How Does Armed Conflict Shape Investment? Evidence from the Mining Sector

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How does conflict affect firms' investment decisions? Past results are mixed: a third of the studies we reviewed report null or mixed correlations; some suggest that conflict increases investment. We rationalize these results, arguing that armed conflict has divergent effects depending on firms' exposure to violence. Conflict can deter investment by disrupting production or raising uncertainty. Yet, conflict can encourage investment by hampering government oversight. We argue that each mechanism operates over different geographic extents. We use data from the mining sector to test these claims and report three main results. Firms operating at conflict sites dramatically reduce investments. By contrast, firms operating in territory surrounding conflict, but separated from fighting, actually increase investment. Firms far from violence see a small negative effect. These divergent responses cannot be inferred from aggregate flows: we show that conflict depresses aggregate investment, but this reflects responses among firms far from fighting.

When firms and individuals fear that future economic returns will be destroyed or expropriated, they have little incentive to invest. This foundational tenet of economic development motivates a large literature in comparative and international political economy which identifies institutions that reassure potential domestic (e.g., Besley and Persson 2011; North 1981; Stasavage 2002) and foreign investors (e.g., Büthe and Milner 2008; Jensen 2003; Vernon 1971).[1] Limiting armed conflict is of primary importance: civil war has been concisely described as "development in reverse" (Collier et al. 2003). By monopolizing violence, states allay fears of predation and realize the "colossal [economic] gains from providing domestic tranquility" (Olson 1993, 567).

In this paper, we argue that armed conflict—which often implies the breakdown of institutions—has divergent effects on investment among firms operating within the same country and industry, depending on each firm's geographic proximity to violence. We propose three channels through which armed conflict affects firms' investment decisions.[2] First, conflict can disrupt or destroy production, discouraging investment. Second, conflict can undermine state capacity, which has theoretically ambiguous effects on investment: firms may enjoy reduced oversight but lament the withdrawal of protection and public services.[3] Finally, conflict can increase uncertainty about the government's standing or policy agenda, leading to divestment.

Critically, we argue that these mechanisms apply to different geographic areas surrounding an armed conflict. Threats to production, we claim, are very local, affecting the small proportion of investments located at the sites of conflict. State

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1. For a recent review of the literature in international political economy, see Pandya (2016).

2. We are not the first to note that firms operating in the same country and sector can be differentially affected by conflict (e.g., Collier and Duponchel 2013; Kobrin 1978).

3. Our focus here is on what shapes investment; we take no stand on whether such investment is welfare enhancing.

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capacity should be diminished in buffer zones—areas affected by armed conflict where the state's control over territory is disputed, but fighting is not active. Both claims reflect the scale of modern armed conflicts, which are characterized by relatively small, sporadic battles that affect limited territory (Berman, Felter, and Shapiro 2018). Finally, uncertainty around policy changes or reputational risk impacts all firms operating in a country with conflict. Conflict may not, thus, have a uniform effect on firms' investment decisions: a firm's proximity to violence shapes how it responds (see fig. 1 for an illustration). Indeed, conflict may not always deter investment—a point underscored in recent work by Osgood and Simonelli (2019), who show that firms with higher exit costs are less responsive to violence.[4]

Social scientists have long worked to quantify the impact of instability on investment (for an early contribution, see Bennett and Green [1972]): our systematic review finds 75 published empirical studies of this relationship since 1990. Most papers (64%) report a negative conditional correlation. Yet, almost all of these past studies use aggregate data to estimate the relationship between conflict and investment at the country level. This recovers a weighted average of effects for firms operating near and far from fighting. When these effects push in different directions, the weighted average masks heterogeneous firm responses.

We advance the literature by addressing this ecological inference problem and offering empirical tests of our theoretical claims, which predict divergent firm-level responses. We construct global panel data on the investments and projects of mining firms, which enable us to measure where armed conflicts occur relative to firms' operations. Our outcome data measure how much each firm invests in exploration activities in every country and in every year between 1997 and 2014. Our data enable a research design in which we compare investment among firms near and far from conflict, before and after the violence occurs. We include firm-by-year, firm-by-country, and country-by-year fixed effects in our models to rule out a large set of potential confounds. In addition to providing a unique source of data, mining is an important domain for evaluating the effects of conflict on investment: the extractives sector accounted for over 30% of greenfield foreign direct investment (FDI) in low-income countries in 2011 (UNCTAD 2012, 64) and is featured in foundational work on the property rights and decision-making of foreign investors (e.g., Moran 1974; Vernon 1971).

We find that a small number of firms with operations at conflict sites (within 5 kilometers of an armed conflict) reduce their investments dramatically following violence. Yet, firms operating in the territory surrounding conflict but separated from the actual fighting (up to 60 km from an armed conflict) actually increase their investment. This effect is largest for firms with an operation that is 30–40 kilometers from an armed conflict. These firms appear to be a safe distance from the violence and yet they are close enough to benefit from how conflict diminishes states' oversight capacity. Finally, we find that firms well removed from violence see a small negative effect. As this last group constitutes the largest share of firms, this small effect contributes most to the country-level finding and, thus, masks responses among the firms more proximately affected by violence. With our data aggregated to the country-year level, we show that armed conflict depresses investment.

We incorporate auxiliary data to explore several mechanisms. First, using mine-level panel data from projects across Africa, we show that armed conflict disrupts production, but only for mines located at conflict sites (within 5 km of the violence). The likelihood that a mine produces anything falls by 30 percentage points two years after nearby conflict. Second, drawing on country-year data, we show that the elasticity between mineral production and tax revenues from natural resources falls after countries experience armed conflicts involving the state. This is consistent with the claim that conflict undermines the state's ability to tax mining activity, one dimension of state capacity that may be affected in buffer zones. Finally, at the country-year level, we show that conflict reduces government stability in conflict-affected states.

We make three contributions: conducting a formal, "systematic review" of prior empirical work; developing a theoretical framework that relates firms' investment responses to their geographic exposure to conflict; and providing new evidence on how and why firms respond, both positively and negatively, to armed conflict. Our theory and analyses help decompose aggregate findings and, in so doing, reveal that analyses of aggregate investment flows can miss the investment-promoting effect of conflict among a subset of firms.

We help advance debates in comparative and international political economy. Influential work in comparative politics argues that states may not monopolize the use of violence; in fact, their capacity does not always extend far beyond capitals or into borderlands (Boone 2003; Herbst 2000). More recent empirical work maps states' limited capacity (Lee and Zhang 2017; Pierskalla, Schultz, and Wibbels 2017). We build on this research by describing the behavior of firms operating in gray zones, where the state's authority is contested. Consistent with case studies from Guidolin and La Ferrara (2007) and

4. Jamison (2019) and Lee (2017) report heterogeneous effects of conflict on investment depending, respectively, on whether a sector enjoys a natural monopoly and the host states' antiterrorism capacity.

Christensen, Nguyen, and Sexton (2019), we find that certain firms can benefit from the state's incomplete control.

Seminal work in international political economy argues that investors shy away from countries that cannot credibly protect their property rights (e.g., Moran 1974; Vernon 1971). More recent contributions expand upon this argument, showing how the characteristics of host governments (e.g., Jensen 2008; Lee 2017), industries (e.g., Burger, Ianchovichina, and Rijkers 2015; Jamison 2019; Wright and Zhu 2018), and individual firms (e.g., Barry 2018; Osgood and Simonelli 2019) affect investment responses to instability and other forms of political risk. We make a complementary contribution, showing that firms' geographic exposure to violence moderates their response to instability.

Finally, the vast majority of papers identified through our systematic review focus on country-level measures of conflict and aggregate investment. We adopt a firm-centered view and introduce a key source of heterogeneity in firms' investment behavior: conflict exposure. In doing so, we parallel developments elsewhere in international political economy in the study of trade and, more recently, foreign investment (Barry 2016; Zhu and Shi 2019).

