

# How Does Armed Conflict Shape Investment? Evidence from the Mining Sector<sup>\*</sup>

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This Draft: January 21, 2020

## Abstract

How does conflict affect firms' investment decisions? Past research on instability and investment generates mixed findings: a third of studies in our systematic review 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' proximity to violence. Conflict can deter investment by disrupting production or raising uncertainty. Yet, conflict can encourage investment by hampering government oversight. 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 the territory surrounding conflict, but at a remove from the fighting, actually increase their 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.

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<sup>\*</sup>We thank Maryam Aljafen, Matthew Amengual, Witold Henisz, Leslie Johns, Ethan Kapstein, In Song Kim, Eric Min, Margaret Peters, Jan Pierskalla, Michael Ross, Aaron Rudkin, Renard Sexton, Mehdi Shadmehr, Jacob Shapiro, and participants at the Strategy and the Business Environment conference 2019, Empirical Studies of Conflict conference 2019, American Political Science Association conference 2019, University College of London, the Vancouver School of Economics, the Symposium on Natural Resource Governance for Young Scholars, and the UCLA IDSS Workshop for helpful comments.

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When firms and individuals fear that future economic returns will be destroyed or expropriated, they have little incentive to invest. This folk theory of development motivates a large literature in comparative and international political economy which identifies institutions that reassure potential domestic (e.g., [North 1981](#); [Stasavage 2002](#); [Acemoglu, Johnson and Robinson 2005](#); [Besley and Persson 2011](#)) and foreign investors (e.g., [Vernon 1971](#); [Jensen 2003](#); [Li and Resnick 2003](#); [Büthe and Milner 2008](#); [Kerner 2009](#)).<sup>1</sup> 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).

This past work considers how institutional differences across countries shape investment. In this paper, we argue that armed conflict — 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 build a theoretical framework that identifies three channels through which armed conflict affects firms’ investment decisions.<sup>2</sup> First, conflict can disrupt or destroy production, discouraging investment. Second, conflict can undermine a state’s capacity, particularly in disputed territories. This could reduce oversight, which might lower costs and encourage investment.<sup>3</sup> Finally, conflict can increase uncertainty about the standing or policy agenda of the embattled government, leading risk-averse investors to divest.

Critically, we argue that these mechanisms operate at different geographic scales. Threats to production, we claim, are very local, affecting the small proportion of investments located at the sites of conflict. State capacity should be diminished in buffer zones — areas affected by armed conflict where the state’s control is disputed, but fighting is not active. Finally, uncertainty around policy changes or reputation risk impacts all firms operating in a country with conflict. In short, conflict should not have a uniform effect on firms’ investment decisions: firm proximity to violence shapes how it responds. Conflict may not always deter investment — a point underscored in recent work by [Barry \(2018\)](#) and [Osgood and Simonelli \(2019\)](#), who show that established MNCs and firms with market-specific investments are less responsive to violence given their higher costs of exit.<sup>4</sup>

Social scientists have long worked to quantify the impact of instability on investment levels (for an early contribution, see [Bennett and Green 1972](#)): our systematic review found 75 published

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<sup>1</sup> For a recent review of the literature in international political economy, see [Pandya \(2016\)](#).

<sup>2</sup> We are not the first to note that firms operating in the same country and sector can be differentially affected by conflict (see e.g., [Kobrin 1978](#); [Henisz and Delios 2001](#); [Collier and Duponchel 2013](#)).

<sup>3</sup> While investment is often thought to be a positive outcome, our focus here is on the determinants of investment; we take no stand on whether such investment is welfare-enhancing. Indeed, there is a substantial literature on the adverse effects of natural resource extraction (for a recent review, see [Ross 2015](#)).

<sup>4</sup> [Jamison \(2019\)](#) and [Lee \(2017\)](#) also report heterogeneous effects of conflict on investment depending, in the first case, on whether a sector enjoys a natural monopoly and, in the second, on the host governments’ capacity to deal with terrorism.

empirical studies of this relationship since 1990. These analyses describes how conflict shapes investment flows, with 64 percent of papers reporting 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.<sup>5</sup>

We advance the literature by addressing this ecological inference problem and offering empirical tests of our theoretical framework, which predicts divergent firm-level responses. We assemble 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. Beyond 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 percent of greenfield FDI in low-income countries in 2011 ([UNCTAD 2012](#), 64) and features in foundational work on the property rights and decision-making of foreign investors (e.g., [Vernon 1971](#); [Moran 1974](#)).

We find that a small number of firms with operations at conflict sites (within five kilometers of an armed conflict) reduce their investments dramatically following violence. Yet, firms operating in the territory surrounding conflict but at a remove from the actual fighting (up to 60 kilometers from an armed conflict) increase their investment. This effect is largest for firms with an operation that is 30 to 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. To empirically illustrate the ecological inference problem, we aggregate our data to the country-year level. Using models with country and year fixed effects, we show that armed conflict depresses aggregate investment.

We incorporate auxiliary data to explore several mechanisms empirically. First, using mine-level panel data from projects across Africa, we show that armed conflict does disrupt production, but only for mines located at conflict sites (within five kilometers of the violence). The likelihood

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<sup>5</sup> [Barry \(2016\)](#) offers a more comprehensive assessment of the additional insights that can be extracted from firm-level data.

that a mine produces anything falls by 30 percentage points two years after nearby conflict. Second, drawing on country-year data on both mineral production and tax revenues from natural resources, we show that the elasticity between mineral production and tax revenues 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 predicts firms’ investment responses based on 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 analysis help decompose aggregate findings and, in so doing, reveal that analysis of aggregate investment flows can miss the investment-promoting effect of conflict among a subset of firms.

