows our main estimates of a linear probability model for under-1 mortality with standard errors clustered at the village level to account for possible serial correlation within villages (Bertrand et al. 2004). Columns 1, 3, and 5 include village fixed effects whereas columns 2, 4, and 6 include mother fixed effects. In the columns with mother fixed effects, the identification of $\hat{\beta}_3$ comes from within mother variation in mortality and hence relies on mothers who had births before and after Dodd–Frank. Columns 1 and 2 include year effects, columns 3 and 4 include province specific year effects, and columns 5 and 6 include territory specific linear trends. All columns include month of birth fixed effects and controls for rainfall, birth order, and infant gender. The specifications with village fixed effects also include time-invariant controls for household size, mother education, marital status, and literacy.

Turning to the key results, $\hat{\beta}_3$ is positive and statistically different from zero in all six specifications. This result indicates that under-1 mortality rates after Dodd–Frank increased in the treated villages relative to the counterfactual villages. For perspective on magnitudes, the mean mortality probability in the treated villages prior to Dodd–Frank was 0.060. Hence, after controlling for other factors, the column 1 estimate of 0.086 implies that Dodd–Frank increased under-1 mortality probabilities by 143 per cent.

The $\hat{\beta}_2$ estimates are significant and negative in four of six specifications, suggesting that being close to a 3T mine would have lowered relative mortality rates in the absence of Dodd–Frank. The finding that $\hat{\beta}_3 + \hat{\beta}_2 > 0$ indicates that mortality inside the policy zone increased in 3T mining villages relative to non-3T mining villages as suggested by Figure 6. The increase, based on column 2, is 0.086 – 0.060 = 0.026. This is a 43 per cent increase relative to the pre-Dodd–Frank mean of 0.06 in the treated, 3T mining villages.

The fact that the *Post DF x Gold Ind. x Policy Zone* coefficients, $\hat{\beta}_5$, are effectively zero in all specifications also bolsters the evidence that a Dodd–Frank-induced boycott, rather than confounding factors, caused the under-1 mortality increase. Dodd–Frank did not cause a widespread slowdown in gold mining and hence we do not expect the policy to have had an adverse impact on mortality in villages near gold-mining sites through the income channel. If confounding factors (specific to the policy zone or to mining-dependent villages) were driving our estimates of $\hat{\beta}_3$ we would expect them to be present in gold-mining areas and produce significant effects on $\hat{\beta}_5$, which we do not see.

Turning to the covariates in Table 2, which are not our main focus, we note the following patterns. First, abnormal rainfall after birth increases mortality rates whereas rainfall pre-birth decreases mortality. Abundant pre-birth rainfall may lower mortality rates by increasing food and income via improved post-birth agricultural harvests. Abundant post-birth rainfall may raise mortality rates by increasing risk of diseases such as malaria and diarrhoea, or by making medical and food supplies more difficult to transport under wet conditions. Second, birth order positively correlates with mortality, meaning that earlier births have higher mortality, perhaps because first births in the DRC are often to younger, inexperienced teenage mothers. Third, male infants have higher mortality rates; this finding is consistent with a large literature in epidemiology on male mortality disadvantage (see e.g. Drevenstedt et al. 2008). Fourth, larger sized households have lower mortality rates, perhaps because larger households have more people in them to take care of infant children, as found, for example, in Han and Foltz (2015).

5.4 Robustness checks

Table 3 shows the main results pass three important robustness checks. In panel 2, we substitute the 3T and Gold Indicator variables with 3T and Gold variables that measure the number of mines within 20km of a village. This modified specification allows the treatment effect to vary with the intensity of pre-Dodd–Frank mining, assuming the number of mines is a good proxy for mining intensity. The results show the estimated impacts of the policies on mortality increases with the number of nearby 3T mines. The column 1 coefficient of $\hat{\beta}_3$=0.016, for example, suggests that mortality probabilities increased by 0.016 for every nearby 3T mine. For treated villages, the mean number of nearby 3T mines was 4.17. Hence, the effect for the average treated village was 4.17 x 0.016 = 0.067. This is a 111 per cent increase in mortality relative to the 0.06 pre-Dodd–Frank mean in these villages.

Panel 3 omits births during 2010 to test the sensitivity of the main results to our assumption that Dodd–Frank treatment began in July 2010, before the formal electronics industry boycott. As the results indicate, the treatment effect remains positive, large, and statistically significant. We conclude that our main inferences do not hinge on the assumption that treatment began with the passage of Dodd–Frank.

Panel 4 allows for spatial correlation in standard errors across villages within the same territories by clustering at the territory rather than village level. Although the standard errors increase in some of these specifications, the coefficients remain statistically significant in four of six specifications, including the most demanding specifications with mother fixed effects. In columns 1 and 3, the p-values on the treatment coefficients are close to p<0.10, at 0.14 and 0.11 respectively.

In Table 4, we examine the robustness of the main results to different distance criteria for defining the mining status of villages. Here we define that status as within 10km, 20km, 30km, 40km, and 50km of a 3T mine. Table A1 in the appendix shows the number of treated and control villages and births at these different distance thresholds. As panels 1–3 of Table 4 illustrate, the estimated treatment effects decline with distance thresholds, moving from 10km to 50km. This is an empirical pattern consistent with the income benefits from mining declining with village distance from a mine. The empirical pattern also suggests that any local effect of 3T mining pollution on under-1 mortality—such as those effects that may be realized only within 10km of a mine—are dominated by mining related income effects.

To summarize the results in this section, the findings are consistent with Dodd–Frank, and the related policies, causing a statistically significant and quantitatively large increase in under-1 mortality. The effect is robust to the inclusion of mother fixed effects, different spatial and temporal definitions of 'treatment', and to accommodation of region and territory specific trends in mortality. In most cases, tests of gold mining fail to generate a similar pattern of results. This collection of results makes a strong case that a Dodd–Frank induced boycott, rather than confounding factors, caused the higher mortality. In the next section, we examine potential channels through which mortality increased.

6 Channels from legislation to higher infant mortality

In section 3, we described three main channels through which the conflict mineral policies may have increased infant mortality. First, the policies may have caused more conflict in mining areas. Second, the policies may have reduced income streams to families and communities previously dependent on artisanal mining. Third, the policies may have reduced availability and access to medical care. We examine the channels below, after first arguing that the findings are unlikely to be explained by selective migration out of 3T mining villages after Dodd–Frank.

6.1 Selective migration

The conflict mineral policies likely caused migration out of the treated villages; however, to our knowledge, there are no data sources that track such migration over our study period. Hence, we are unable to directly assess actual migration patterns. Because the mortality data are based on mother recall—and all 2007–12 births are considered to be at the location of a mother's residence in 2013–14—we are confident that selective migration does not substantially bias our treatment effect estimates when we control for mother fixed effects.

We explain our reasoning in more detail in Appendix 2, but here we lay out the main arguments. If Dodd–Frank caused healthier-than-average families to migrate out of 3T mining villages during 2010–12, then the solid line in Figures 5 and 6 understate the true mortality rates of this group, both before and after Dodd–Frank. Although levels of mortality rates are affected by this type of selective migration, the before-versus-after Dodd–Frank *difference* in mortality is not affected because all birth outcomes are attributed to the same, 2013–14 mother's village. This is important because our econometric strategy with mother fixed effects identifies treatment effects from differences, rather than levels, of mortality outcomes before and after Dodd–Frank from the subset of mothers who had children during both time periods. Similarly, if Dodd–Frank caused less-healthy-than-average families to migrate out of 3T mining villages during 2010–12, then the solid line in Figures 5 and 6 would overstate true mortality. But again, the before-versus-after Dodd–Frank difference in mortality is not affected because of the recall feature. Based on this simple reasoning—which we elaborate on in more detail in Appendix 2—we conclude that selective migration is unlikely to be an important explanation for our main findings.

6.2 Armed conflict

To test for the extent to which armed conflict was an important channel through which Dodd–Frank increased conflict, we add child-specific conflict measures to the right-hand side of the econometric model described in Equation (1) above. If conflict is an important channel, then we expect the treatment effect coefficient to decrease once we add controls for conflict. We measure the number of armed conflicts within a 20km radius of each DHS village during the 12 months following each child's birth using data from ACLED.[27] This dataset provides information on internal conflict disaggregated by date, location, and by actor or actors for a number of African countries, including the DRC.[28] We measure the number of conflicts in total, as well as measures disaggregated into 'battle' and 'violence against civilians' categories. Table A2 in Appendix 1 shows summary statistics. The mean number of conflicts for each of the 7,697 infants in our study was 5.01 over 2007–12, with a standard deviation of 15.26. Of the children in the sample, 36 per cent were exposed to at least one conflict event (within 20km) within the first 12 months following their birth.