SYSTEMATIC REVIEW OF EXISTING EMPIRICAL WORK

Nearly five decades ago scholars began quantitatively studying how political instability shapes investment, using newly available cross-national data (e.g., Bennett and Green 1972). To assess the weight of this evidence, we conduct a formal systematic review.[5] The goal is to identify and summarize all research that meets prespecified criteria, rather than focusing on a researcher-selected subset that may, for example, exclude earlier work or research from adjacent disciplines. Using the protocol detailed in appendix H.1, we examined 15,583 books and articles to identify 75 peer-reviewed studies that meet four criteria: (1) published in 1990 or later; (2) published in a peer-reviewed social science or business journal or by a university press; (3) examines the relationship between conflict and foreign investment, with a measure of conflict as an independent variable and investment as a dependent variable; and (4) includes a point estimate (see fig. H.1; figs. B.1–B.5, C.1, D.1–D.4, E.1, F.1, G.1, G.2, and H.1 are available online).[6]

Table A.1 (tables A.1, B.1–B.3, D.1–D.4, E.1, F.1–F.3, and G.1–G.4 are available online) describes the individual studies. The data used in each study cover multiple years, spanning 1950–2013, with the bulk of the observations coming from the four decades between 1970 and 2010. Sixty-four percent find a negative conditional correlation between instability or conflict and investment (see table 1).[7] Scholars have identified this negative relationship in broad cross-national samples, in industrialized democracies, and in low-income countries.

In the paper most immediately relevant to our own, Guidolin and La Ferrara (2007) turn the conventional wisdom on its head: they find that diamond mining companies actually benefited from Angola's civil war. The sudden end of the conflict in 2002 led to a 4-percentage-point drop in cumulative abnormal returns for companies holding concessions in Angola. "No matter how high the costs to be borne by diamond mining firms in Angola during the conflict," they write, "the war appears to have generated some counterbalancing 'benefits' that in the eye of investors more than outweighed these costs" (Guidolin and La Ferrara 2007, 1978).

Many studies fail to consistently find a significant correlation between conflict or instability and investment. Null or mixed findings make up more than one-third of the studies.

The papers in this literature differ along several dimensions, relying on different samples, dependent variables, and measures of conflict or instability, and exploiting different sources of variation. This makes it difficult to pinpoint why their findings diverge. We focus on three common features of past studies. First, only half of the studies include unit fixed effects (see table 1). Without them, estimates may reflect omitted variable bias from characteristics that make countries susceptible to conflict and inhospitable to investment (e.g., autocracy). Few (12) include time fixed effects, which raises the additional possibility that estimates are confounded by investment booms that happen to coincide with changes in the frequency of armed conflict. Second, 40% of the studies rely on a composite measure of political risk, of which violence is only one component (for a critique of these measures, see Henisz [2000, 3]).[8] Finally, likely due to data availability, more than 80% of the studies focus on country-level measures of investment and violence. Yet, investment decisions are made at the firm or project level, and the violence these firms confront is increasingly localized—sporadic insurgent attacks rather than large-scale wars (Berman et al. 2018).

5. We follow the PRISMA guidelines (see app. H; apps. A–H are available online).

6. Related studies measure firm exit or entry as a categorical variable (e.g., Barry 2018; Camacho and Rodriguez 2013).

7. Our coding reflects both the sign and statistical significance (at any level) of the point estimate. Appendix H describes our rules for selecting among multiple models.

8. A common measure is the Worldwide Governance Indicators variable "Political Stability and Absence of Violence/Terrorism," which does not directly measure violence.

Table 1. Mixed Findings from Past Studies of Instability and Investment

Effect Direction	Studies (1)	Unit Fixed Effects (2)	Time Fixed Effects (3)	Instrumental Variables (4)
Negative	40	25	7	4
No effect	21	6	4	1
Positive	6	4	0	3
Mixed	8	2	1	0
All studies	75	37	12	8

Note. This table summarizes our systematic review. We tabulate the number of studies that report positive, null, negative results, or mixed results (where in a single paper key results were a mix of positive, negative, and/or null). Columns 2 and 3 report the number of studies that employ unit and time fixed effects, respectively; and col. 4 reports the number employing instrumental variables designs. See table A.1 for the list of studies and their results.

THEORY OF CONFLICT EXPOSURE AND INVESTMENT

Past theoretical work has highlighted that instability and conflict can have very different effects on firms operating in the same country. Kobrin (1978, 114) lays out the firm's calculus: "The manager should be interested in political instability only to the extent that it is likely to constrain actual or potential operations. One must ask two questions. What is the probability of a given irregular event occurring and, given that event, what is the probability it will affect my firm? . . . Political risk is not a homogenous phenomenon; vulnerability is clearly industry, firm, and even project specific." Recent empirical work uncovers firm-level heterogeneity. Osgood and Simonelli (2019), for example, find that US multinational corporations with immobile assets are less responsive to terrorism. Relatedly, Barry (2018, 283) finds that conflict deters new ventures but that established firms weather low-level conflict. Others argue that political connections (Fisman 2001) and diversification (Dai, Eden, and Beamish 2017; Witte et al. 2016) moderate firms' exposure to instability and conflict.

Recognizing this heterogeneity, we develop a framework to predict how investors' responses to armed conflict vary based on their proximity to violence. First, conflict could disrupt production by making operations unsafe or infeasible. Second, it could undermine state capacity and, thus, hamper oversight or undermine property rights or public services. Third, conflict may increase uncertainty around the government's domestic or international policy agendas. Finally, firms may fear that their reputations will be damaged from operating in a conflict-affected state. These mechanisms can generate countervailing effects. Production stoppages might discourage investment, but less regulation could be a boon for the private sector. Limited oversight might reduce operating costs, and yet, firms' reputations could take a hit for working alongside a government embroiled in civil conflict. As the *Economist* (2000) summarizes, "for brave businessfolk, there are thus rich pickings in grim places. But there are also immense obstacles and risks." An investor's response to conflict, thus, depends on which of these mechanisms apply and their relative magnitudes.

We argue that these mechanisms apply to different areas around an armed conflict event.[9] We delineate three concentric extents: (1) the conflict site, where fighting actually takes place; (2) the buffer zone surrounding the conflict site, where the state's control may be disputed but fighting is not active; and (3) the country with conflict. Figure 1 illustrates these three extents of exposure for a hypothetical conflict in Sierra Leone. To demarcate the conflict site and buffer zone, we use circular buffers that emanate from where fighting takes place.

Firms operating at conflict sites are directly threatened by violence and most likely to see their operations disrupted. Mihalache-O'keef and Vashchilko (2010) offer examples from insurance claims submitted to the Overseas Private Investment Corporation, a US government agency that provides political risk insurance to US firms. In 1979, government troops and Sandinistas took turns occupying and bombarding American Standard's facilities in Nicaragua. In 1977, Freeport Mineral's copper mine in West Papua, Indonesia, was targeted by separatists; the firm paid for military personnel to secure its site. For these firms, violence threatened physical capital or critical infrastructure, discouraging continued investment.

Armed civil conflict, almost by definition, implies that the state has lost its monopoly on violence in some part of its territory. Beyond the specific sites of battles, the buffer zones surrounding conflicts are often regarded as ungoverned or no-go areas, where the legitimacy or capacity of the central state

9. While they do not enumerate the same mechanisms or geographic extents, Dai et al. (2013, 557) show that proximity to armed conflict affects the survival of Japanese firms' subsidiaries.