Our research helps to advance several 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 (Herbst 2000; Boone 2003; Scott 2009). 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 grey zones, where the state’s authority is contested. Consistent with case studies from Guidolin and La Ferrara (2010) and Christensen, Nguyen and Sexton (2019), we find that certain firms can benefit from the state’s incomplete control.

This study also intersects with two strands of work in international political economy. First, seminal work argues that investors shy away from countries that cannot credibly protect their property rights (e.g., Vernon 1971; Moran 1974). More recent contributions expand upon this argument, showing how the characteristics of host governments (e.g., Jensen 2008; Lee 2017; Pinto and Zhu 2018), industries (e.g., Burger, Ianchovichina and Rijkers 2015; Lee 2016; Wright and Zhu 2018; Jami-son 2019), 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. Armed conflict generates divergent investment responses even among firms operating in the same country and sector.

Second, 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 (for a review, see [Kim and Osgood 2019](#)) and, more recently, foreign investment ([Barry 2016](#); [Zhu and Shi 2019](#)), which draw on rich firm-level data to develop and test new theories.

## 1. Systematic Review of Existing Empirical Work

Nearly five decades ago scholars began quantitatively studying how political instability shapes investment, using newly-available cross-national datasets ([Bennett and Green 1972](#); [Green and Cunningham 1975](#)). To assess the weight of this evidence, we conduct a formal systematic review, a form of meta-analysis used in medicine and other fields but rarely employed in the social sciences.<sup>6</sup> The goal is to surface and summarize all research that meets pre-specified criteria, rather than focusing on a researcher-selected subset which may, for example, exclude earlier work or research from adjacent disciplines. Using the protocol detailed in Appendix G.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 Figure A.14).<sup>7</sup>

Table A.1 describes the individual studies. The data used in each study cover multiple years, spanning 1950 to 2013, with the bulk of the observations coming from the four decades between 1970 and 2010. 64 percent find a negative conditional correlation between instability or conflict and investment (see Table 1).<sup>8</sup> 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 *benefitted* from Angola's civil war. The sudden end of the conflict in 2002 led to a four-percentage-point drop in cumulative abnormal returns for companies holding concessions in Angola. (Seven percentage points relative to a control portfolio of mining companies not invested in Angola.) "No matter how high

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<sup>6</sup> We follow the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines (see Appendix G.2).

<sup>7</sup> Related studies measure firm exit or entry as a categorical variable (e.g., [Barry 2018](#); [Camacho and Rodriguez 2013](#)). While our criteria led to the exclusion of these studies, these edge cases represent important contributions to the literature.

<sup>8</sup> Our coding reflects both the sign and statistical significance (at any level) of the point estimate, as defined by the author(s). When a study reports more than one model, we select models that include fixed effects or employ instrumental variables when available. All eight "mixed" studies report significantly negative results alongside null and/or significantly positive correlations between instability or conflict and investment. Results from one study, [Li, Murshed and Tanna \(2017\)](#), rely on separate models due to the use of FDI from different sectors as dependent variables.

**Table 1:** Mixed Findings from Past Studies of Instability and Investment

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

\* Studies are coded as mixed if they report point estimates that are not all of the same sign and statistical significance.

**Table 1** summarizes our systematic review. The number of studies are tabulated by whether they report positive results, null results, negative results, or mixed results (where in a single paper key results were a mix of positive, negative, and/or null). Columns 3 and 4 report the number of studies that employ unit and time fixed effects, respectively; and Column 5, reports the number employing instrumental variables designs. See Table A.1 for the list of studies and their results.

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” (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, measures of conflict or instability, and exploiting different sources of variation. This makes it difficult to pinpoint whether and why their findings diverge. We focus on three common limitations of past studies. First, despite having panel data, only half of the studies include unit fixed effects to address concerns about time-invariant omitted variables (see Table 1). Second, 40 percent of the studies rely on a composite measure of political risk, of which violent events is only one component (for a critique of these subjective measures, see [Henisz 2000, 3](#)).<sup>9</sup> Finally, likely due to data availability, more than 80 percent of 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 national-scale wars ([Berman, Felter and Shapiro 2018](#)).

<sup>9</sup> A common measure is the Worldwide Governance Indicators variable “Political Stability and Absence of Violence/Terrorism,” which does not directly measure violence; instead, it captures “perceptions of the likelihood that the government will be destabilized or overthrown by unconstitutional or violent means, including politically-motivated violence and terrorism” ([Kaufmann, Kraay and Mastruzzi 2011, 4](#)).

## 2. 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 has uncovered two important sources of firm-level heterogeneity. Firms with higher exit costs react less to conflict. [Osgood and Simonelli \(2019\)](#), for example, find that U.S. multinational corporations (MNCs) with immobile assets are less responsive to terrorist violence. Relatedly, [Barry \(2018, 283\)](#) finds that conflict deters new ventures, but that established firms attempt to weather low-level conflict: “MNCs seem reluctant to quit a foreign venture until conditions become extreme.” Others argue that firms vary in the vulnerability to conflict, finding that political connections ([Fisman 2001](#)) and diversification ([Witte et al. 2016](#); [Dai, Eden and Beamish 2017](#)) moderate firms' exposure to instability and conflict.<sup>10</sup>

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 weaken property rights. Third, conflict may increase uncertainty around the government's domestic or international policy agendas. Finally, conflict may risk damaging a firm's reputation for working in a conflict-affected state. These mechanisms 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 amid conflict or 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 to its projects and their relative magnitudes.