Table 5 shows the results. Some specifications employ aggregate measures of conflict and other specifications employ the disaggregated measures. Other specifications include indicators for whether or not a conflict event occurred, rather than the number of conflict events. Regardless of how conflict is measured, the Table 5 results indicate that controlling for conflict has almost no impact on the size of the policy treatment coefficients. Overall, the Table 5 estimates suggest that conflict was not an important channel through which Dodd–Frank increased infant mortality.

The coefficient estimates on the conflict variables in Table 5 provide some evidence that conflict exposure within the first year increases infant mortality in terms of statistical significance, though less clearly in terms of magnitudes. To put these estimates into perspective, consider the panel 2, column 5 coefficient of 0.00074 (which is rounded up to 0.001), which means an additional conflict within 20km of a child's village (during the 12 months after birth) is associated with a 0.00074 increase in the probability of an infant dying.

Because Dodd–Frank was advanced as a policy to reduce violence, it is worth considering how much the policy would have had to reduce conflict in order to offset the increased infant mortality caused by the policy through other channels. Suppose Dodd–Frank was successful at decreasing conflict incidents by three standard deviations so that infant mortality probabilities declined by 15.26 x 3 x 0.00074 = 0.034. While this would be an impressive decline in mortality, it would still not offset the 0.086 increase in infant mortality that our estimates ascribe to the

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[27] We chose the ACLED data set, rather than alternatives such as the Uppsala Conflict Data Program (UCDP) because the ACLED data set measures more events—such as those not resulting in fatalities–that could affect infant mortality.

[28] The ACLED data are available at 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 three 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. Parker and Vadheim (2016) find evidence that Dodd–Frank increased conflict in the eastern DRC, as measured by ACLED events.

Dodd–Frank boycott, based on the column 1 baseline treatment effect. To offset the boycott effect, the legislation would have had to eliminate 0.086/0.00074=116 conflicts during the child's first 12 months. But this magnitude of reduction would be possible for only 29 out of 7,697 (0.4 per cent) child-birth observations in our sample that were actually exposed to 116 or more conflict events. We can make similar calculations based on the largest coefficient estimate on conflict, which is the panel 3, column 1 coefficient of 0.0016 on the number of battle events (rounded up to 0.002). Based on that coefficient, the legislation would have to eliminate 54 battles, which would be possible for only 0.5 per cent of the child-birth observations in the sample.[29]

To summarize, we conclude that an increase in armed conflict was not a key channel through which conflict minerals policies increased infant mortality. We also find suggestive evidence that armed conflict, while bad for infant mortality, is of second order importance relative to other mortality risks in the eastern DRC including, potentially, the extent to which mothers can consume health care goods and services.

6.3 Consumption of health goods and services

To study the health consumption channel, we cannot rely on only the 2013 DHS survey wave because it contains only cross-sectional information about a mother's income and health, except for recall data on child mortality. Instead, we draw from both the 2007 and 2013 DHS survey waves. Using both surveys, we create a data set of health care consumption outcomes covering pre- and post-Dodd–Frank time periods. The data set is similar to, but not quite, a repeated cross section of villages because different villages within similar enumeration areas are represented in the different survey years. This means we cannot include village (or mother) fixed effects in econometric models.

The 2007 and 2013 DHS surveys include several variables that are candidate measures of a mother's wealth and health care consumption. The candidate outcomes that we examined are: 1) a wealth index based on an assessment of family assets; 2) whether or not the mother (presumably with infant) slept under a bednet; 3) whether or not the mother had prenatal care for her most recent birth; 4) whether or not the mother visited a health facility while pregnant for her most recent birth; 5) whether or not the mother reported money as being a constraint to getting prenatal care; 6) whether or not the mother reported distance to a health care facility as being a constraint to getting prenatal care; and 7) whether or not the mother received birth assistance from a health care practitioner.

To trim this list down to the subset of variables most likely to affect under-1 mortality, we performed the following analysis. First, we added each of the seven 2013 survey outcomes, individually, to the right-hand side of the columns 1, 3, and 5 regression specifications shown in Table 2. The purpose was to identify which outcomes are most robustly related to infant mortality. Note that this exercise is possible for the specifications with village fixed effects, but it is not possible for the specifications with mother fixed effects because these are mother level variables. The results show the following variables to be related to infant mortality: a) whether or

[29] These comparisons are informative but estimating the causal effect of conflict on infant mortality is not our main focus. By contrast, a working paper by Dagnelie et al. (2015) focuses on trying to estimate the effect of conflict during the 1997–2004 DRC civil war on infant mortality. They conclude that conflict affected mortality for girls, but not for boys. For girls, their coefficients appear to be larger in magnitude than ours but the results are not directly comparable because their analysis is at a more aggregated spatial level—the 38 districts of entire DRC—and because their study spans a period of war when mortality rates were generally higher.

not the mother slept under a bednet; b) whether or not the mother had prenatal care; c) whether or not the mother had a prenatal visit at a health facility.[30] All of these indicator variables, which equal 1 if yes, have negative relationships with infant mortality probabilities at the mother level. Because the prenatal care and prenatal visit variables are highly correlated (ϱ=0.94), we analyse only prenatal care, but the results are nearly identical for prenatal visits.

Table A3 in the appendix shows summary statistics for the two outcomes we analyse. The literature shows strong evidence that mothers sleeping under bednets reduces diseases such as malaria that can cause infant mortality (Killeen et al. 2007), but also that purchase and use of bed nets in African countries is very price sensitive (Dupas 2009, 2014). This latter feature of bednets implies that it is a health good susceptible to income shocks, such as those potentially caused by a Dodd–Frank-induced boycott. In the eastern DRC sample, only 19.3 per cent of the mothers slept under bednets in 2007 compared to 58.3 per cent in 2013. This growth is consistent with the general increase in bednet use across Sub-Saharan Africa during the same time frame due to better availability, lower prices, and concerted information campaigns. There is less of an upward trend in prenatal visits in part due to already high levels in the baseline 2007 data; 83.7 per cent of mothers reported prenatal care for their most recent birth in the 2007 survey compared to 87.3 per cent in the 2013 survey. Note that, for the prenatal care variable, we compare births occurring in 2007 to births occurring during 2010–12 to most precisely capture the potential Dodd–Frank effects.

To study the potential effects of the conflict minerals legislation on these outcomes, we estimate a regression model similar to our main regression estimates in Equation (1), although we cannot include mother or village fixed effects. Another key differences is that here we do not control for weather shocks because we cannot link the timing of bednet use and prenatal care to these shocks. Aside from these key differences, however, the econometric specifications are similar to those in Equation (1).

More specifically, we estimate the following econometric model.

$$\begin{aligned} outcome_{mvtpy} &= \alpha_t + \ (\mu_{2013} \text{ or } \delta_{2013\times p}) + \gamma X_m + \lambda_1(3TInd_v) + \\ &\lambda_2(3TInd_v \times PolicyZone_v) + \lambda_3(PolicyZone_v \times y_{2013}) + \lambda_4(3TInd_v \times PolicyZone_v \times y_{2013}) \\ &+\omega_1(Gold_v) + \omega_2(Gold_v \times PolicyZone_v) + \omega_3(GoldInd_v \times PolicyZone_v \times y_{2013}) + \varepsilon_{mvtpy}. \end{aligned} \quad (2)$$

Here m = mother, v = village, t = territory, p = province, and y = 2007 or 2013. The term α_t denotes fixed effects for each of the 70 eastern DRC territories. The term μ_{2013} denotes the individual, area wide year effect for the 2013 survey, while other specifications allow each of the five provinces to have their own 2013 effects ($\delta_{2013\times p}$).