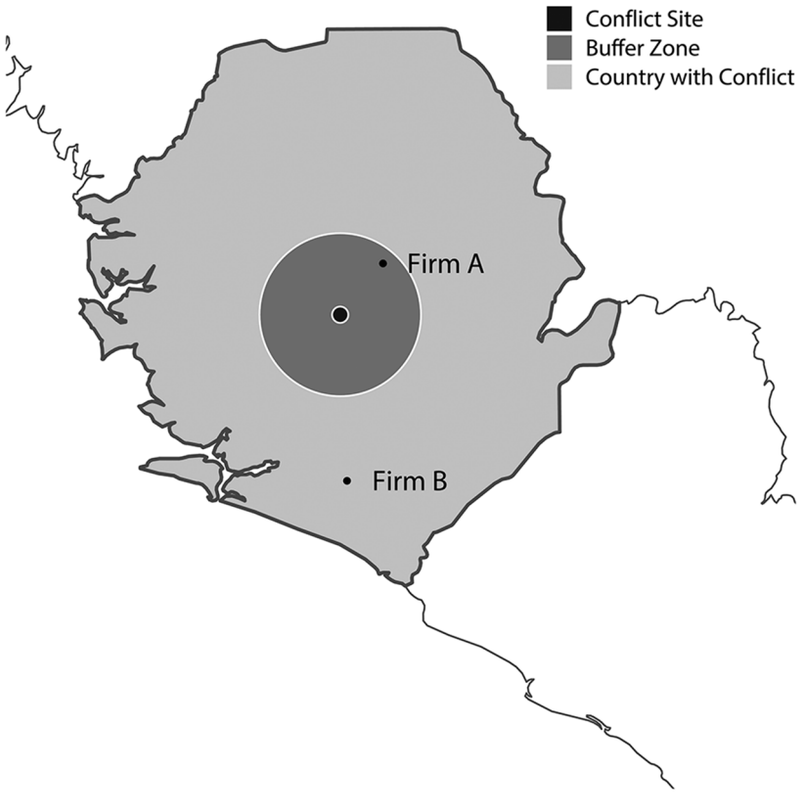

Figure 1. Geographic extents at which conflict affects firm activity. This figure uses a hypothetical armed conflict in Sierra Leone to define three concentric areas around an event: (1) a conflict site (black); (2) a buffer zone (dark gray); and (3) the conflict-affected country (light gray). Two mining projects are depicted to illustrate their exposure to conflict: firm A's project is in the buffer zone, while firm B's project is only within the conflict-affected country.

is contested. This could benefit firms operating in buffer zones around conflict if it inhibits the state's capacity to tax firms (formally or informally) or enforce regulations (e.g., environmental or labor standards). If conflict renders buffer zones inaccessible or unsafe for bureaucrats, firms can more easily evade tax and regulatory efforts (Ch et al. 2018).

Le Billon (2008, 1) outlines the challenge facing governments attempting to oversee mining firms in buffer zones (see also Guidolin and La Ferrara 2007; van den Boogaard et al. 2018): "Governments often suffer from lack of knowledge about the resources available for exploitation and recent developments in the sector—due, for example, to lapses in surveys, undocumented wartime resource exploitation, death or flight of qualified personnel, and outdated training. As a result, governments fail to maximize revenue collection, especially when negotiating with better informed companies." In addition to a reduced tax burden, mining firms may also be able to engage in cost-saving measures only possible with limited state oversight: encroaching on land without prior consent or compensation, engaging in unlicensed activity (e.g., starting production on an exploration license), or employing methods that violate environmental or labor standards (Smith and Rosenblum 2011).

Recent empirical work generalizes these arguments, finding that internal conflict depresses states' fiscal capacity (e.g., Chowdhury and Murshed 2016).[10] Besley and Persson (2008, 528), for example, find that countries facing internal conflict have a tax-to-GDP ratio that is 7% lower. Moreover, governments may provide special financing, supplemental insurance coverage, or statutory tax relief for firms that continue to operate despite nearby conflicts (Berman 2000).

However, diminished state capacity could also harm firms operating in buffer zones and cause them to reduce investment. A capacitated state may secure firms' property rights by both protecting assets (Besley and Persson 2008; McDougal 2010) and limiting extortion by state or nonstate actors (e.g., protection rackets run by corrupt local officials or rebel groups) (Collier 1999; Keen 1998). Moreover, if firms rely on infrastructure impacted by conflict (e.g., road networks that have been damaged or disrupted by road blocks), this could increase operating costs (Collier 1999; Mills and Fan 2006).

10. While these studies emphasize taxation, conflict also hampers nonstate (e.g., civil society, journalistic) efforts to enforce standards that can increase firms' costs.

Finally, while less of a concern in enclave industries like mining (Banerjee et al. 2014), the state's provision of public services or utilities may be disrupted, forcing firms to devise costly stopgaps or delay activities while they await permits. These risks could sour investors, inhibiting firms' access to finance.

Most firms mine far from violence. Armed conflict in the borderlands of northern Myanmar, for example, does not directly impact coal mines located hundreds of kilometers away. This reflects an important feature of modern armed conflicts: they are not geographically encompassing campaigns but, rather, "small wars" (Berman et al. 2018). Blattman and Miguel (2010, 39) observe that "civil wars are also often localized and fought with small arms and munitions, so they do not necessarily see the large-scale destruction of capital caused by bombing" (on downward trends in battle deaths, see Lacina, Gleditsch, and Russett [2006]). This is apparent in our data: for firms operating within 20 kilometers of fighting, the average conflict they are exposed to involves only 5.6 deaths on average.

Research on political risk argues that firms far from fighting can still be adversely impacted by conflict-induced policy changes. "If instability is to affect significantly foreign investors," Kobrin (1978, 115) writes, "it is most likely to do so through a change in government policy." If violence in northern Myanmar, to continue our example, affects the government's domestic or international standing or generates other policy uncertainty, this could deter investment. In the extreme, would-be investors may worry about regime change or the expropriation of assets (Jensen 2003) provoked by the fiscal demands of conflict.[11] Short of government turnover or expropriation, investors may fear changes related to license fees, the terms of joint ventures with the state, currency restrictions or devaluations, or travel restrictions (for a theory of when governments breach contracts with foreign firms, see Wellhausen [2014]).

A distinct, country-level mechanism concerns the reputation of firms among shareholders or consumers, who may avoid companies operating in conflict-affected states (Henisz 2017). The *Economist* observes that "firms doing business in countries with unpleasant governments have been pilloried by nongovernmental organizations (NGOs), endangering the most priceless of assets, their good name" (quoted in Bennett 2001, 2). Blanton and Blanton (2007, 145) use Apple's rapid divestment from Myanmar as an example of companies avoiding countries with poor human rights records, a characteristic correlated with civil conflict.

We collect these mechanisms in table 2. Armed conflict could amplify or deter investment depending on a firm's proximity to violence and the relative magnitudes of these mechanisms. Relying on aggregate data, existing empirical work has been unable to estimate the effects of these different extents of conflict exposure. We do so in this paper and test the following four hypotheses:

H1 (Conflict site). Firms reduce their investment in countries where their operations are located at conflict sites.

H2 (Buffer zone). Firms change their investment in countries where their operations are located in a buffer zone around armed conflict, with the direction of change depending on the magnitude of countervailing mechanisms.

H3 (Country with conflict). Firms reduce their investment in conflict-affected countries where their operations are distant from armed conflict.

H4 (Aggregate effect). As most firms' operations are distant from armed conflict, the effect of armed conflict on aggregate investment in a country is negative.

DATA

We use fine-grained data from the mining sector to test these theoretical predictions using a research design that overcomes inferential challenges in past work. Mining is an important sector, particularly in developing, conflict-prone countries. Forty percent of greenfield FDI in low- and lower-middle-income countries between 2003 and 2015 went into extractives projects (*Financial Times* 2019). The next largest sectors are real estate, communications, and financial services. Over 50 countries depended on natural resources for more than 20% of exports or 10% of GDP between 1995 and 2015 (Davy and Tang-Lee 2018, 2). This scale has attracted academic attention. Influential work on the political economy of foreign investment focuses on the mining sector (e.g., Moran 1974; Vernon 1971), and conflict has been an important outcome for scholars studying the consequences of extractive industries (e.g., Berman et al. 2017; Christensen 2019).

Without comparable firm-country-year investment data from other sectors, we cannot assess whether our estimates generalize to other industries. Past work suggests that mining investments may be less vulnerable to violence. First, mining is tied to fixed geologic features and, thus, not easily relocated. In

11. The need to redeploy funding to security services could also deprive other parts of government, generating uncertainty around policy implementation.