We argue that these mechanisms operate at different geographic scales.<sup>11</sup> We delineate three concentric scales: (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

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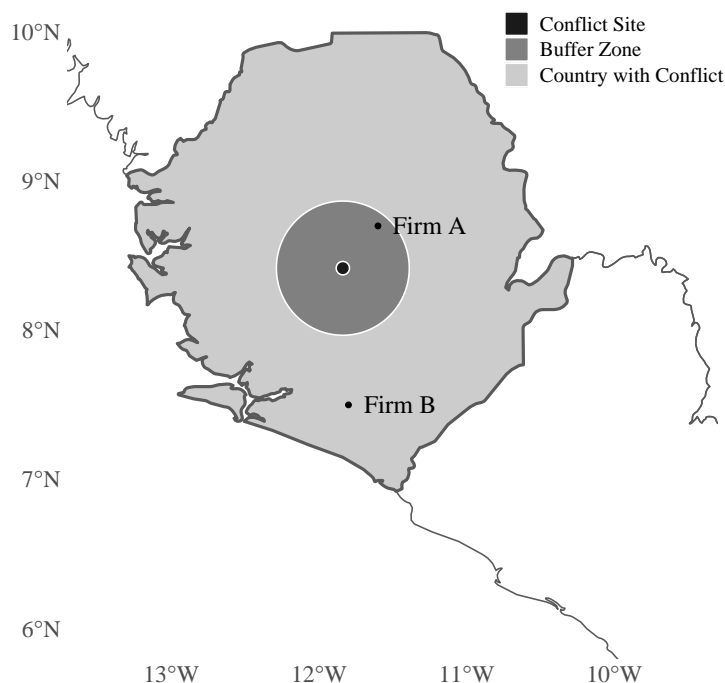
<sup>10</sup> Notably, [Witte et al. \(2016\)](#) do not find that armed conflict affects FDI among firms in resource-related sectors; their confidence intervals permit both sizable positive or negative effects on FDI by resource-related firms.

<sup>11</sup> While they do not enumerate the same mechanisms or geographic scales, [Dai, Eden and Beamish \(2013, 557\)](#) show that proximity to armed conflict affects the survival of Japanese firms' subsidiaries.



(3) the *country with conflict*. Figure 1 illustrates these three levels of exposure for a hypothetical conflict in the West African country of Sierra Leone. To demarcate the conflict site and buffer zone, we use circular buffers that emanate from where fighting takes place.<sup>12</sup>

**Figure 1:** Geographic Scales at which Conflict Affects Firm Activity



**Figure 1** uses a hypothetical conflict in Sierra Leone to define three concentric areas around conflict events: (1) a conflict site (black); (2) a buffer zone (dark grey); and (3) a country with conflict (light grey). Two mining projects are depicted to illustrate their exposure to conflict. Firm A's project is in the buffer zone and Firm B's project is operating in the country with conflict, but outside the buffer zone. In Section 3.3, we precisely define the distances used to construct these areas.

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 ([Deininger 2003](#)).

<sup>12</sup> This is a stylized example; in our empirical analysis, we vary the radii of these buffers, permitting finer demarcations.



Armed civil conflict, almost by definition, implies that the state has lost its strict monopoly on violence in some part of its territory. Beyond the specific sites of battles, the buffer zones surrounding conflicts are often considered ungoverned or no-go areas, where the legitimacy or capacity of the central state are contested. In Myanmar, for example, a recent report from the Asia Foundation classifies 118 of its 330 townships as contested, hosting active or latent conflict and harboring ethnic armed organizations that challenge the central government's authority (Burke et al. 2017, 2). Two important roles of the state are to tax firms and to provide oversight of labor, safety, and environmental standards. Guidolin and La Ferrara (2007, 1986) suggest that the state's diminished capacity to perform these functions could be a boon for investors: limited government capacity means reduced taxation and rent extraction. They relay reports from Angolan miners working in the "twilight zone between UNITA [rebel] and government control" where they could, for example, evade license fees.<sup>13</sup> Le Billon (2008, 1) outlines the challenge facing governments attempting to oversee buffer zones (see also 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 [from natural resources], especially when negotiating with better informed companies."

Recent empirical work supports this claim, finding that internal conflict depresses states' fiscal capacity (e.g., Thies 2010; Chowdhury and Murshed 2016). Besley and Persson (2008, 528), for example, find that countries facing internal conflict have a tax to GDP ratio that is seven percent lower than non-conflict countries. While these studies emphasize taxation, conflict hampers state and non-state (e.g., civil society-led) efforts to enforce other standards that raise operating costs.

The opposite could also be true: firms operating in buffer zones may fear expropriation and, thus, decrease their investment. Nearby conflict could diminish the state's ability to protect firms against predation by armed groups. Alternatively, state security forces deployed to the area in response to the conflict could extort firms.<sup>14</sup> In either case, firms' fears of expropriation could increase, and we would expect investment to fall in buffer zones.