The λ coefficients are of primary interest, particularly λ_4, which is the triple difference estimate of the policy effect on bednets and prenatal health usage. A finding that $\hat{\lambda}_4 < 0$ implies that a mother's consumption of health care goods and services decreased over 2007-2013 in treated villages, relative to consumption in the control group villages. As before the control group

[30] While it may seem surprising that the DHS assets index, the only available measure of income, is not strongly correlated with infant mortality outcomes, this is likely because the asset index measures much longer-term accumulation including measures of a number of non-liquid assets that are not available to feed or care for children, such as the type of roof, floor and plumbing.

villages are a) within 20km of 3T mines outside the policy zone and b) inside the policy zone but >20km from a 3T mine.

Table 6 shows the results from linear probability estimates of Equation (2). All specifications include mother level covariates such as education, household size, marital status, literate, and age. Columns 1–2 include area wide 2013 effects and columns 3–4 include province specific 2013 effects. All standard errors are clustered at the territory level.

The key coefficients, $\hat{\lambda}_4$, are negative in all specifications and statistically significant in three of four specifications. Although these specifications are not as rigorous as the mortality estimates, the evidence in Table 6 is consistent with Dodd–Frank increasing infant mortality through a reduction of mother consumption of prenatal care and disease-reducing bednets. Recalling the general trend of rising bednet use, the coefficient of -0.182 means that bednet use was 18.2 percentage points lower in treated villages relative to how much bednet use would have grown without the Dodd–Frank-induced boycott. The negative coefficients on prenatal care also suggest that its use was lower than what would have been consumed in the absence of Dodd–Frank. Given the price sensitivity of bednet usage found in Africa by Dupas (2009, 2014), it is plausible that negative shocks to family income induced by the Dodd–Frank boycott drove the decreases in health care consumption. The decreases in health care consumption could have also been driven by reductions in access to or increases in the price of health care goods and services in the treated villages, due to a decrease in the transport of goods and services into those villages as their economic importance declined.

The other coefficient estimates in Table 6, which are not our focus, make intuitive sense. The positive and generally significant $\hat{\lambda}_2$ coefficients suggest that mother's consumption of health care increased more than average in villages near 3T mines, perhaps because of the high world prices of 3Ts during 2010—12. These positive estimates on $\hat{\lambda}_2$ provide a counterfactual for what may have happened to health care consumption near 3T mines in the absence of Dodd–Frank. The coefficients on the mother level variables all sensibly show that measures of human capital (e.g., education, literacy, age) relate positively to health care consumption.[31]

While we believe that our infant mortality results—particularly those with mother fixed effects - are robust to migration, the negative estimates of $\hat{\lambda}_4$ could possibly be explained by selective migration out of 3T mining villages during 2007–13. If mothers motivated to seek health goods moved away from treated villages after Dodd–Frank, for example, then part of the measured decrease in health care consumption could be explained by selective migration (see Appendix 2). While we cannot rule out this explanation, we note that all specifications include mother human capital covariates that should account for much of the selective migration effects. Moreover, because selective migration is unlikely to explain the main infant mortality results, it strikes us as also plausible that selective migration does not explain the observed reduction in health care consumption. It is, though, possible that the observed reduction in health care consumption actually understates the true value because of selective migration, which would be the case if mothers who were less inclined to seek health goods tended to disproportionately leave treated villages after Dodd–Frank.

[31] The estimates of the gold mining treatment coefficients, $\hat{\omega}_3$, are statistically related to the outcomes in several specifications but the signs and significance follow erratic patterns and are sensitive to our treatment of time effects. For these reasons, we are not confident that the estimated relationships reflect true, robust patterns and therefore do not make causal claims based on them.

To summarize the findings in this section, the available evidence suggests the conflict minerals legislation increased infant mortality in 3T mining villages during 2010–12 in part through a decrease in mothers' consumption of health care goods and services. Such a reduction in health care goods and services usage could have been caused by the boycott's negative shock to family income or because it increased the cost or reduced the availability of medical goods and services.

7 Discussion: mortality from intended and unintended boycotts

The evidence thus far indicates the conflict mineral policies caused increased infant mortality in 3T villages within the policy zone, and that this increase was plausibly driven by a decrease in mother health care consumption. In this section, we consider heterogeneous infant mortality effects across these 'treated' villages. The key source of heterogeneity is the presence of an armed group at a mine prior to the Dodd–Frank legislation, which sought to 'cut off funding to people who kill people' (Aronson 2011). Although one might consider higher infant mortality near mines with an armed group as necessary collateral damage from a well-targeted policy, the infant mortality effects near non-targeted mines operated by non-targeted operators falls into another category of unintended consequences: poor targeting.

Table 7 shows tests for heterogeneous mortality effects, based on the number of 3T mines with and without an armed group presence within 20km of a policy-zone village.[32] Panel 2 shows that point estimates of mortality effects are larger for 3T mines with an armed group presence in 5 out of 6 specifications. The point estimates, however, are also positive and statistically significant for 3T mines without an armed group presence and, in most specifications, not statistically different from the estimates for 3T mines with armed groups. These 3T mines lacking armed groups were not explicit targets of Dodd–Frank but, based on these results, they appear to have also lost human lives by being subjected to a de facto boycott.

Panel 3 further disaggregates the 3T mining policy villages by separating the armed groups into two categories: rebel and government militias.[33] The explicit goal of Section 1502 was to reduce funding to illicit armed militias (i.e. rebel groups). In contrast to rebel groups, the DRC's government militias may have been considered legitimate actors by companies who attempted to acquire conflict-free minerals and thus potentially exempt from the policy. In both panels, consistent with rebel-controlled mines being the most directly targeted by the boycott, the point estimates of mortality effects are largest for the rebel-controlled mines because there are smaller statistically significant infant mortality effects from being near government militia controlled mines. As in panel 2, however, the differences in point estimates between armed groups and other mine owners are not always statistically significant suggesting the boycott was blunt and imprecisely targeted.

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[32] Of the 23 villages near a 3T mine in the zone, 14 were near at least one with an armed group presence and 15 were by at least one mine without an armed group presence. There were 7 policy zone villages <20km from both types of 3T mines. At the mine level, there were 213 3T mines in the policy zone and 34.3 per cent had an armed group presence.

[33] The rebel armed groups in the IPIS maps (see Spittaels and Hilgert 2009; Spittaels 2010), include the Forces for the Liberation of Rwanda (FDLR), the Forces Républicaines Fédéralistes (FRF), and Mayi Mayi militias (an umbrella term for loosely affiliated groups of local militias). The government armed groups include the Armed Forces of the Democratic Republic of Congo (FARDC) and the National Congolese Police (PNC). Of the 14 villages near at least one armed group controlled 3T mine, 8 were near a rebel controlled mine and 11 were near a government controlled mine. Four villages were near both types of 3T mines. At the mine level, 29 per cent of the 3T mines in the policy zone were visited regularly by rebel groups.

8 Conclusion

When citizens of developed nations pressure their governments to protect vulnerable populations in foreign lands, they often request economic sanctions to punish human rights violators with the goal of generating positive change. In the case we study, US human rights advocacy groups successfully lobbied for Section 1502 of the US Dodd–Frank Act, which effectively reduced international demand for Congolese 3T minerals. While this human rights policy appears to have had its intended effect of reducing militia revenue from 3T mining, evidence here indicates it has also produced unintended consequences that call into question whether the policy has benefited the victims of the human rights abuses.

The problem our paper highlights is that Dodd–Frank's successes in slowing 3T mining have generated two kinds of unintended consequences. First, infant mortality rates rose in villages whose economic fates are tied to intentionally boycotted, armed group controlled 3T mines. Second, infant mortality rates also increased in villages whose economic fates are tied to unintentionally boycotted, and potentially 'conflict free', minerals. This second type of unintended consequence is arguably more regrettable, and suggests important flaws in this and potentially other conflict-free certification programmes. High transaction costs of following supply chains from source to product have produced unintentional boycotts. Rather than absorbing these costs and the associated regulatory burdens, many companies have preferred to source elsewhere, which has had unfortunate effects on child health.

These results should serve as a cautionary tale for policy makers considering economic sanctions or certification programmes. The general problem is that it is difficult to successfully withhold economic transactions from perpetrators of human rights abuses without the brunt of the effects being absorbed by vulnerable populations. The findings are particularly stark in that they elucidate a case in which continued mining seemed preferable to local populations even in a setting with armed militias and weak formal governing institutions. While clearly not the final word on the subject, our results suggest that, in terms of infant mortality, while living close to a mine that helps finance armed militias may not lead to the best outcomes, it is even worse for that mine near one's home to be shut down or boycotted.