Table 2. Mechanisms Linking Violence to Investment

Mechanism	Effect Direction	Geographic Scale: Conflict Site	Geographic Scale: Buffer Zone	Geographic Scale: Country with Conflict
Disrupted production:				
Fighting disrupts operations.	−	✓		
State capacity:				
Tax and regulatory obligations decline in disputed territory.	+	✓	✓	
Ability to protect property rights and maintain critical infrastructure and services declines in disputed territory.	−	✓	✓	
Policy change:				
Conflict increases uncertainty around the standing or actions of government.	−	✓	✓	✓
Reputation:				
Conflict creates risk of reputation loss from investors, home governments, media, or NGOs.	−	✓	✓	✓

response to conflict, mining firms—unlike manufacturers—cannot easily relocate to protect their assets.[12] Second, recognizing that exit is not possible, mining firms may also spend more on private security and utilities to reduce their vulnerability to conflict. The World Bank, for example, reports that "many mining companies [in sub-Saharan Africa] are still opting to supply their own electricity with diesel generators rather than buy power from the grid" (World Bank 2015). The immovability of mining investments and firms' endogenous expenditure on private precautions likely dampen the effect of conflict on investments relative to other sectors. However, using data from fDi Markets (*Financial Times* 2019), we find no evidence that armed conflict has differential effects on aggregate investment in the natural resource sectors (fig. E.1) or, specifically, metals and minerals (table E.1) relative to other sectors (app. E describes these analyses).

Investment data

Our outcome is mining firms' exploration investment (deflated to real USD in 1997), based on data from SNL Metals and Mining. SNL Metals and Mining obtains data through a survey of companies and, in the event of nonresponse or refusal, the budgets are compiled by SNL and sent to the firms for confirmation or adjustment. The data are at the firm-country-year level: we observe how much the same firm invests in different countries in the same year. The data provide global coverage from 1997 to 2014 for major minerals, including base metals (e.g., copper, tin), diamonds, gold, iron, platinum group metals, rare earths, silver, uranium, and others.[13] This investment is not exclusively FDI, as it includes investments by domestically owned firms; nonetheless, figure B.1 shows that aggregate exploration investment and net FDI inflows are positively correlated.

To understand the expenses that fall under exploration investment, we randomly sampled 80 firm-year observations where conflict occurred within 30 kilometers of a firm's mining project. All of the available annual reports (62) detail exploration investment, listing costs related to drilling, surveying, assaying, scoping, and feasibility studies. We checked whether firms include security-related costs under exploration, and only 17 mention security concerns: seven do not list any security expenditure; nine explicitly exclude security spending from exploration spending, including it instead under general, administrative, or other costs; and only one (Torex Gold Resources Inc. in 2014) lists security spending under exploration. Companies exposed to conflict may spend on security, but this is not captured by our outcome variable.

Our data include 4,331 firms investing in 177 countries (the decision not to invest is also an observation in our data). This is not a balanced panel: a firm does not enter our data set until it invests in at least one country. The data exclude investments under US$100,000; nonetheless, SNL estimates that this covers

12. This also limits spillovers that result from the rapid reallocation of investment across space.

13. Iron ore exploration expenditure was added in 2011. Fuel minerals are not included.

95% of commercially oriented nonferrous exploration expenditure.[14] Table B.1 summarizes the regions and commodities that constitute our data.

Table B.2 shows that total annual investment over our study period closely tracks global prices for metals. While developing a mine is a long-term investment, exploration activity responds rapidly to changes in prices and market sentiment. This is because most exploration is undertaken by small, "junior" firms that rely on fickle equity financing (Humphreys 2015, 129). The typical mining exploration firm invests in a small number of countries: the average firm invests for roughly six years in just over two countries. This average level of diversification is pulled up by outliers: a very small number of firms invest globally, in up to 60 countries. The modal firm concentrates its investments in a single country, and, even when firms do invest in multiple countries, they tend to concentrate spending in a single country. We show this in figure B.3(a) by plotting the effective number of countries in which firms invest.[15]

This low level of diversification highlights that the largest mining companies (e.g., BHP or Rio Tinto) do not represent the vast majority of firms. Indeed, globally there are only 100–150 "major" mining firms (Humphreys 2015, 10), whereas our sample includes exploration investment by 4,331 firms. Most companies engaged in mining exploration are "junior" mining firms—small companies that often specialize in exploration and mine development; 91% of mining projects in our data are owned exclusively by these junior firms.

Descriptions of these junior firms suggest that they prefer weakly regulated environments. They "[take] 'short cuts' by using bribes and other corrupt inducements to attain their objectives" and often fail to meet environmental or social standards (Marshall 2001, 17). Junior companies do not boast the large corporate social responsibility programs of their major counterparts. Rather, they often fail to engage their host communities, manage their environmental impacts, or encourage sustainable development (Dougherty 2013). This tendency to skirt regulations and industry standards relates to three common features of these companies: (1) their financiers typically do not require compliance with environmental and social standards; (2) these little-known firms do not worry about scandals damaging their reputations; and (3) these companies (sometimes described as "cowboys") lack strong corporate governance and, instead, reward employees who advance short-term objectives using unethical or corrupt methods (Dougherty 2013; Marshall 2001).

14. Mining investments typically exceed this threshold given the high costs of specialized inputs.

15. The average country-year includes over 20 different firms making investments (see fig. B.3(b)).

Armed conflict data

To code our independent variable, we use the Uppsala Conflict Data Program's Georeferenced Event Dataset (UCDP GED).[16] A conflict event is "an incident where armed force was used by an organized actor against another organized actor, or against civilians, resulting in at least one direct death at a specific location and a specific date" (Croicu and Sundberg 2017, 2). When conducting analyses at the firm-country-year level, we only retain those conflicts that can be geocoded to an exact location or nearby place name (see fig. B.5 for a mapping of all such events; table B.2 summarizes the severity of conflict across continent and subregion).[17] We further restrict attention to events between 1997 and 2014, the years for which we have exploration investment data.

We also separately examine three different types of conflict classified in the UCDP data: (1) state-based events: an organized actor uses armed force against another organized actor, of which at least one is the central government; (2) one-sided events: the government uses armed force against civilians; and (3) nonstate events: an organized actor uses armed force against another organized actor, neither of which is the government.

Measuring exposure to armed conflict

SNL provides data on the locations of commercial (nonfuel) mining projects (see fig. B.4). We know the owners of each project (and their respective shares) and use this information to link projects to the firms making exploration investments.[18]

By mapping both mining projects and armed conflicts, we can determine whether a conflict occurred within a certain distance of a project (partially) owned by a specific firm. Rather than choosing a single distance cutoff, we use multiple bandwidths—buffers around mining projects of varying radii (see fig. C.1). For every firm-country-year, we count the number of conflicts that occur within a given bandwidth across all of their projects. A firm can only be directly exposed to conflict if it

16. We exclude the Quebec Biker War—a turf war in Montreal between the Hells Angels and the Rock Machine, which took place between 1994 and 2002. Canada is otherwise coded as having an eight-year armed conflict.

17. For the countries and years in our sample, just over 27% of events can only be geocoded to the second-order administrative district (e.g., counties in the United States). As our analyses hinge on measures of proximity, we exclude such events. This does not distort our results. We drop firm-country-years where a firm operated a project in an ADM2 (and year) with an excluded event; our point estimates remain stable, and our inferences unchanged (see table D.1).

18. We use the detailed work histories associated with each project to extract the first and last years that activity took place at each mining site. This allows us to incorporate early-stage projects that have not yet started producing but where prospecting or construction has started.

already operates a project in the country where violence takes place.

The Euclidean distance between conflicts and mining projects has attractive features: it is easy to understand, can be computed globally and does not vary over time, does not require auxiliary data, and follows past work from Dai, Eden, and Beamish (2013, 2017). Yet, mining projects operating in the buffer zones around armed conflict are often in rural and rugged parts of low- and middle-income countries—settings with limited infrastructure, where travel is difficult. Using a global data set of roads, we estimate how long you would have to travel to get from the conflict site to a mining operation that falls in the first, 5–20 kilometer buffer zone (see app. C.1). While the average Euclidean distance is 13.7 kilometers, the average path distance along any known road is three times larger (39.3 km). Yet, even this shortest path distance is an understatement, as it does not account for road quality. (Moreover, for 4% of cases, the roads closest to the conflict do not even connect to the roads closest to the mine.) Using weights that reflect the estimated travel speeds along different roads, we estimate that the average weighted distance from conflict sites to mines is over 71. That is, these sites are separated by a "travel distance" equivalent to getting on a clear freeway and driving just over 71 kilometers, which is five times the average crow-flies distance. Travel costs dampen mines' exposure to nearby conflicts. Exposure could, of course, be measured in other ways given additional data (e.g., the destruction of transport infrastructure).