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 to the southwest. And yet, much of the work on political risk implies that these firms may still be adversely

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<sup>13</sup> As suggested by this quote, we also assume that rebel groups' fiscal capacity is diminished in a buffer zone.

<sup>14</sup> The state may also be less able to provide public services, such as water, electricity, and sewage in buffer zones, raising operating costs for some firms. Industries, like mining, that operate as enclaves may be better equipped to function without such public services (see Le Billon 2001).

impacted, as armed conflict could cause 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 the violence in northern Myanmar, to continue our example, affects the government’s domestic or international standing or generates other policy uncertainty, this unpredictability could deter investment. At one extreme, would-be investors may worry about regime change ([Bates 2001](#)) or the expropriation of assets or income flows ([Jensen 2003](#)) provoked by the fiscal demands of conflict.<sup>15</sup> Short of government turnover or expropriation, investors may fear changes related to license fees, the terms of joint ventures with the state, foreign currency restrictions or currency 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 non-governmental organizations (NGOs), endangering the most priceless of assets, their good name” (qtd. 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 scales 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.

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<sup>15</sup> 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	Buffer Zone	Country with Conflict
<b>Disrupted Production</b> Fighting disrupts operations.	—	✓		
<b>State Capacity</b> Tax and regulatory capacity declines in disputed territory.	+	✓	✓	
Ability to protect property rights declines in disputed territory.	—	✓	✓	
<b>Policy Change</b> Conflict increases uncertainty around the standing or actions of government.	—	✓	✓	✓
<b>Reputation</b> Conflict creates risk of reputation loss from investors, home governments, media, or NGOs.	—	✓	✓	✓

### 3. Data

We take advantage of 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 economically important sector, particularly in developing, conflict-prone countries. Nearly 40 percent of greenfield FDI in low- and lower-middle-income countries between 2003 and 2015 went into extractives projects ([fDi Markets 2019](#)). Over 50 countries globally depended on natural resources for more than 20 percent of exports or 10 percent of GDP between 1995 and 2015 ([Davy and Tang-Lee 2018](#), 2). The scale of the sector has attracted academic attention. Canonical work on the political economy of foreign investment focuses on the mining sector (e.g., [Vernon 1971](#); [Moran 1974](#)). And conflict has been an important outcome for scholars studying the consequences of extractive industries, which not only includes mining but also oil and gas (for a recent review, see [Ross 2015](#)).

Without comparable firm-level data from other sectors, we are unable to assess whether our estimates generalize to other industries. Past work, however, suggests that mining investments may

be less vulnerable to violence. First, mining investments are tied to fixed geologic features and, thus, not easily relocated. In response to conflict, mining firms — unlike producers of annual cash crops or manufacturers — cannot easily relocate to protect their assets (e.g., [Bates and Lien 1985](#); [Boix 2003](#)).<sup>16</sup> 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 — often because of shortcomings in national power systems in the region” ([Ghosh Banerjee et al. 2014](#)). The immovability of mining investments and firms’ endogenous expenditure on private precautions likely dampen the effect of conflict on mining investments relative to other sectors.

### 3.1 Investment Data

Our outcome is mining firms’ exploration investment, based on data from SNL Metals and Mining.<sup>17</sup> SNL Metals and Mining obtains the information 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.<sup>18</sup> This investment is not exclusively FDI, as it includes investments by domestically owned firms; nonetheless, Figure A.1 shows that aggregate exploration investment and net FDI inflows are positively correlated.

To understand the expenses that firms include under exploration investment, we randomly sampled 80 firm-year observations where conflict occurred within 30 kilometers of a firm’s mining operation. All of the available annual reports (62) discuss exploration spending in detail, listing costs related to drilling, surveying, assaying, scoping, and pre-feasibility and feasibility studies. We also 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 expend more on security, but this is not captured by our outcome variable.

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<sup>16</sup> This also limits spillovers that result from the rapid reallocation of investment across space.

<sup>17</sup> We deflate investment to real USD in 1997. Exploration covers pre-production phases related to mineral discovery and assessing the feasibility of production.

<sup>18</sup> Expenditure on iron ore exploration was added in 2011. Fuel minerals, such as coal, oil, and natural gas, are not included.

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 dataset until it invests in at least one country. The data excludes small investments, totaling less than 100,000 USD; nonetheless, SNL estimates that this covers 95 percent of commercially-oriented nonferrous exploration expenditure.<sup>19</sup> Table A.2 provides additional detail on the regions and commodities that comprise our data.

Figure A.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 A.3(a) by plotting the effective number of countries in which firms invest.<sup>20</sup>

This low level of diversification highlights that the largest, commonly known mining companies (e.g., BHP, Rio Tinto) do not represent the vast majority of firms. Indeed, globally there are only 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.

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 (Marshall 2001; Dougherty 2013).

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<sup>19</sup> Mining investments less than this amount are unusual given the high costs of equipment and specialized personnel.

<sup>20</sup> The average country-year includes over twenty different firms making investments. Figure A.3(b) plots the distribution of the number of firms by country.

## 3.2 Armed Conflict Data

To code our independent variable, we use the Uppsala Conflict Data Program's Georeferenced Event Dataset (UCDP GED).<sup>21</sup> 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). When conducting analyses at the firm-country-year level, we only retain those conflicts that can be geo-coded to an exact location or nearby place-name (see Figure A.5 for a mapping of all such events; Table A.3 summarizes the severity of conflict across continent and sub-region). 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) non-state events: an organized actor uses armed force against another organized actor, neither of which is the government.