Although our research casts a dim light on the success of the Dodd–Frank human rights policy, it does not imply that such unintended consequences are inevitable. A better-targeted certification programme could, potentially, cause less collateral damage, especially if it were offset with commensurate healthcare and income aid to help families near mining villages. Another important qualifier is that we focus on the short-run aftermath of the conflict minerals policies, for children born through 2012. Advocates of Dodd–Frank Section 1502, for example, argue the conflict minerals legislation needs more time to work and it is possible that the long-run outcomes will improve. But if infants have died due to the policy, then the policy has already had a long-run effect through its effect on future human capital stocks. We hope that future research can measure longer-run outcomes.

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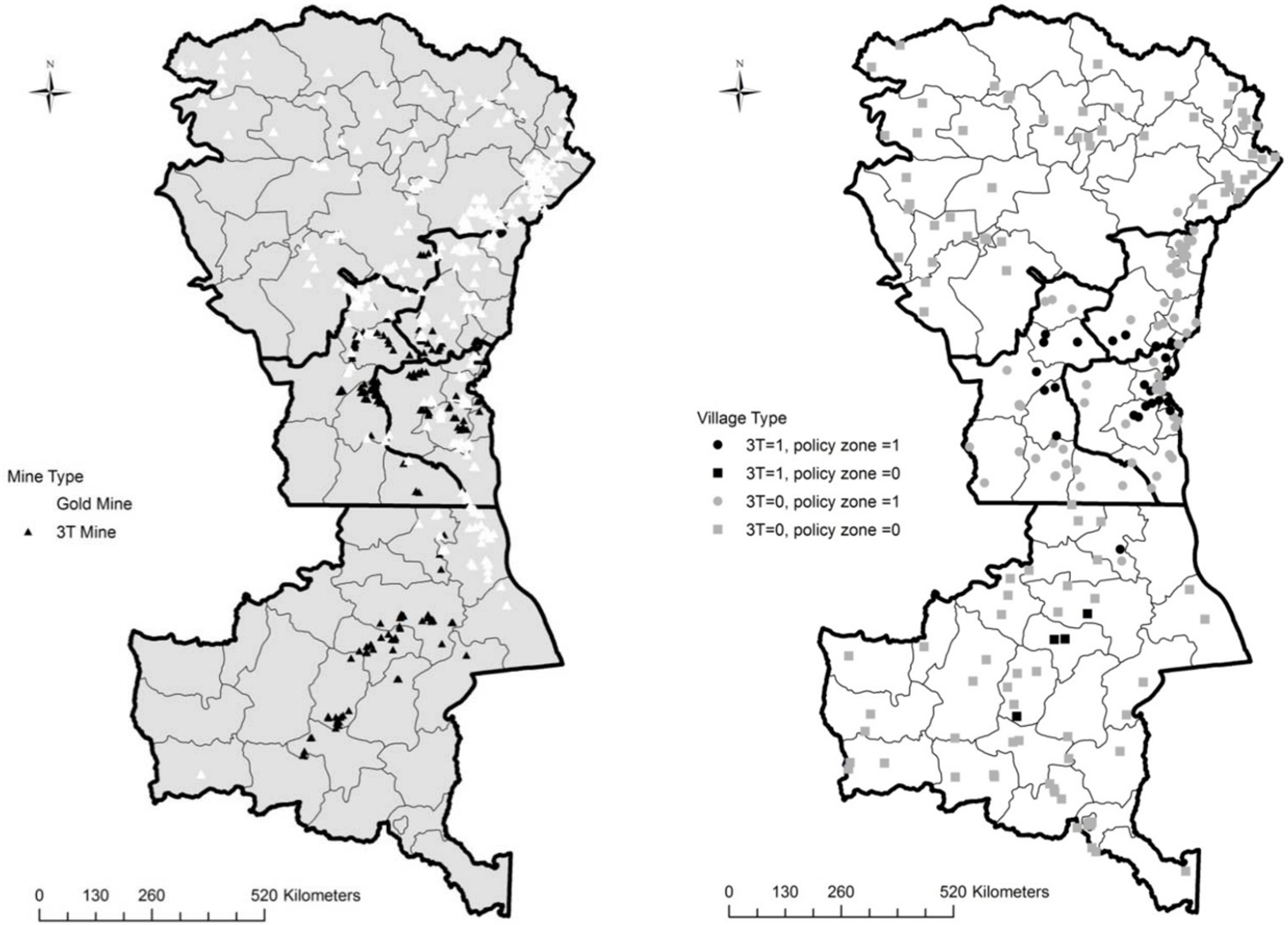
Figures and tables

Figure 1: Study area (the provinces of the Eastern DRC)

Notes: The provinces of the eastern DRC are Katanga, Maniema, South Kivu, North Kivu, and Orientale.

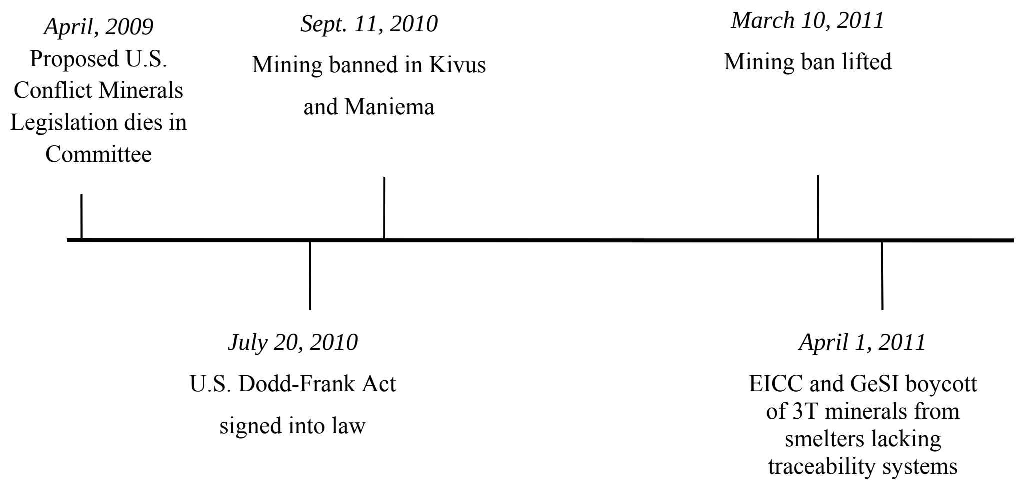
Source: see text.