Christensen (2019) finds that relatively few commercial mines in Africa have been the sites of armed conflicts. Those results are consistent with what we find globally: we identify just 94 firm-country-years where a conflict occurred within 10 kilometers of a mining project in the same or previous year, but 914 firm-country-years where conflict occurred within 60 kilometers of a mining project in the same or previous year (see table B.3). These 914 firm-country-year observations represent US$3.31 billion of exploration investment.

RESEARCH DESIGN

We evaluate the effects of conflict exposure on firms' investments in conflict-affected countries. We estimate three causal effects that correspond to different extents of exposure: (1) the effect for firms with operations at a conflict site (τ^{site}); (2) the effect for firms with operations in the buffer zone (τ^{buffer}); and (3) the effect for firms with operations within a conflict-affected country, but outside the buffer zone (τ^{country}). If a firm has a project at a conflict site, that project is also within a buffer zone and in a conflict-affected country. Our model allows us to decompose the total effects that we estimate and, thus, separate the potentially cross-cutting effects of operating near a conflict site that is nested in a larger buffer zone. Specifically, we assume:

$$\begin{aligned} \tau^{\text{site}} &= \zeta + \eta + \theta \\ \tau^{\text{buffer}} &= \eta + \theta \\ \tau^{\text{country}} &= \theta, \end{aligned}$$

where ζ parameterizes the effect attributable to operating at a conflict site; η, to operating in buffer zones; and θ to operating in a conflict-affected country. With three equations and three unknowns (ζ, η, θ), we use our estimates to recover these parameters (e.g., $\hat{\zeta} = \hat{\tau}^{\text{site}} - \hat{\tau}^{\text{buffer}}$).[19]

We also estimate the effect of armed conflict on aggregate investment. This both helps to relate our setting to past studies of aggregate investment and is a relevant quantity for those interested in predicting total cross-border flows. This effect ($\bar{\tau}$) is a weighted sum of τ^{site}, τ^{buffer}, and τ^{country}, with weights equal to the number of firms within each extent of exposure in a country-year:

$$\bar{\tau} = N^{\text{site}} \cdot \tau^{\text{site}} + N^{\text{buffer}} \cdot \tau^{\text{buffer}} + N^{\text{country}} \cdot \tau^{\text{country}}. \qquad (1)$$

This equation highlights the danger associated with inferring firms' behavior from changes in aggregate investment. If τ^{site} is negative but τ^{buffer} is positive, the aggregate effect could appear to be zero. Yet, the inference that firms do not respond to conflict in their investments would be exactly wrong in that case: they respond, just in opposing directions. Conflict may create winners and losers among mining firms who are exposed to violence at different levels. However, this heterogeneity cannot be uncovered in the aggregate data.

In our data, we observe how much a firm separately invests in each country annually (i.e., an observation is the firm-country-year). To estimate the causal effects of different extents of conflict exposure, we employ a generalized difference-in-differences design, leveraging the differential change in investment among exposed firms (technically, firm-countries) relative to the change among unexposed firms. This design invokes the standard parallel trends assumption—namely, that exposed and unexposed firms would have the same trends in investment absent any exposure to conflict.[20]

We fit a linear two-way fixed effects model with firm-country and firm-year fixed effects. Firm-country fixed effects absorb time-invariant features that explain why firms' investment levels differ across countries (e.g., political connections in a specific state). Firm-year fixed effects address time-varying, firm-specific factors (e.g., changes in management) that could affect investment across the countries in a firm's

19. We take the natural logarithm of investment, so additivity implies that the percentage change differs, not the levels.

20. We also invoke stable unit treatment-value and a no-carryover assumptions.

portfolio. This also rules out confounding from time-varying global shocks. We estimate:

$$y_{ict} = \alpha_{ic} + \delta_{it} + \theta C_{ct} + \sum^{k} \kappa^k D^k_{ict} + \nu_{ict}, \quad (2)$$

where $i \in \{1, 2, \ldots, 4{,}331\}$ indexes firms, $c \in \{1, 2, \ldots, 177\}$ indexes countries; and $t \in \{1, 2, \ldots, 18\}$, year. The term y_{ict} is exploration investment (logged); C_{ct} is an indicator for whether an armed conflict occurred in country c in year t or in the previous year $t - 1$; D^k_{ict}, our measure of conflict exposure, is an indicator for whether a conflict occurred in bandwidth k for any of firm i's projects in country c and year t or $t - 1$. This coding captures changes in firms' investment that manifest in the year of and after conflict, recognizing that instantaneous adjustment may not be possible. The term ν_{ict} is a firm-country-year-specific error term. We cluster our standard errors at the firm-year level.

Our second specification omits C_{ct} and includes country-year fixed effects, which account for any country-specific, time-varying factors affecting conflict and investment (e.g., regime change):

$$y_{ict} = \alpha_{ic} + \delta_{it} + \gamma_{ct} + \sum^{k} \kappa^k D^k_{ict} + \nu_{ict}. \quad (3)$$

This represents a generalized triple-difference design. Our results are consistent using different sets of fixed effects. We present results from equations (2) and (3) below and include results from a simpler model with firm-country and year fixed effects in table D.2.

For the analysis of aggregate investment, we rely on a two-way fixed effects design with country and year fixed effects, comparing changes in investment between countries that are differentially affected by armed conflict. We estimate the following panel model:

$$Y_{ct} = A_c + \Delta_t + \beta C_{ct} + \varepsilon_{ct}, \quad (4)$$

where Y_{ct} is aggregate investment (logged), A_c represents the country fixed effects, and Δ_t represents the year fixed effects. We cluster our standard errors on country.

RESULTS

We find that firms operating at conflict sites reduce investment in response to conflict; that firms operating in territory surrounding conflict, but separated from fighting, decrease investment; and that firms far from violence see a small negative effect. We show that the aggregate result of these effects is a negative relationship between investment in a country and conflict. We explore mechanisms underlying these effects and show that conflict disrupts production near mining operations, undermines state capacity, and creates policy uncertainty or reputational risk.

Effect on investment at the firm-country level

Across specifications and samples in table 3, we consistently find three main results. First, firms dramatically reduce their exploration investment in countries where their operations are located at conflict sites (within 5 km of an armed conflict). Second, firms actually increase their investment in countries where their operations fall between 5 and 60 kilometers of armed conflict. Finally, firms modestly reduce their investment in conflict-affected countries where their operations reside far from the fighting (beyond 60 km).

In the first two models of table 3 we report estimates from equation (2). Model 1 includes a larger set of bandwidths, which code whether a firm has operations in a country within 0–5, 5–20, 20–30, 30–40, 40–50, or 50–60 kilometers of an armed conflict; model 2 collapses several of these bandwidths, coding just whether a firm has operations within 0–5 or 5–60 kilometers of an armed conflict. As these models do not include country-year fixed effects, we can also estimate the response of firms in conflict-affected countries but operating beyond 60 kilometers from fighting. In models 3 and 4 we include country-year fixed effects per equation (3), which absorb the effect of operating further than 60 kilometers from an armed conflict.

We find that firms cut their investment in countries where their operations abut the site of an armed conflict (i.e., fall within 0–5 km). After excluding firm-country pairs with no investment over our study period, average exploration investment (logged) is 5.9 (SD = 5). In model 1, the estimated effect of having operations within 0–5 kilometers of conflict is roughly 40% of this mean (or half of a standard deviation). This coefficient remains stable when we include the additional country-year fixed effects in model 3. While large, these estimates are imprecise given the small number of firms within this extent of conflict exposure (see table B.3).