## 3.3 Measuring Exposure to Armed Conflict

SNL provides data on the locations of commercial (non-fuel) mining projects (see Figure A.4).<sup>22</sup> We know the owners of each project (and their respective shares) and use this information to link projects to the firms making exploration investments.<sup>23</sup>

By mapping both mining projects and armed conflicts, we can determine whether a conflict occurred within a certain distance (e.g., 10 kilometers) of a project (partially) owned by a specific firm. For example, we know that Randgold Resources Limited operated a mining project in the Democratic Republic of the Congo that fell within 10 kilometers of an armed conflict in 1997. Rather than choosing a single distance cutoff for exposure, we use multiple bandwidths — buffers around mining projects of varying radii (see Figure A.6). For every firm-country-year, we count the number of conflicts that occur within a given bandwidth across all of their projects. By construction, a firm can only be directly exposed to conflict if it already operates a project in the country where violence takes place.

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<sup>21</sup> 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.

<sup>22</sup> A subset of this data is used in Berman et al. (2017) and Christensen (2019).

<sup>23</sup> 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 activity has started.

Past empirical research overwhelmingly relies on aggregate data and, thus, never confronts the question of how to measure firms' exposure to conflict. We follow Dai, Eden and Beamish (2013; 2017) in using spatial proximity to violence to measure conflict exposure. We acknowledge, however, that exposure could be measured many ways given additional data (e.g., disruption along critical 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 A.4).<sup>24</sup> These 914 firm-country-year observations represent 3.31 billion USD of exploration investment.

## 4. 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 scales of exposure: (1) the effect for firms with operations at a conflict site ( $\tau^{\text{site}}$ ); (2) the effect for firms with operations in the buffer zone ( $\tau^{\text{buffer}}$ ); and (3) the effect for firms with operations within a conflict-affected country, but outside the buffer zone ( $\tau^{\text{country}}$ ).

If a firm has a project at a conflict site, that project is also within a buffer zone and in a conflict-affected country. For theory-testing, we assume a simple structural model that allows us to decompose the total effects that we estimate. This model enables us to isolate how much a firm's response is due to having a project at the conflict site, above and beyond the effects of having a project in the buffer zone and within a conflict-affected country. Specifically, we assume:

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

where  $\zeta$  parameterizes the effect attributable to operating at a conflict site;  $\eta$ , to operating in buffer zones; and  $\theta$  to operating in a conflict-affected country.<sup>25</sup> With three equations and three unknowns ( $\zeta, \eta, \theta$ ), we use our estimates to recover these parameters (e.g.,  $\hat{\zeta} = \hat{\tau}^{\text{site}} - \hat{\tau}^{\text{buffer}}$ ).

<sup>24</sup> Our main independent variable codes whether, for a given cross-sectional unit (the country or firm-country, depending on the level of analysis), conflict occurred in a given year ( $t$ ) or in the year prior ( $t - 1$ ).

<sup>25</sup> We log our dependent variable, so the additivity assumption is a claim about the percentage change differing, and not a claim about the absolute levels.



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  $\tau^{\text{site}}$ ,  $\tau^{\text{buffer}}$ , and  $\tau^{\text{country}}$ , with weights equal to the number of firms at each scale of exposure in a country-year:

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

This equation highlights the danger associated with inferring firms' behavior from changes in aggregate investment. If  $\tau^{\text{site}}$  is negative but  $\tau^{\text{buffer}}$  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 scales. 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 scales 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 experienced the same trends in investment absent any exposure to conflict.<sup>26</sup> We implement this basic strategy using a two-way fixed effects (firm-country and year) in an OLS regression. 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). Year fixed effects rule out time-varying factors that affect all firms in all countries (e.g., world commodity prices).

Our preferred specification saturates this basic model to rule out other sources of confounding. First, we replace the year fixed effects with firm-year fixed effects, which address time-varying, firm-specific factors (e.g., changes in management) that could affect investment across the countries in a firm's portfolio. Of course, this strategy continues to rule out time-varying global shocks. We estimate:

$$y_{ict} = \alpha_{ic} + \delta_{it} + \theta C_{ct} + \sum_{k=1}^K \kappa^k D_{ict}^k + \nu_{ict} \quad (2)$$

where  $i \in \{1, 2, \dots, 4331\}$  indexes firms,  $c \in \{1, 2, \dots, 177\}$  indexes countries;  $t \in \{1, 2, \dots, 18\}$ , year.  $y_{ict}$  is exploration investment (logged).  $C_{ct}$  is an indicator for whether an armed conflict

<sup>26</sup> We also invoke a stable unit treatment-value assumption (SUTVA) as well as a no-carryover assumption, which together imply that outcomes are unaffected by the treatment statuses of other units from any time period as well as itself from a past time period.

occurred in country  $c$  in year  $t$  or in the previous year  $t - 1$ .  $D_{ict}^k$ , 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.  $\nu_{ict}$  is a firm-country-year-specific error term. We cluster our standard errors at the firm-year level.<sup>27</sup>

In a second specification, we omit  $C_{ct}$  and include a third set of fixed effects, at the country-year level, which account for any time-varying factors affecting conflict and investment at the country-level (e.g., regime change):

$$y_{ict} = \alpha_{ic} + \delta_{it} + \gamma_{ct} + \sum_k \kappa^k D_{ict}^k + \nu_{ict} \quad (3)$$

This represents a generalized triple-difference design. Our results are consistent across these different sets of fixed effects. We present results from Equations 2 and 3 below and include results from the more basic model in the appendix (see Table A.5).