Figure 2: IPIS mines and DHS villages in the Eastern DRC



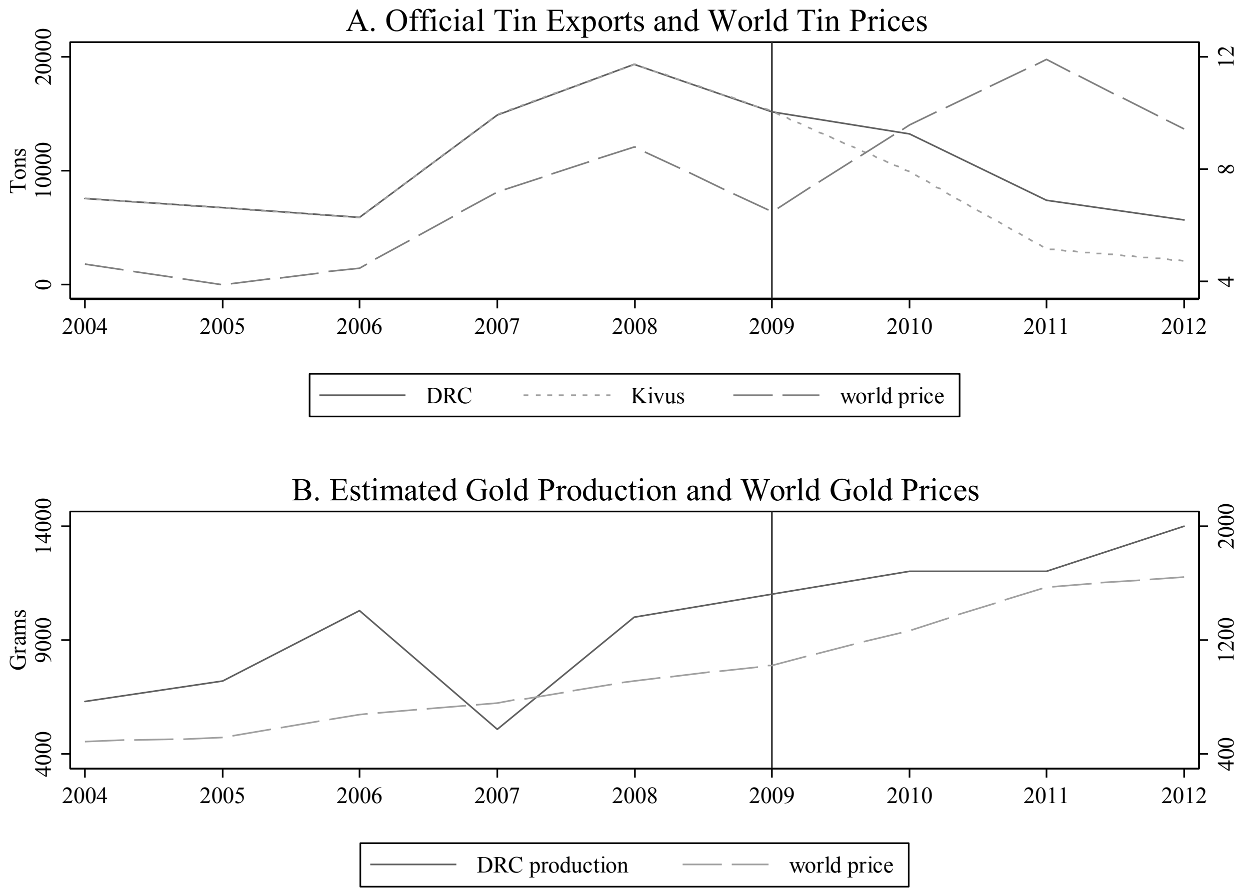
Notes: The dark coloured lines outline the provinces of Katanga, Maniema, South Kivu, North Kivu, and Orientale. The light lines outline the 70 territories in these five provinces. The 'Policy Zone' comprises the union of villages in provinces where mining was banned (i.e. Maniema, North Kivu, and South Kivu) and those with geo-coordinates falling within the US 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.

Figure 3: Timeline of key regulations



Source: see text.

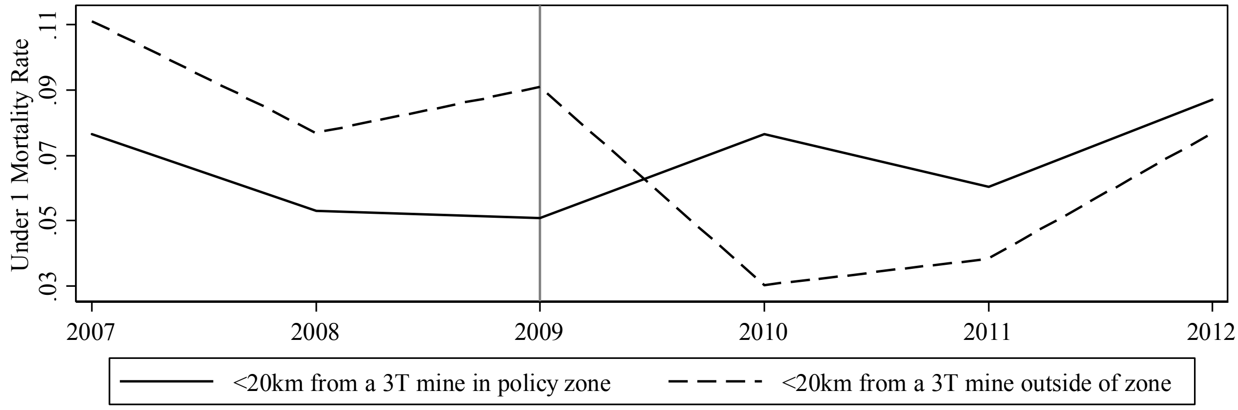
Figure 4: Mineral exports and production



Notes: The vertical line at 2009 signifies the final year prior to the conflict mineral policies. The axis on the left side indicates the official exports of tin, in tons from the DRC. All of the exports were sent from North and South Kivu (the 'Kivus') prior to 2010. After 2009, some of the exports were sent from Katanga. The difference between the Kivus and DRC lines indicates the tons exported from Katanga. The world price of tin, per pound, is shown on the right-hand side axis. The tin price is the monthly average of the cash official price paid by buyers on the London Market Exchange, in 2012 USD per ton. The gold price represents the monthly average of the PM spot prices on the London Market Exchange, in 2012 USD per gram.

Source: The export and production data i come from USGS Mineral Yearbooks Reports, 2008–12, available at http://minerals.usgs.gov/minerals/pubs/country/2012/myb3-2012-cg.pdf (accessed on 15 November 2016).

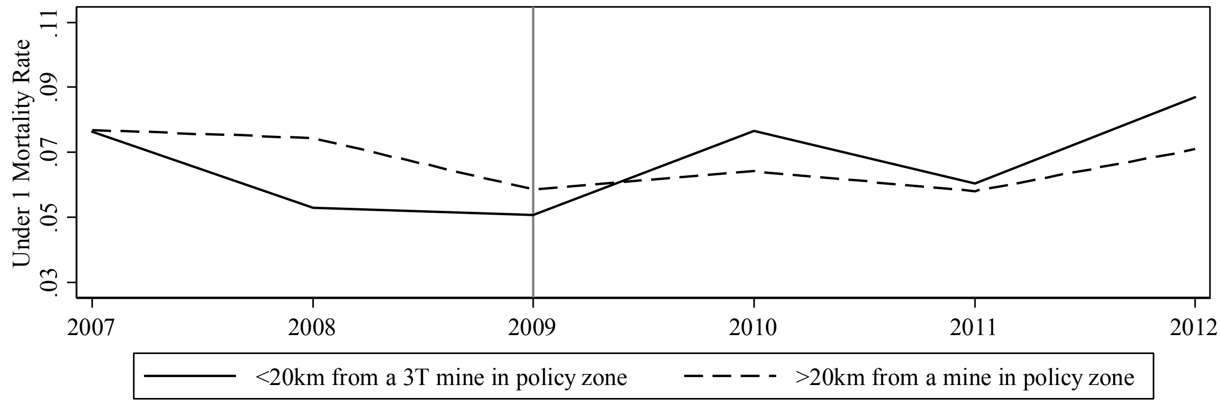
Figure 5: Mean mortality rates across DHS Villages that are within 20km of a 3T mine, inside versus outside the policy zone



Notes: The vertical line at 2009 signifies the final year prior to the conflict mineral policies. There were 1,152 births in DHS villages within 20 km of a 3T mine during 2007–12. Of these births, 992 were spread across 23 DHS villages inside the policy zone and 160 were spread across 4 DHS villages outside the policy zone.

Source: see text.

Figure 6: Mean mortality rates across DHS villages in the policy zone, near versus distant to 3T mines



Notes: The vertical line at 2009 signifies the final year prior to the conflict mineral policies. There were 3,499 births in DHS villages within the policy zone during 2007–12. Of these births, 992 were spread across 23 DHS villages within 20 km of a 3T mine and 2,507 were spread across 67 DHS villages farther than 20 km from a 3T mine.

Source: see text.