By contrast, we find that firms increase their investment in countries where their projects fall in the buffer zone surrounding armed conflict. We estimate a positive and significant investment response for firms in countries where their operations fall 5–60 kilometers from fighting. Our estimates initially increase in magnitude as we move further from the conflict, peaking at 30–40 kilometers. This pattern is apparent in figure 2, which plots the coefficients from models 3 and 4. Our estimate from model 4 implies that firms increase their investment by over 25% of the mean in countries where their operations fall 5–60 kilometers from armed conflict.[21]

21. Our results are not driven by high-intensity civil wars (see table D.3 and fig. D.1).

Table 3. Effect of Armed Conflict on Investment at the Firm-Country Level

	Log(Exploration Investment + 1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0–5 km	−2.48**	−2.43*	−2.43*	−2.39*	−3.54**	−2.36*	
	(1.26)	(1.25)	(1.26)	(1.25)	(1.77)	(1.42)	
5–20 km	1.52***		1.54***		1.73***	1.88***	1.36***
	(.45)		(.45)		(.48)	(.47)	(.52)
20–30 km	1.15**		1.16**		1.96***	.80	1.07*
	(.49)		(.49)		(.60)	(.52)	(.56)
30–40 km	2.87***		2.87***		2.80***	2.68***	2.68***
	(.43)		(.43)		(.50)	(.47)	(.49)
40–50 km	1.64***		1.65***		1.25**	1.75***	1.65***
	(.44)		(.43)		(.53)	(.50)	(.48)
50–60 km	.83*		.83*		.67	.81	1.19**
	(.46)		(.46)		(.55)	(.54)	(.51)
5–60 km		1.63***		1.64***			
		(.24)		(.24)			
>60 km	−.002*	−.002*					
	(.001)	(.001)					
Firm (F) sample	All	All	All	All	Single country	Single project	All
Country (C)-year (Y) sample	All	All	All	All	All	All	No projects at conflict site
Firm-country FE	768,888	768,888	768,888	768,888	768,888	735,373	768,518
Firm-year FE	42,544	42,544	42,544	42,544	36,933	33,919	42,544
Country-year FE	0	0	3,186	3,186	3,186	2,832	3,168
Observations	7,530,288	7,530,288	7,530,288	7,530,288	6,537,141	5,992,771	7,482,685

Note. This table reports results from OLS models estimated using eqq. (2) (models 1–2) and (3) (models 3–7). We cluster standard errors at the firm-year level, shown in parentheses. The dependent variable is logged exploration investment. The independent variables code whether a fatal armed conflict occurred in a given year (t) or in the year prior ($t - 1$) within a specified distance from a mining project. Models 3–7 include country-year fixed effects (FE), which absorb the >60 km term. We subset to firms that invest in a single country in model 5 or in a single project in model 6. In model 7, we subset to countries with no projects at a conflict site.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

Finally, firms modestly reduce investment where they operate further from violence (i.e., beyond 60 km). For any single firm-country, this negative effect is small. Yet, the vast majority of firms exposed to conflict only see fighting from afar. Per equation (1), this small negative effect weighs heavily when estimating the effect of conflict on aggregate country-level investment.

We bolster the identifying parallel-trends assumption by showing that there is no evidence that investment trends diverge prior to conflict. Figure D.4 plots the coefficients on the leads and lags of the indicator for having operations within a buffer zone, and we see no significant differential change in investment prior to conflict.

We parameterized the effect of operating at a conflict site as ζ, of operating in a buffer zone as η, and being in a country with conflict as θ. Using model 2, we present estimates for these three parameters in table 4.[22] First, operating at the site of battles dramatically reduces investment ($\hat{\zeta} = -4.06$). Second, operating in a buffer zone encourages investment by mining

22. We use the following mapping: $\hat{\tau}^{\text{site}} = \hat{\kappa}^{[0-5]}$; $\hat{\tau}^{\text{buffer}} = \hat{\kappa}^{[5-60]}$; $\hat{\tau}^{\text{country}} = \hat{\theta}$.

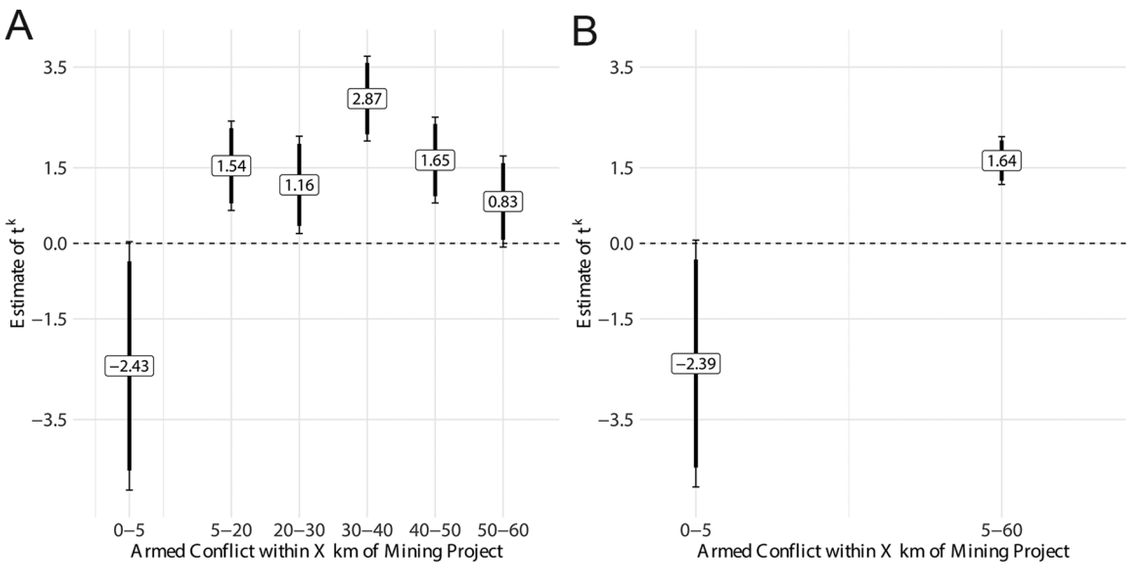

Figure 2. Effect of armed conflict on investment at the firm-country level. This figure displays coefficients and 95% confidence intervals from table 3, column 3 (panel *A*) and table 3, column 4 (panel *B*). In panel *B*, we reject the null that $\tau^{0\text{–}5} = \tau^{5\text{–}60}$ with $p < .005$. *A*, Multiple bandwidths. *B*, Two bandwidths.

firms ($\hat{\eta} = 1.63$). Finally, operating in a country with conflict deters investment, though the effect is minimal if a firm is far from the fighting ($\hat{\theta} = -0.002$). The effects are all significantly different from zero at the $\alpha = 0.05$ level. The difference in effects between the conflict site and buffer zone and between the conflict site and conflict-affected country are each significant at the $\alpha = 0.01$ level.

Table 4 also relates our findings to our first three hypotheses: we find a large negative response in countries where firms operate at conflict sites; a smaller, but still substantial, positive response where firms operate in buffer zones; and a small negative effect in countries where firms' operations are well removed from the fighting. While there could still be offsetting considerations within buffer zones—firms may both enjoy weakened oversight and lament weakened property rights—the investment-encouraging mechanisms appear to dominate. Two characteristics of the mining sector may mitigate the negative effects: firms cannot relocate their assets in response to conflict, as mines are tied to geological features; and, as a consequence, firms may spend more on security and other interrupted public services, mitigating harms that might make other firms halt investment.

We might worry that firms reallocate from conflictual to more peaceful environments and that these responses amplify our estimates. Our context helps mitigate such concerns. Exploration portfolios cannot be quickly adjusted. Adding properties to an exploration portfolio, particularly from a new country, typically takes years and requires several lengthy steps: (1) local incorporation, which may take one to three months; (2) exploration license application writing and review, at least three months; (3) access approval from surface rights holders and indigenous consultations, at least two months; (4) water permitting, at least a month; and (5) an environmental impact study, at least three months. Our estimates reflect firms' investment response in the year of or immediately following conflict; reallocation over such a short time scale would be exceptional.

