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

Graeme Blair†
Darin Christensen‡ and
Valerie Wirtschafter§

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Abstract

How does conflict affect firms' investment decisions? Past results are mixed: a third of studies we reviewed report null or mixed correlations; some suggest 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 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 at a remove 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 conflict depresses aggregate investment, but this reflects responses among firms far from fighting.

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†Assistant Professor of Political Science, UCLA, graeme.blair@ucla.edu, https://graemeblair.com

‡Assistant Professor of Public Policy and Political Science, UCLA, darinc@luskin.ucla.edu, https://darinchristensen.com

§Ph.D. candidate in political science, UCLA, vwirtschafter@ucla.edu.

When firms and individuals fear that future economic returns will be destroyed or expropriated, they have little incentive to invest. This foundational tenet 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).[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 — 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 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 reputation risk impacts all firms operating in a country with conflict. Conflict should not, thus, have a uniform effect on firms' investment decisions: a firm's proximity to violence shapes how it responds (see Figure 1 for an illustration). And 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]

[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., Kobrin 1978; Collier and Duponchel 2013).

[3] Our focus here is on the determinants of investment; we take no stand on whether such investment is welfare-enhancing.

[4] Jamison (2019) and Lee (2017) report heterogeneous effects of conflict on investment depending, respectively, on if a sector enjoys a natural monopoly and the host states' anti-terrorism capacity.

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 percent) 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.[5]

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 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) actually 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 and show that armed conflict depresses aggregate 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 five kilometers of the violence). The likelihood that a mine

[5] See Barry (2016) on the opportunities presented by firm-level data.

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 (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.

Seminal work in international political economy 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; Jamison 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.

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 (for a review, see Kim and Osgood 2019) and, more recently, foreign investment (Barry 2016; Zhu and Shi 2019; Doctor and Bagwell 2020), which use 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 data (Bennett and Green 1972; Green and Cunningham 1975). To assess the weight of this evidence, we conduct a formal systematic review.[6] 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 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 Figure A.15).[7]

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).[8] 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 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.

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[6] We follow the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines (see Appendix H).

[7] 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.

[8] 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.

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
All studies	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. 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 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 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 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 or price cycles that happen to coincide with changes in the frequency of armed conflict. 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).[9] Finally, likely due to data availability, more than 80 percent 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, Felter and Shapiro 2018).

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:

[9] 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).

> "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 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. Others argue that political connections (Fisman 2001) and diversification (Witte et al. 2016; Dai, Eden and Beamish 2017) moderate firms' exposure to instability and conflict.[10]

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 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 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 apply to different areas around an armed conflict event.[11] 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

[10] 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.

[11] While they do not enumerate the same mechanisms or geographic extents, Dai, Eden and Beamish (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