Table 1: Summary statistics

	Mean	St. Dev	Min	Max	Description
Under-1 mortality[a]	0.072	0.259	0	1	=1 if child died within first 12 months of life, 0 otherwise
Policy zone[b]	0.454	0.498	0	1	=1 if DHS village is in policy zone (see Figure 2), otherwise = 0
3Ts [c]	0.657	1.992	0	11	# of Cassiterite, Coltan, and Wolframite mines within 20 km of DHS village
3T indicator[c]	0.149	0.357	0	1	= 1 if 3T 20 km is nonzero, 0 otherwise
Gold[c]	1.138	2.944	0	22	# of Gold mines within 20 km of DHS villages
Gold indicator[c]	0.311	0.462	0	1	= 1 if Gold 20 km is nonzero, 0 otherwise
Post DF	0.433	0.463	0	1	= Proportion of 12 months post-birth during which Dodd–Frank was law
Post DF *x* 3T indicator[c]	0.078	0.269	0	1	Interaction of variables defined above
Post DF *x* gold indicator [c]	0.164	0.370	0	1	Interaction of variables defined above
Post DF *x* 3T ind *x* policy zone	0.068	0.252	0	1	Interaction of variables defined above
Post DF *x* gold ind *x* policy zone	0.096	0.296	0	1	Interaction of variables defined above
Rain after birth[d]	0.094	0.969	-2.04	4.50	Standardized average of rainfall 3 months before birth
Rain before birth[d]	0.104	0.978	-2.04	4.02	Standardized average of rainfall 3 months after birth
Wet season[d]	0.255	0.435	0	1	=1 if birth is in 3 wettest months in each territory, based on 1982–2013
Dry season[d]	0.256	0.436	0	1	=1 if birth is in driest months in each territory, based on 1982–2013
Birth order[a]	1.948	0.904	1	7	Birth order, lower indicates younger
Male	0.493	0.499	0	1	=1 1 if child is male, otherwise =0
Household size[a]	7.102	3.088	1	24	Household size at time of interview
Years of education[a]	4.179	3.667	0	18	Years of education of mother
Married[a]	0.864	0.342	0	1	=1 if mother is married or living with partner, otherwise =0
Literate[a]	0.469	0.499	0	1	=1 if mother is literate, otherwise =0
Observations	7,697				

Source: Authors' calculations based on (a) DHS 2013–14 data set; (b) US State Department Section 1502 map; (c) interactive maps described in Spitaels and Hilgert (2008), Spittaels and Hilgert (2009), Spittaels (2010), and Spittaels and Hilgert (2010); and (d) Climatic Research Unit Time-Series Version 3.22.

Source: see text.

Table 2: Linear fixed effects estimates of under-1 mortality

	(1)	(2)	(3)	(4)	(5)	(6)
Post DF x policy zone (β_1)	-0.021	-0.034*	0.004	-0.006	-0.038	-0.020
	(0.014)	(0.017)	(0.029)	(0.036)	(0.027)	(0.032)
Post DF *x* 3T indicator (β_2)	-0.060	-0.090***	-0.064	-0.086***	-0.136***	-0.121***
	(0.043)	(0.027)	(0.044)	(0.027)	(0.031)	(0.038)
Treatment effect (β_3)						
Post DF *x* 3T ind *x* policy zone	**0.086***	**0.116*****	**0.095***	**0.119*****	**0.182*****	**0.143*****
	(0.048)	**(0.035)**	**(0.049)**	**(0.034)**	**(0.039)**	**(0.044)**
Post DF *x* gold indicator (β_4)	0.002	-0.017	0.006	-0.028	0.010	-0.026
	(0.018)	(0.018)	(0.024)	(0.026)	(0.023)	(0.034)
Post DF *x* gold ind *x* policy (β_5)	-0.000	0.017	-0.002	0.035	-0.033	-0.004
	(0.026)	(0.026)	(0.030)	(0.031)	(0.031)	(0.039)
Covariates						
Precip. after birth	0.007	0.016***	0.008*	0.016***	0.007	0.018***
Precip. before birth	-0.010**	-0.013**	-0.009*	-0.015**	-0.008*	-0.012*
Wet season	0.005	0.016	0.005	0.018	0.003	0.011
Dry season	-0.000	-0.004	0.000	-0.008	0.004	-0.001
Birth order	0.036***	0.026*	0.036***	0.024	0.037***	0.031**
Male	0.020***	0.027***	0.020***	0.026***	0.019***	0.026***
Household size	-0.006***	0.000	-0.006***	0.000	-0.006***	0.000
Years of education	0.001		0.001		0.001	
Married	-0.031***		-0.031***		-0.031***	
Literate	-0.008		-0.009		-0.008	
Village fixed effects	Yes	No	Yes	No	Yes	No
Mother fixed effects	No	Yes	No	Yes	No	Yes
Month of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Area wide year fixed effects	Yes	Yes	No	No	Yes	Yes
Province specific year effects	No	No	Yes	Yes	No	No
Territory specific linear trends	No	No	No	No	Yes	Yes
Observations	7697	7697	7697	7697	7697	7697
R^2 (within)	0.019	0.016	0.022	0.024	0.033	0.042

Notes: Standard errors in parentheses, clustered by DHS village. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. We suppress the standard errors on the control variables in order to fit the results on a single page.

Source: see text.

Table 3: Robustness checks of mortality estimates

	(1)	(2)	(3)	(4)	(5)	(6)
1. Baseline						
Post DF *x* 3T ind *x* policy zone	**0.086****	**0.116******	**0.095****	**0.119******	**0.182******	**0.143******
Post DF *x* gold ind *x* policy zone	-0.000	0.017	-0.002	0.035	-0.033	-0.004
Observations	7697	7697	7697	7697	7697	7697
R^2 (within)	0.019	0.016	0.022	0.024	0.033	0.042
2. Interact with number of mines						
Post DF *x* 3Ts *x* policy zone	**0.016******	**0.021******	**0.017******	**0.022******	**0.027******	**0.021******
Post DF *x* gold *x* policy zone	0.003	0.009*	0.003	0.009*	0.000	0.008
Observations	7697	7697	7697	7697	7697	7697
R^2 (within)	0.020	0.018	0.023	0.025	0.033	0.041
3. Omits 2010						
Post DF *x* 3T ind *x* policy zone	**0.089****	**0.083*****	**0.098****	**0.080****	**0.223******	**0.152******
Post DF *x* gold ind *x* policy zone	0.006	0.026	0.011	0.052	-0.022	0.025
Observations	6314	6314	6314	6314	6314	6314
R^2 (within)	0.018	0.018	0.022	0.027	0.036	0.055
4. SE clusters at territory						
Post DF *x* 3T ind *x* policy zone	**0.086**	**0.116******	**0.095**	**0.119******	**0.182******	**0.143******
Post DF *x* gold ind *x* policy zone	-0.000	0.017	-0.002	0.035	-0.033	-0.004
Observations	7697	7697	7697	7697	7697	7697
R^2 (within)	0.016	0.014	0.019	0.022	0.074	0.128
Precipitation controls	Yes	Yes	Yes	Yes	Yes	Yes
DHS controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother fixed effects	No	Yes	No	Yes	No	Yes
Village fixed effects	Yes	No	Yes	No	Yes	No
Month of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Area wide year fixed effects	Yes	Yes	No	No	Yes	Yes
Province specific year effects	No	No	Yes	Yes	No	No
Territory specific linear trends	No	No	No	No	Yes	Yes

Notes: Standard errors in parentheses, clustered by DHS village. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All specifications follow the same sequencing of covariate use shown in Table 2. Panel 1 shows the benchmark results from the previous table. Panel 2 interacts the post-2009 indicators with the number of mines within a 20km radius, rather than with an indicator for the presence of at least one mine as in the baseline. Panel 3 drops observation from 2010, the year in which Dodd–Frank and the mining ban policies were passed. Panel 4 clusters the standard errors at the territory.

Source: see text.

Table 4: Mortality estimates with different radius lengths

	(1)	(2)	(3)	(4)	(5)	(6)
1. Baseline results (20 km)						
Post DF *x* 3T ind *x* policy zone	**0.086***	**0.116*****	**0.095***	**0.119*****	**0.182*****	**0.143*****
Post DF *x* gold ind *x* policy zone	-0.000	0.017	-0.002	0.035	-0.033	-0.004
R^2 (within)	0.019	0.016	0.022	0.024	0.033	0.042
2. 10 km radius						
Post DF *x* 3T ind *x* policy zone	**0.144*****	**0.127*****	**0.151*****	**0.124*****	**0.198*****	**0.167*****
Post DF *x* Gold Ind *x* Policy Zone	0.029	0.064**	0.024	0.070**	-0.006	0.024
R^2 (within)	0.020	0.017	0.023	0.024	0.033	0.042
3. 30 km radius						
Post DF *x* 3T ind *x* policy zone	**0.082****	**0.108*****	**0.089****	**0.109*****	**0.161*****	**0.133*****
Post DF *x* gold ind *x* policy zone	-0.015	-0.025	-0.017	-0.008	-0.055*	-0.046
R^2 (within)	0.020	0.017	0.023	0.024	0.034	0.042
4. 40 km radius						
Post DF *x* 3T ind *x* policy zone	**0.023**	**0.045**	**0.038**	**0.061***	**0.088****	**0.080**
Post DF *x* gold ind *x* policy zone	-0.000	-0.019	-0.001	-0.009	-0.053*	-0.052
R^2 (within)	0.020	0.016	0.023	0.025	0.033	0.042
5. 50 km radius						
Post DF *x* 3T ind *x* policy zone	**0.013**	**0.040**	**0.021**	**0.045**	**0.070***	**0.069**
Post DF *x* gold ind *x* policy zone	0.001	-0.009	-0.001	0.006	-0.064**	-0.047
R^2 (within)	0.020	0.016	0.023	0.025	0.034	0.042
Precipitation controls	Yes	Yes	Yes	Yes	Yes	Yes
DHS controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother fixed effects	No	Yes	No	Yes	No	Yes
Village fixed effects	Yes	No	Yes	No	Yes	No
Month of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Area wide year fixed effects	Yes	Yes	No	No	Yes	Yes
Province specific year effects	No	No	Yes	Yes	No	No
Territory specific linear trends	No	No	No	No	Yes	Yes

Notes: Standard errors in parentheses, clustered by DHS village. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All specifications follow the same sequencing of covariate use shown in Table 2. All regressions employ 7,697 observations.