We use subgroup analysis to empirically assess the plausibility of such reallocation. We expect firms invested in multiple

Table 4. Parameter Estimates

	Parameter	Estimate	SE	2.5%	97.5%
Conflict site	$\hat{\zeta}$	−4.060	1.240	−6.491	−1.630
Buffer zone	$\hat{\eta}$	1.636	.243	1.160	2.112
Conflict-affected country	$\hat{\theta}$	−.002	.001	−.003	0

Note. This table's estimates are based on table 3, model 2; standard errors are computed via the delta method.

countries to be better able to reallocate exploration resources in response to conflict.[23] We drop firm-years in our sample that were invested in multiple countries based on a two-year running lag and reestimate equation (3) in model 5. Our inferences are unchanged (see fig. D.2). Even if a firm is working in a single country, perhaps it can reallocate across multiple projects. No firm in our data has projects affected by conflict at the site of violence and in buffer zones. Similarly, no firm has operations in buffer zones and far from violence in a conflict-affected country. In model 6, we restrict the sample to those firms with a single project and reestimate equation (3). Our inferences are again unchanged (see fig. D.3).

Specialized capital and labor employed by mines at conflict sites might flee violence, leading to increased supply in the surrounding area. Firms in the buffer zone (or beyond) might increase investment to take advantage of lower resulting input prices. Our data allow us to rule out this concern. First, conflict rarely occurs at mining sites, making displacement unlikely (table B.3). When we observe a firm operating within a buffer zone around conflict, there is often no mining operation at the conflict site from which capital or labor might have fled. Nevertheless, in model 7, we drop country-years where any mining project is at a conflict site and continue to find that firms increase investment in countries where they operate within buffer zones surrounding conflict.

A final related concern is that firms reallocate their exploration investment over time. Specifically, firms operating projects in buffer zones may ramp up their investments in an effort to complete exploration before nearby conflicts escalate or creep closer. Such behavior is inconsistent with the business strategy literature, which argues that firms typically adopt a "wait and see" approach and avoid committing major resources when facing emerging risks (Courtney, Kirkland, and Viguerie 1997, 74). Moreover, we assess this empirically by looking at whether heightened investment in buffer zones immediately after conflict is then followed by reduced investment—the pattern consistent with shifting the timing of investment without changing the overall level. Figure D.4 and table D.4 demonstrate that, in fact, the positive effects of exposure to conflict in the buffer zone persist for several years, ruling out such temporal displacement.

Effect on investment at country level

Our country-level results, which provide a test of hypothesis 4, are consistent with a majority of existing literature: the incidence of fatal armed conflict reduces exploration investment. Table 5 reports consistent estimates from equation (4) using different samples and measures of conflict.

Model 1 includes our full sample—177 countries over 18 years—and finds that the incidence of at least one fatal armed conflict in the current or previous year reduces aggregate investment by 0.77 log points. This is just over one-quarter of the average within-country standard deviation (2.78) and roughly 8% of the mean ($\overline{y_{ct}} = 9.98$). Model 2 drops countries with no investment, and our estimates are of similar magnitudes. We also examine the extensive margin: conflict reduces the number of firms operating in the country with conflict (see table E.1).

These country-level results are consistent with the estimates from our model based on the firm-level analyses above. The vast majority of firms investing (or considering investing) in a country operate outside of conflict sites and buffer zones. When we aggregate the effects of conflict to the country level, the largest component of that sum is the negative effect of these firms with minimal conflict exposure. The parameters η and ζ can be sizable, but if they only apply to a relatively small proportion of firms, they will be washed out when we aggregate the data.

Finally, we investigate how effects vary by the type and intensity of violence. Model 3 shows, intuitively, that settings with multiple conflicts see a larger reduction in exploration investment; however, both coefficients are negative, and the magnitudes are not significantly different. Models 4–7 look at whether different types of fatal armed conflict—state-based, one-sided, or nonstate—have differential effects on exploration investment. Focusing attention on model 7, we find that one-sided and nonstate conflicts have larger, negative effects.[24]

In appendix F.1, we report analyses that separate "major" or multinational mining firms—firms that, by virtue of their size and visibility, may be especially concerned about their reputations. We do not find a significantly different response among these subsets of firms (see table F.3).[25] This analysis does not, of course, rule out reputational effects—these could affect all firms, or large firms may have other compensating features.

Our empirical strategy assumes parallel trends in investment (logged) among countries that are and are not affected by

23. As noted above, junior miners, who represent the vast majority of firms, tend to concentrate their investments in a single country or even on a single project (see fig. B.3).

24. This variation requires investigation beyond the scope of this paper. One ex post rationalization would be that state-based violence involves well-defined combatants; one-sided and nonstate conflicts may be less predictable and involve greater uncertainty about the extent of collateral damage.

25. However, while the interaction term is insignificant, major mining firms appear to respond more aversely to state-based conflict, which could reflect concern about being associated with repressive states (per Henisz 2017).

Table 5. Effect of Armed Conflict on Investment at the Country Level

	Log(Exploration Investment + 1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1 (Conflicts > 0)	−.65**	−.56					
	(.32)	(.34)					
1 (Conflicts = 1)			−.41				
			(.37)				
1 (Conflicts > 1)			−.74**				
			(.37)				
1 (State-based > 0)				−.17			.07
				(.39)			(.39)
1 (One-sided > 0)					−.87***		−.85***
					(.31)		(.32)
1 (Nonstate > 0)						−.69	−.62
						(.45)	(.46)
p-value on *F*-statistic	.04	.1	.12	.65	.01	.13	.02
Country-year sample	All	Recipients	All	All	All	All	All
Country FE	177	145	177	177	177	177	177
Year FE	18	18	18	18	18	18	18

Note. This table reports the results from OLS models estimated using eq. (4). We cluster standard errors at the country level, shown in parentheses. The dependent variable is exploration investment (logged plus one). The main independent variable codes whether conflict occurred in a given year (t) or in the year prior ($t - 1$). Models 2–7 report estimates from eq. (4) using different samples or measures of conflict. FE = fixed effects.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

fatal armed conflict. While untestable, we bolster this assumption by showing that investment does not change in anticipation of conflict. Figure F.1 plots the coefficients on the leads and lags of the indicator for a fatal armed conflict (see also table F.2). We see no differential change in investment prior to conflict (i.e., the coefficients on the leads are close to zero), suggesting that the countries that will be attacked are not seeing a spike or falloff in investment in the years before conflict breaks out. The figure also reveals that the negative effects on investment materialize in the first and second years after conflict. The effect on investment is not immediate, suggesting that the allocation of exploration investment may not be updated in real time but adjusted annually (e.g., at the start of the fiscal year).

MECHANISMS

We incorporate auxiliary data to explore the mechanisms outlined in our theory of conflict exposure and investment: that conflict disrupts production at proximate mining operations, undermines state capacity, and creates policy uncertainty or reputational risk. We regard these as secondary, and more speculative, analyses given data and design limitations we note below.

Disrupted production

Mihalache-O'keef and Vashchilko (2010) recount stories of operations being seized or suspended during conflicts. Local violence threatens staff, severs supply chains, and can destroy critical infrastructure. Ksoll, Macchiavello, and Morjaria (2016, 3) find, for example, that flower exporters in regions affected by Kenya's postelection violence saw their exports fall by 38%. At the height of the violence, half of their employees were not showing up for work. Research in Sierra Leone (Collier and Duponchel 2013) and Colombia (Camacho and Rodriguez 2013) echo these findings, showing lower production and more business closures in high-conflict areas.

We assess this mechanism using the subset of mining projects in Africa, for which we have annual production data (e.g., how many tons of lead or ounces of silver a mine pulled out of the ground). A single mine can produce multiple minerals, so our unit of analysis is the project-mineral-year.

We look at the change in production at projects near the site of a recent conflict (within 5 km) versus further afield. Employing a specification similar to equation (3), but with project, year, and mineral fixed effects, we find changes on the extensive and intensive margins for mines at conflict sites: the probability of any production declines by 20 percentage points; the quantity produced (logged) falls by about 20% of the mean (see table G.1). The latter, while sizable, is not significant.[26] For projects in a buffer zone but outside of a conflict site (5–60 km from a recent conflict), we find small and insignificant negative effects on the likelihood and intensity of production. The effect of being within 5–60 kilometers of a conflict is an order of magnitude smaller than being next to the fighting (model 2). While these differences are large in magnitude, our estimates are imprecise, and we cannot rule out the null hypothesis of no difference between projects located at conflict sites or further afield.

This pattern is consistent with our earlier findings on investment: operating at a conflict site can hamper production and, as a consequence, discourage investment. Yet, these dampening effects are not apparent in the broader buffer zone that surrounds these conflict sites.