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,  $\Delta_t$  represents the year fixed effects, and our notation is otherwise unchanged from Equation 2. We cluster our standard errors on country.

## 5. Results

### 5.1 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 five kilometers 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 kilometers).

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<sup>27</sup> Our conclusions are unchanged if we cluster at the firm-country level.

In the first two models of Table 3 we first 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 replicate the first two models but include country-year fixed effects per Equation 3. These additional fixed effects 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 kilometers). 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 percent 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 experiencing this scale of conflict exposure (see Table A.4).

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 between 5 and 60 kilometers of the fighting. Our estimates initially increase in magnitude as we move further from the conflict, peaking at 30–40 kilometers before attenuating. 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 percent of the mean in countries where their operations fall 5–60 kilometers from armed conflict.<sup>28</sup>

Finally, firms cut their investment very modestly in countries where they operate further from violence (i.e., beyond 60 kilometers). For any single firm-country, this negative effect is small. The vast majority of firms exposed to conflict only see fighting from this distance. Per Equation 1, this small negative effect weighs heavily when estimating the effect of conflict on aggregate investment at the country-level.

We bolster the identifying parallel-trends assumption by showing that there is no evidence that investment trends diverge prior to conflict. Figure A.8 plots the coefficients on the leads and lags

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<sup>28</sup> Our results are not driven by high-intensity civil wars, operationalized here as country-years where armed conflicts generate over 1,000 battle deaths (see Table A.6 and Figure A.7). Firm responses to conflict exposure are similar in high- and low-intensity conflicts.

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

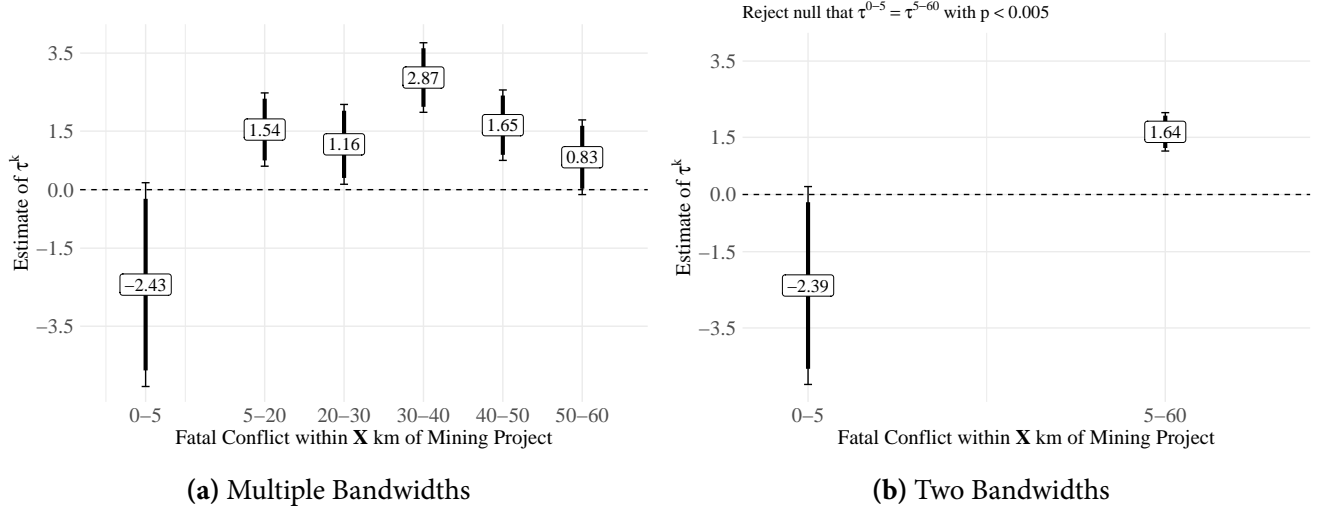
	<i>Dependent variable:</i>						
	Log(Exploration Investment + 1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0-5 km	-2.48*	-2.43*	-2.43*	-2.39*	-3.54*	-3.82	
	(1.33)	(1.32)	(1.33)	(1.32)	(1.89)	(2.41)	
5-20 km	1.52***		1.54***		1.73***	2.12***	1.36**
	(0.48)		(0.48)		(0.51)	(0.70)	(0.55)
20-30 km	1.15**		1.16**		1.96***	1.01	1.07*
	(0.52)		(0.52)		(0.64)	(0.71)	(0.59)
30-40 km	2.87***		2.87***		2.80***	2.59***	2.68***
	(0.46)		(0.45)		(0.53)	(0.72)	(0.52)
40-50 km	1.64***		1.65***		1.25**	1.10	1.65***
	(0.46)		(0.46)		(0.57)	(0.74)	(0.51)
50-60 km	0.83*		0.83*		0.67	1.31	1.19**
	(0.49)		(0.49)		(0.59)	(0.86)	(0.54)
5-60 km		1.63***		1.64***			
		(0.26)		(0.26)			
Beyond 60 km	-0.002*	-0.002*					
	(0.001)	(0.001)					
Firm Sample	All	All	All	All	Single country	Single project	All
Country-Year 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