Source: see text.

Table 5: Mortality estimates with controls for ACLED conflict <20km from villages

	(1)	(2)	(3)	(4)	(5)	(6)
1. Baseline (no conflict controls)						
Post DF *x* 3T ind *x* policy zone	**0.086***	**0.116*****	**0.095***	**0.119*****	**0.182*****	**0.143*****
Post DF *x* gold ind *x* policy zone	-0.000	0.017	-0.002	0.035	-0.033	-0.004
R^2 (within)	0.019	0.016	0.022	0.024	0.033	0.042
2. All ACLED conflicts						
Post DF *x* 3Ts *x* policy zone	**0.088***	**0.116*****	**0.096***	**0.119*****	**0.187*****	**0.146*****
Post DF *x* gold *x* policy zone	0.001	0.017	0.001	0.036	-0.031	-0.004
No. of Conflict events	0.000	0.000	0.000*	0.000	0.001**	0.000
R^2 (within)	0.019	0.016	0.022	0.024	0.034	0.042
3. ACLED conflicts by type						
Post DF *x* 3Ts *x* policy zone	**0.086***	**0.116*****	**0.093***	**0.118*****	**0.185*****	**0.145*****
Post DF *x* gold *x* policy zone	0.000	0.017	-0.001	0.035	-0.030	-0.003
No. of battle events	0.002**	0.001	0.001**	0.001	0.002**	0.001
No. of violence vs. civilian event	-0.001	-0.001	-0.001	-0.000	-0.000	-0.001
R^2 (within)	0.020	0.017	0.022	0.024	0.034	0.042
4. All ACLED conflicts						
Post DF *x* 3Ts *x* policy zone	**0.084***	**0.116*****	**0.092***	**0.119*****	**0.178*****	**0.144*****
Post DF *x* gold *x* policy zone	0.002	0.017	0.000	0.034	-0.031	-0.005
Indicator of conflict event	-0.012	0.002	-0.012	0.002	-0.011	0.002
R^2 (within)	0.019	0.016	0.022	0.024	0.033	0.042
5. ACLED conflicts by type						
Post DF *x* 3Ts *x* policy zone	**0.083***	**0.110*****	**0.091***	**0.111*****	**0.179*****	**0.135*****
Post DF *x* gold *x* policy zone	0.000	0.017	-0.002	0.034	-0.032	-0.005
Indicator of battle event	-0.011	-0.020	-0.012	-0.020	-0.008	-0.019
Indicator of violence vs. civilian event	0.006	0.022	0.010	0.029*	-0.000	0.022
R^2 (within)	0.019	0.017	0.022	0.026	0.033	0.043
Precipitation controls	Yes	Yes	Yes	Yes	Yes	Yes
DHS controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother fixed effects	No	Yes	No	Yes	No	Yes
Village fixed effects	Yes	No	Yes	No	Yes	No
Month of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Area wide year fixed effects	Yes	Yes	No	No	Yes	Yes
Province specific year effects	No	No	Yes	Yes	No	No
Territory specific linear trends	No	No	No	No	Yes	Yes

Notes: Standard errors in parentheses, clustered by DHS village. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All specifications follow the same sequencing of covariate use shown in Table 2. All regressions employ 7,697 observations.

Source: see text.

Table 6: Linear probability estimates of health & income outcomes from DHS surveys

	Bednet (1)	Prenatal care (2)	Bednet (3)	Prenatal care (4)
3T indicator (λ_1)	-0.308***	-0.106	-0.310***	-0.094
	(0.058)	(0.097)	(0.063)	(0.099)
2013 *x* 3T indicator (λ_2)	0.198**	0.234**	0.200**	0.222**
	(0.076)	(0.096)	(0.080)	(0.100)
2013 *x* policy zone (λ_3)	-0.131	0.161**	0.084	-0.317*
	(0.091)	(0.074)	(0.089)	(0.187)
Treatment effect (λ_4)	-0.164	-0.200*	-0.182**	-0.188*
Post DF *x* 3T ind x policy zone	(0.101)	(0.113)	(0.085)	(0.107)
Gold indicator (ω_1)	-0.066	-0.102	-0.059	-0.197
	(0.095)	(0.096)	(0.102)	(0.131)
2013 *x* gold indicator (ω_2)	-0.052	0.141	-0.058	0.236*
	(0.091)	(0.090)	(0.100)	(0.128)
Post DF *x* gold ind *x* policy zone (ω_3)	0.240**	-0.124	0.213*	-0.132
	(0.112)	(0.106)	(0.116)	(0.139)
<u>Covariates (mother level)</u>				
Urban	0.104***	0.078***	0.103***	0.080***
Years of education	0.002**	0.003*	0.002***	0.003**
Household size	-0.004	0.001	-0.005*	0.002
Married	0.079***	0.022	0.080***	0.024*
Literate	0.044***	0.026*	0.043***	0.026*
Age	-0.002**	-0.002***	-0.002**	-0.002***
Territory fixed effects	Yes	Yes	Yes	Yes
Area wide 2013 time effects	Yes	Yes	No	No
Province x 2013 time effects	Yes	Yes	Yes	Yes
Territory x 2013 time effects	No	No	No	No
Observations	7794	4393	7751	4386
R^2	0.247	0.202	0.248	0.203

Notes: Standard errors in parentheses, clustered by territory. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: see text.

Table 7: Mortality estimates with heterogeneous policy effects, due to armed group presence

	(1)	(2)	(3)	(4)	(5)	(6)
1. Baseline (panel 2 of Table 3)						
Post DF *x* 3T ind *x* policy zone	0.016***	0.021***	0.017***	0.022***	0.027***	0.021***
R^2 (within)	0.020	0.018	0.023	0.025	0.033	0.041
2. Armed group interactions						
Post DF *x* 3Ts w AG *x* policy zone	0.021**	0.026***	0.037***	0.021***	0.028***	0.029***
Post DF *x* 3Ts w/o AG *x* policy zone	0.013**	0.012**	0.018***	0.022***	0.018***	0.012
R^2 (within)	0.020	0.023	0.032	0.018	0.026	0.040
3. Armed group interactions by type						
Post DF *x* 3Ts rebel AG *x* policy zone	0.034	0.041**	0.042**	0.023	0.032**	0.039*
Post DF *x* 3Ts govt AG *x* policy zone	0.017*	0.023**	0.036***	0.020**	0.027***	0.027***
Post DF *x* 3Ts w/o AG *x* policy zone	0.013**	0.012**	0.018***	0.022***	0.018***	0.012
R^2 (within)	0.020	0.023	0.032	0.018	0.026	0.040
Gold mine controls (same as baseline)	Yes	Yes	Yes	Yes	Yes	Yes
Precipitation controls	Yes	Yes	Yes	Yes	Yes	Yes
DHS controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother fixed effects	No	Yes	No	Yes	No	Yes
Village fixed effects	Yes	No	Yes	No	Yes	No
Month of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Area wide year fixed effects	Yes	Yes	No	No	Yes	Yes
Province specific year effects	No	No	Yes	Yes	No	No
Territory specific linear trends	No	No	No	No	Yes	Yes

Notes: Standard errors in parentheses, clustered by DHS village. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All specifications follow the same sequencing of covariate use shown in Table 2. The number of observations in all specifications is 7697. The notation 'AG' stands for 'armed group'. The rebel armed groups in the IPIS maps (see Spittaels and Hilgert (2009) and Spittaels (2010)), include the Forces for the Liberation of Rwanda (FDLR), the Forces Républicaines Fédéralistes (FRF), and Mayi Mayi militias (an umbrella term for loosely affiliated groups of local militias). The government armed groups include the Armed Forces of the Democratic Republic of Congo (FARDC) and the National Congolese Police (PNC).