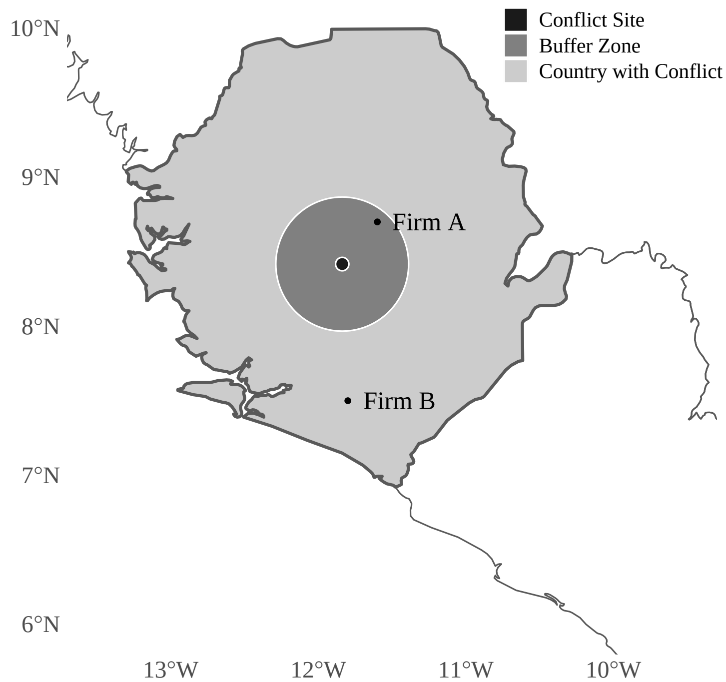


Figure 1 uses a hypothetical conflict in Sierra Leone to define three concentric areas around conflict events: (1) a conflict site (black); (2) a buffer zone (dark grey); and (3) a country with conflict (light grey). Two mining projects are depicted to illustrate their exposure to conflict. Firm A's 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.

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.[12]

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.

[12] 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 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 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*b*, 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 (see Smith and Rosenblum 2011).

Recent empirical work generalizes these arguments, finding that internal conflict depresses states' fiscal capacity across sectors (e.g., Thies 2010; Chowdhury and Murshed 2016).[13] 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. 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 non-state actors (e.g., protection rackets run by corrupt local officials or rebel groups) (Le Billon 2008*a*; Keen 1998; Collier 1999).[14] Moreover, if firms rely on infrastructure impacted by conflict (e.g.,

[13] While these studies emphasize taxation, conflict also hampers non-state (e.g., civil society, journalistic) efforts to enforce standards that can increase firms' costs.

[14] McDougal (2010) recounts managers' experiences of looting around Monrovia during the Liberian Civil War. For a detailed account of pillage by the Congolese armed forces in Eastern Congo, see Garrett, Sergiou and Vlassenroot (2009).

road networks that have been damaged or disrupted by road blocks), this could increase operating costs (Stewart 1993; Collier 1999; Mills and Fan 2006). 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, Felter and Shapiro 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 also 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, 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 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.[15] 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.

[15] 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.	−	✓	✓	✓

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.

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. 40 percent of greenfield FDI in low- and lower-middle-income countries between 2003 and 2015 went into extractives projects (fDi Markets 2019). The next largest sectors are real estate, communications, and financial services. 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. Influential 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 (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 response to conflict, mining firms — unlike manufacturers — cannot easily relocate to protect their assets (e.g., Bates and Lien 1985; Boix 2003).[16] 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" (Banerjee et al. 2014). 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 (2019), we find no evidence that armed conflict has differential effects on aggregate investment in the natural resource sectors (Figure A.11) or, specifically, metals and minerals (Table A.9) relative to other economic sectors (Appendix E describes these data and analyses).

3.1 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,

[16] This also limits spillovers that result from the rapid reallocation of investment across space.

gold, iron, platinum group metals, rare earths, silver, uranium, and others.[17] 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.

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.[18] 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.[19]

[17] Expenditure on iron ore exploration was added in 2011. Fuel minerals, such as coal, oil, and natural gas, are not included.

[18] Mining investments typically exceed this threshold given the high costs of specialized inputs.

[19] 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.

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 percent 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 (Marshall 2001; Dougherty 2013).

3.2 Armed Conflict Data

To code our independent variable, we use the Uppsala Conflict Data Program's Georeferenced Event Dataset (UCDP GED).[20] 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 geocoded 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).[21] 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

[20] 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.

[21] For the countries and years in our sample, just over 27 percent of events can only be geocoded to the second-order administrative district (e.g., counties in the US). As our analyses hinge on measures of proximity, we exclude such events. This does not distort our results. We drop all firm-country-years where a firm operated in a project in an ADM2 (and year) with an excluded event; our point estimates remain stable, and our inferences are unchanged (see Table A.5).

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).[22] We know the owners of each project (and their respective shares) and use this information to link projects to the firms making exploration investments.[23]

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.

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 dataset 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 Appendix 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 kilometers). Yet, even this shortest path distance is an understatement, as it does not account for road quality. (Moreover, for four percent 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

[22] A subset of this data is used in Berman et al. (2017) and Christensen (2019).