**Table 3** reports results from OLS models estimated using Equation 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 exploration investment (logged plus one). The independent variable in models (1) and (3) codes whether a fatal conflict occurred in a given year (t) or in the year prior (t-1) between 0–5 km, 5–20 km, 20–30 km, 30–40 km, 40–50 km, or 50–60 km from a mining project (see Figure A.6). In models (2) and (4), we employ only two bandwidths: 0–5 km or 5–60 km. Models 3–7 include country-year fixed effects, which absorbs the “Beyond 60 km” term. In models (5) and (6) we subset to firms that invest in only a single country (5) or only a single project (6). In model (7), we subset to countries that have no projects at the conflict site. Significance: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

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  $\zeta$ , of operating in a buffer zone as  $\eta$ , and being in a country with conflict as  $\theta$ . Using our findings from model (2), we present estimates

**Figure 2:** Effect of Armed Conflict on Investment at the Firm-Country Level



**Figure 2** displays coefficient estimates and 95% confidence intervals based on the estimates of Equation 3 in Table 3 model (3) in panel (a) and model (4) in panel (b).

for these three parameters in Table 4.<sup>29</sup> First, operating at the site of battles dramatically reduces investment ( $\hat{\zeta} = -4.06$ ). Second, operating in a buffer zone encourages investment by mining 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:** Parameter Estimates

	Parameter	Estimate	Std. Error	2.5 %	97.5 %
Conflict site	$\hat{\zeta}$	-4.060	1.313	-6.633	-1.488
Buffer zone	$\hat{\eta}$	1.636	0.257	1.132	2.140
Conflict-affected country	$\hat{\theta}$	-0.002	0.001	-0.004	0.000

**Table 4** computes estimates based on results from Table 3, model (2) and computes standard errors using the delta method.

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

<sup>29</sup> We use the following mapping:  $\hat{\tau}^{\text{site}} = \hat{\kappa}^{[0-5]}$ ;  $\hat{\tau}^{\text{buffer}} = \hat{\kappa}^{[5-60]}$ ;  $\hat{\tau}^{\text{country}} = \hat{\theta}$ .

zones — firms may both enjoy weakened oversight and lament weakened property rights — the investment-encouraging mechanisms appear to dominate.

We might worry that firms reallocate from conflictual to more peaceful environments and that this response amplifies our estimates. Our context helps to 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 time-consuming steps: (1) local incorporation, which may take one to three months; (2) exploration license application writing and review process, 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 (Haldar 2018).

We use sub-group analysis to empirically assess the plausibility of such reallocation. We expect firms invested in multiple countries to be better able to reallocate exploration resources in response to conflict.<sup>30</sup> 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 Figure A.9).

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), to address the possibility of reallocation, we restrict the sample to those firms with a single project and reestimate Equation 3. Our inferences are again unchanged (see Figure A.10).

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 rare (Table A.4). 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 their investment in countries where they have operations within the buffer zone surrounding conflict.

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<sup>30</sup> 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 Figure A.3).

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, 8). 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 A.8 and Table A.7 demonstrate that, in fact, the positive effects of exposure to conflict in the buffer zone persist for several years, ruling out such temporal displacement.

## 5.2 Effect on Investment at Country Level

Our results at the country level, which provide a test of Hypothesis 4, are consistent with a majority of the existing literature: the incidence of fatal armed conflict reduces exploration investment. Table 5 reports consistent estimates from Equation 4 using both 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 eight percent of the mean ( $\bar{y}_{ct} = 9.98$ ). Model (2) drops countries with no investment (32 countries). Across both models, 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 A.8).

These country-level results are consistent with the structural estimates 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.  $\eta$  and  $\zeta$  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



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

	<i>Dependent variable:</i>						
	Log(Exploration Investment + 1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1(Conflicts > 0)	−0.77** (0.35)	−0.69* (0.37)					
1(Conflicts = 1)			−0.53 (0.40)				
1(Conflicts > 1)			−0.86** (0.39)				
1(State-Based > 0)				−0.28 (0.41)			−0.02 (0.41)
1(One-Sided > 0)					−0.94*** (0.32)		−0.90*** (0.33)
1(Non-State > 0)						−0.69 (0.47)	−0.61 (0.47)
F-stat	5.03	3.55	2.54	0.47	8.45	2.18	3.36
p-value	0.02	0.06	0.08	0.49	0.00	0.14	0.02
Sample	Full	Recipients	Full	Full	Full	Full	Full
Country FE	177	145	177	177	177	177	177
Year FE	18	18	18	18	18	18	18
Observations	3,186	2,610	3,186	3,186	3,186	3,186	3,186

**Table 5** reports the results from OLS models estimated using Equation 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 Equation 4 using different samples or measures of conflict. Significance: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

(4) to (7) look at whether different types of fatal armed conflict — state-based, one-sided, or non-state — have differential effects on exploration investment. Focusing attention on model (7), we find that one-sided and non-state conflicts have larger, negative effects.<sup>31</sup>

Our empirical strategy assumes parallel trends in investment (logged) among countries that are and are not affected by fatal armed conflict. While untestable, we bolster this assumption by showing that investment does not change in anticipation of conflict. Figure A.11 plots the coefficients on the leads and lags of the indicator for a fatal armed conflict (see also Table A.9). 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 fall off in investment in the years

<sup>31</sup> 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 non-state conflicts may be less predictable and involve greater uncertainty about the extent of collateral damage.