Source: see text.

Appendices

Appendix 1: Supplementary empirical tables

Table A1: Number of villages and observations near at least one 3T mine

	Distance Threshold				
	10km	20km	30km	40km	50km
Panel A: Villages					
3T = 1, policy zone = 1	13	23	33	47	53
3T = 1, policy zone = 0	2	4	5	10	10
3T = 0, policy zone = 1	77	67	57	43	37
3T = 0, policy zone = 0	109	107	106	101	101
Panel B: Births					
3T = 1, policy zone = 1	571	992	1,379	1,904	2,157
3T = 1, policy zone = 0	96	160	213	446	446
3T = 0, policy zone = 1	2,928	2,507	2,120	1,595	1,342
3T = 0, policy zone = 0	4,102	4,038	3,985	3,752	3,752

Source: Authors calculations.

Table A2: Summary statistics on ACLED conflicts with 20 km of DHS village during first 12 months post-birth

	Mean	St. dev	Min	Max
ACLED all conflict, number	5.01	15.26	0	199
ACLED all conflict, indicator	0.36	0.48	0	1
ACLED battle, number	2.71	8.58	0	112
ACLED battle, indicator	0.29	0.45	0	1
ACLED violence vs. civilians, number	1.88	6.03	0	75
ACLED violence vs. civilians, indicator	0.26	0.44	0	1
Observations	7,697			

Notes: To assign conflicts to DHS villages, we determined distance from village to conflict geo-coordinates.

Source: Authors' calculations based on data from ACLED, as described by Raleigh et al. (2010).

Table A3: Summary statistics of health and income outcomes from DHS surveys

	2007 Mean	2013 Mean	Obs.
Bednet mom (=1 if yes, otherwise =0)	0.193	0.583	7,794
Prenatal care (=1 if yes, otherwise =0)	0.837	0.873	4,394

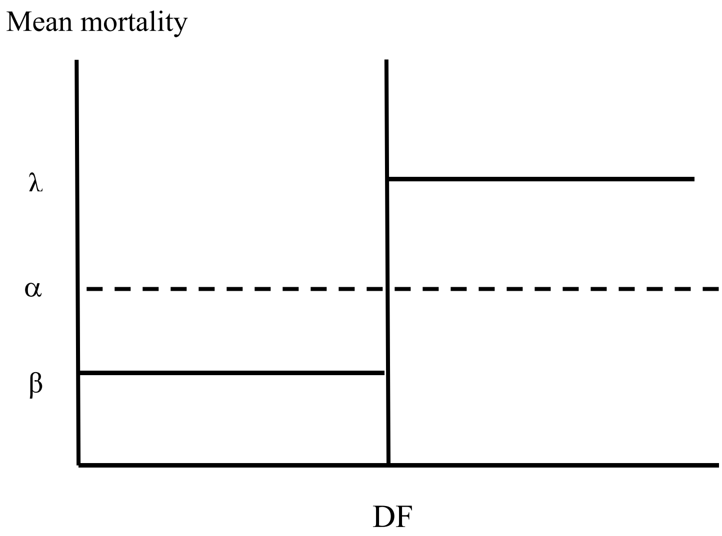
Notes: There are only 4,394 Prenatal Care and Prenatal Visit variables because we restrict comparisons to births occurring in 2007 versus births occurring in 2010-2012.

Source: Authors' calculations based on data from the DHS surveys conducted in 2007 and 2013.

Appendix 2: Effect of selective migration on empirical estimates

This appendix clarifies our argument that selective migration is unlikely to drive the infant mortality findings when the variation comes from within mother births before and after Dodd–Frank, as with the inclusion of mother fixed effects. The coefficients are largely unaffected by selective migration because the recall data position each child's birth location based on where the mother is living in 2013–14. To clarify the argument, we compare against a hypothetical scenario in which we observed repeated cross-sectional mortality data from villages. In that scenario, selective migration would potentially have large effects on the coefficients.

1. <u>Benchmark scenario of no selective migration.</u>



β = mean in treated villages before Dodd–Frank

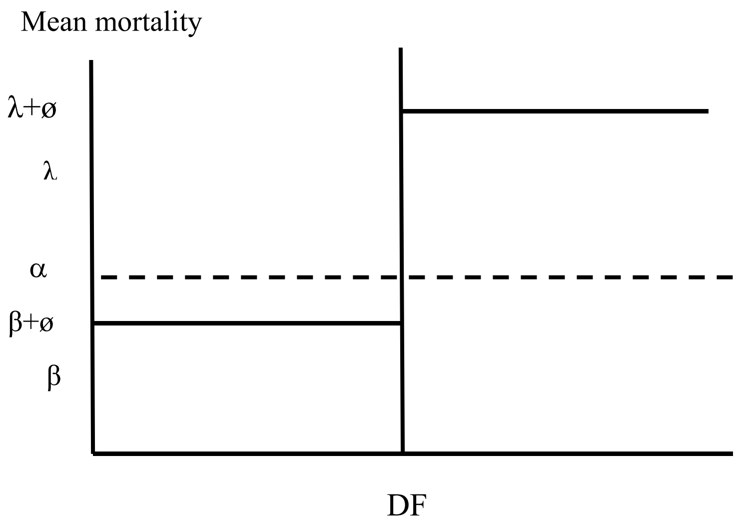
λ = mean in treated villages after Dodd–Frank

α = mean in control area

Measured Treatment Effect = (λ – β) – (α - α) = λ – β

2. <u>DHS data scenario with selective migration and mother recall data</u>

Assume healthy mothers leave treated areas after Dodd–Frank. Assume migration destinations are highly diluted so that the health composition of the control area is unaffected.



β + ø= mean in treated area before Dodd–Frank

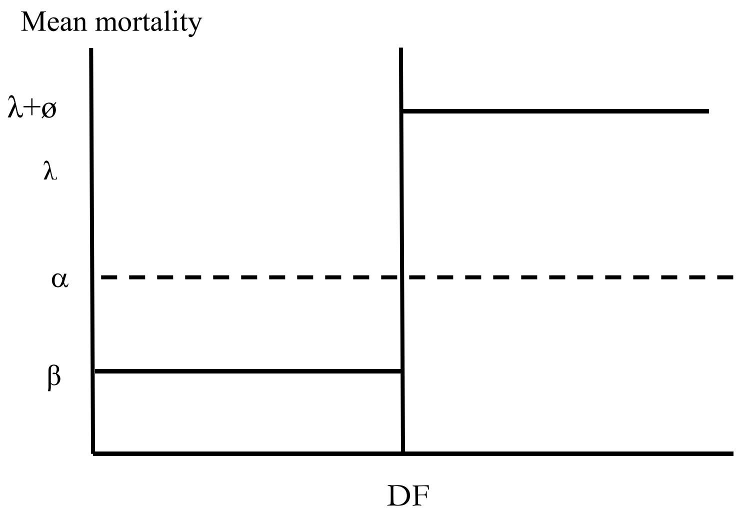
λ + ø= mean in treated area after Dodd–Frank

Measured Treatment Effect = (λ + ø – β+ ø) – (α - α) = λ – β

There is no bias due to migration because selective migration inflates both the before and after measures of mortality when using recall data.

3. <u>Hypothetical Scenario with Selective Migration and Repeated Cross-Sectional Data</u>

Assume healthy mothers leave after Dodd–Frank. Assume migration destinations are highly diluted so that the health composition of the control area is unaffected.



Measured Treatment Effect (Diff-N-Diff) = $(\lambda + ø - \beta) - (\alpha - \alpha) = \lambda - ø - \beta$

The measured treatment is larger due to selective migration in this case, with repeated cross-sectional data, rather than mother recall data.