State capacity

Conflict could be a boon for companies if it reduces costly oversight. We assess whether conflict reduces the tax revenues derived from natural resource production. We emphasize that this is not the only aspect of state capacity that may affect firms' decisions in buffer zones around conflict. It is, however, one dimension that we can measure. We estimate the elasticity between natural resource production and resource tax revenues, and whether this elasticity is reduced (i.e., less tax revenue is derived from production) in countries experiencing armed conflict.

We lack firm-level data on tax payments and rely on a country-level measure of resource tax revenues from the International Centre for Tax and Development (ICTD).[27] We also compile data from the World Mineral Statistics on annual production for roughly 100 minerals for nearly every country.[28] To compute the value of natural resource production, we merge this production data with world commodity prices tracked by the World Bank, US Geological Survey, and US Energy Information Administration. Thus, for every year, we can calculate the dollar value of resources produced (our independent variable) and the amount of resource tax revenue collected (our dependent variable). We log both measures to estimate an elasticity and interact our measure of resource production with a country-level indicator for armed conflict. We focus on the change in this elasticity, as the direct effect of conflict on tax revenues conflates conflict-induced changes in both production and fiscal capacity. Our goal here is to better isolate the latter.

As is apparent in figure G.2 (see also table G.2), the production elasticity of resource tax revenues is lower in countries affected by one-sided or state-based conflicts in the current or previous year (models 1 and 3). For a given amount of mineral production, governments recently affected by these types of conflicts collect less in taxes, a finding that is consistent with conflict diminishing fiscal capacity.[29] We find no significant effect of nonstate conflicts, which do not involve the government. These results suggest that conflict involving the state may undermine fiscal capacity.

While we cannot specify where state fiscal capacity erodes, our findings align with case studies of mining companies profiting from operations in ungoverned areas (e.g., Reno 1999). They could also help explain why we see greater exploration investment among companies operating projects in buffer zones that surround recent fatal armed conflicts: the companies in these gray zones suffer minor production disruptions while benefiting from less oversight.

Policy change

We look at whether conflict raises concerns about changes in policy. Concretely, we estimate equation (4) using two different outcomes. First, as a manipulation check, we look at whether the incidence of UCDP armed conflicts shifts the "Internal Conflict Index" compiled by the International Country Risk Guide (ICRG)—a data set used by firms that contains measures of multiple components of political risk.[30] We find that the armed conflicts we use in our analyses raise concerns that political violence will impact the country's governance (see

26. Standard errors are clustered on project. Our independent variable here captures whether a conflict occurred in that bandwidth in any of the three previous years, i.e., from $t-1$ to $t-3$. A shorter lag structure generates results in the same direction but of smaller magnitudes. Our estimates from a dynamic panel model (fig. G.1) indicate that production for mines at conflict sites continues to decline three years after violence takes place.

27. ICTD's Government Revenue Dataset (GRD) provides a comprehensive, standardized data set of tax revenue. We focus on countries that report any revenues from natural resources in 1997, at the start of our study period.

28. The WMS draws on "home and overseas government departments, national statistics offices, specialist commodity authorities, company reports, and a network of contacts throughout the world" (British Geological Survey 2017).

29. The direct effects of conflict on taxation are included in all models in table G.2 but are omitted from the table.

30. The index assess political violence "and its actual or potential impact on governance" (PRS Group 2012).

table G.3). In the year of or immediately following a fatal armed conflict, ICRG's Internal Conflict Index falls a half point on a 12-point scale (50% of the average within-country standard deviation for this index). While the UCDP data include small skirmishes and battles, these events shape country-level assessments of internal violence and its impacts on governance.

Second, we consider the effect of fatal armed conflict on ICRG's Government Stability Index, which provides an "assessment both of the government's ability to carry out its declared program(s), and its ability to stay in office" (PRS Group 2012, 3).[31] This measure operationalizes two concerns about investors' aversion to policy change: investors worry both about whether the current government will survive in office and, if so, whether it will be forced to change course. We find that the incidence of fatal armed conflict decreases assessments of government stability (model 4 of table G.3): a reduction of 0.2 is roughly 15% of the average within-country standard deviation for the Government Stability Index. This finding is robust to multiple ways of measuring conflict (model 5). The effect is larger for nonstate and one-sided conflicts (model 6).

We also look at whether conflict deters entry by new companies. As new entrants are unlikely to invest at conflict sites and will not be subject to taxation for several years, the estimated effect among these firms helps us isolate the aggregate country-level effect, which we attribute to increased uncertainty around policy changes or reputational risk.[32] We estimate equation (4) but limit our dependent variable to exploration investment in country c in year t to firms that had not previously invested in country c. Our estimates in table G.4 are comparable in magnitude to those reported for the full sample. Conflict does deter investment by potential new entrants to a country.

DISCUSSION

Earlier empirical work largely supports the oft-repeated claim that conflict is bad for business. This idea underlies policy efforts to prevent and end armed conflict that assume private sector support. A 2016 report from the World Economic Forum, for example, argues that "international and local businesses have a critical role to play in finding ways to minimize fragility and build resilience in violence-affected societies. A key reason, among others, is because fragility—including conflict and crime—is bad for business. It generates direct and indirect opportunity costs all along the value chain" (World Economic Forum 2016, 6).

Yet, past research supporting this claim relies overwhelmingly on cross-national analysis, which masks differential effects of conflict on firms operating (or considering operating) in a country. Theoretically, we argue that conflict may deter investment by disrupting production or raising policy uncertainty but that it may encourage investment where it hampers oversight. Moreover, whether these mechanisms apply to a firm depends on its geographic exposure to violence.

Using firm-level panel data on mining exploration investment, we show that effects depend on the conflict exposure of firms. We show that mining firms pull back investments at the sites of violence, and the disruption of mineral production may explain why. However, in buffer zone surrounding the fighting—where neither the state nor its armed challengers fully control territory—firms seem to double down on exploration investment. In these areas of imperfect control, the state may be unable to oversee the sector, thereby lowering costs in the short term. We find that effective mineral tax rates decline during conflict. Finally, we show that armed conflicts raise concerns that political violence will impact governance and undermine government stability. This suggests that conflict could deter investment by raising the likelihood of policy change or government turnover.

These results demonstrate that conflict is not uniformly bad for business. Indeed, some firms may benefit from how conflict weakens state capacity. Where firms can privately secure their property and do not depend on public services, they may associate improved state capacity with costly regulation or taxation. Different sectors and firms can, thus, vary in their propensity to invest in weak and fragile states. These findings expand upon past case studies (e.g., Fisman 2001), which find that conflict and instability benefit (or only harm) certain companies. The results also parallel efforts to understand when states selectively tolerate or even encourage instability in ungoverned spaces (Christensen et al. 2019). Our findings suggest new directions for research into how heterogeneous exposure to conflict—and the ungoverned gray zones created by conflict—affect conflict termination, postwar economic growth, and the distributional consequences of war.

We focus on the effects of armed civil conflict, which is only one factor included in commonly used measures of political risk. As Snider (2005) points out, the measurement of political risk is atheoretical: the ICRG, for example, weights armed conflict heavily but does not articulate why internal conflict receives twice the weight of democratic accountability. We note at least three ways that political risks differ: (1) specificity (do they apply to all or a subset of firms), (2) severity (what is the scale of potential losses), and (3) mitigation (can firms mitigate the risk through private precautions). As we describe earlier, modern armed conflicts tend to be localized, and

31. The index is a composite of three items: "government unity," "legislative strength," and "popular support."

32. The effect of policy uncertainty or reputational risk on investment may be different for existing and new investors. We only estimate the latter in this exercise.

individual conflict events near mining projects involve relatively few fatalities. Moreover, firms can employ private security to protect their operations. By contrast, nationalization of an industry affects all firms in a sector, implies the loss of immobile assets, and would be difficult for any individual firm to mitigate. We, thus, think of the latter risk as being less specific, more severe, and harder to mitigate than localized armed conflicts. Future research could provide a richer framework for differentiating political risks as well as empirical assessments of how these different risks affect investment behavior.

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