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).

## 6. Mechanisms

We incorporate auxiliary data to explore the mechanisms outlined in Section 2: that conflict disrupts production at proximate mining operations, undermines state capacity, and creates policy uncertainty or reputation risk. We regard these as secondary, and typically more speculative, analyses given data and design limitations that we note below.

### 6.1 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, none of which is good for business. Ksoll, Macchiavello and Morjaria (2016, 3) find, for example, that flower exporters in regions affected by Kenya’s post-election violence saw their exports fall by 38 percent. At the height of the violence, half of their employees were not showing up for work. Looney (2006, 995) argues that conflict and insurgency in Iraq led to the downsizing or closing of firms in the formal sector. 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 five kilometers) 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 twenty percentage points; the quantity produced (logged) falls by about twenty percent of the mean (see Table A.10). The latter, while sizable, is not significant.<sup>32</sup> For projects in a buffer zone but outside of a conflict site (5–60 kilometers from a recent conflict), we find small and insignificant negative effects on the likelihood and intensity of production. The effect of being within 5 to 60 kilometers of a conflict is an order of magnitude

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<sup>32</sup> 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 (Figure A.12) indicate that production for mines at conflict sites continues to decline three years after violence takes place.

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 of results is consistent with our earlier findings on investment: operating at a conflict site can hamper production and, as a consequence, limit companies' capacity or willingness to invest. Yet, these dampening effects are not apparent in the broader buffer zone that surrounds these conflict sites.

## 6.2 State Capacity

Conflict could be a boon for mining 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 systematically. 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 that recently experienced a fatal 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).<sup>33</sup> We also compile data from the World Mineral Statistics on annual production for roughly 100 minerals for nearly every country.<sup>34</sup> 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 A.13 (see also Table A.11), the production elasticity of resource tax revenues is lower in countries affected by one-sided or state-based conflicts in the current or previous

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<sup>33</sup> ICTD's [Government Revenue Dataset \(GRD\)](#) combines information from six cross-country sources, including the IMF, the World Bank, the OECD and CEPAL, to create a comprehensive, standardized dataset of government revenue from taxation. We focus on countries that report any revenues from natural resources in 1997, at the start of our study period.

<sup>34</sup> The WMS extends back to 1913 and 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](#)).

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.<sup>35</sup> We find no significant effect of non-state conflicts, which do not involve the government. These results suggest that armed conflict involving the state may undermine fiscal capacity.

While we cannot specify where the state’s fiscal capacity erodes, our findings align with case studies of mining companies profiting from operations in ungoverned areas (e.g., [Reno 1999](#); [Vanden Eynde 2015](#)). They could also help to explain why we see greater exploration investment among companies operating projects in the buffer zones that surround recent fatal armed conflicts: the companies in these grey zones suffer minor production disruptions while benefitting from less oversight.

### 6.3 Policy Change

We look at whether conflict raises concerns about changes in policy due to, for example, government turnover. 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 dataset used by firms that contains measures of multiple components of political risk.<sup>36</sup> We find that the armed conflicts we use in our analyses raise concerns that political violence will impact the country’s governance (see Table A.12). In the year of or immediately following the incidence of a fatal armed conflict, ICRG’s Internal Conflict Index falls a half point on a 12-point scale (50 percent of the average within-country standard deviation for this index). Larger values of ICRG’s indices correspond to better outcomes; thus, a lower score on the Internal Conflict Index corresponds to more concern that political violence impacts governance. While the UCDP data includes small skirmishes and battles, these events shape country-level assessments of internal violence.

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](#)).<sup>37</sup> This measure operationalizes two concerns raised in arguments 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

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<sup>35</sup> The direct effects of conflict on taxation are included in all models in Table A.11 but are omitted from the table.

<sup>36</sup> ICRG’s Internal Conflict Index is an “assessment of political violence in the country and its actual or potential impact on governance” and consists of three items: “civil war/coup threat,” “terrorism/political violence,” and “civil disorder” ([PRS Group 2012](#)).

<sup>37</sup> The index is a composite of three items, measuring “government unity,” “legislative strength,” and “popular support.”

find that the incidence of fatal armed conflict decreases assessments of government stability (model 4 of Table A.12): a reduction of 0.2 is roughly 15 percent 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 non-state 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 reputation risk.<sup>38</sup> 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 A.13 are comparable in magnitude to those reported for the full sample. Conflict does deter investment by potential new entrants to a country.

## 7. 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 February 2016](#)).

Yet, the past research supporting this claim relies overwhelmingly on cross-national analysis, which masks the differential effects that conflict has on firms operating (or considering new operations) in the same 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 investments, we show that indeed effects depend on the conflict exposure of firms. We show that mining firms pull back investments at the sites of violence and that the disruption of mineral production may explain why. However, in the buffer zone surrounding the fighting — where neither the state nor its armed challengers fully control the 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

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<sup>38</sup> The effect of policy uncertainty or reputation risk on investment may be different for existing and new investors. We only estimate the latter in this exercise.

term. Indeed, 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 prefer war in order to profit from the local state weakness it creates. 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 ([Callen et al. 2019](#)). Our findings suggests new directions for research into how heterogeneous exposure to conflict — and the ungoverned grey zones created by conflict — affect conflict termination, postwar economic growth, and the distributional consequences of war.

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