[23] 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.

average crow-flies distance. Travel costs dampen mines' exposure to nearby conflicts.[24] 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 A.4). 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 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.[25] With three equations and three unknowns (ζ, η, θ), we use our estimates to recover these parameters (e.g., $\widehat{\zeta} = \widehat{\tau}^{\text{site}} - \widehat{\tau}^{\text{buffer}}$).

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

[24] Firms operating in buffer zones may, of course, engage combatants, seeking reassurances that their projects will not become enmeshed in violence.

[25] We take the natural logarithm of our dependent variable, so the additivity assumption is a claim about the percentage change differing, and not a claim about the absolute levels.

predicting total cross-border flows. This effect ($\overline{\tau}$) 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:

$$\overline{\tau} = N^{\text{site}} \cdot \tau^{\text{site}} + N^{\text{buffer}} \cdot \tau^{\text{buffer}} + N^{\text{country}} \cdot \tau^{\text{country}} \tag{1}$$

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 experienced the same trends in investment absent any exposure to conflict.[26]

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

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

where $i \in \{1,2,\ldots,4331\}$ indexes firms, $c \in \{1,2,\ldots,177\}$ indexes countries; $t \in \{1,2,\ldots,18\}$, year. 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. ν_{ict} is a firm-country-year-specific error term. We cluster our standard errors at the firm-year level.

[26] We also invoke stable unit treatment-value and a no-carryover assumptions.

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):

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

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 A.6.

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:

$$Y_{ct} = A_c + \Delta_t + \beta C_{ct} + \varepsilon_{ct} \tag{4}$$

where Y_{ct} is aggregate investment (logged), A_c represents the country fixed effects, Δ_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).

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.

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

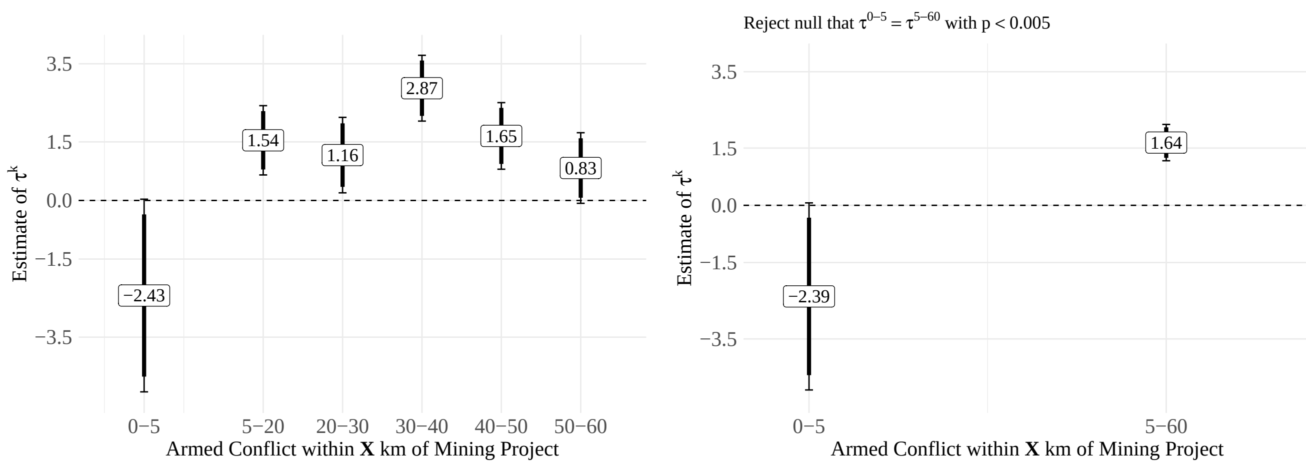
	Dependent variable:						
	Log(Exploration Investment + 1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0-5 km	-2.48^{**}	-2.43^{*}	-2.43^{*}	-2.39^{*}	-3.54^{*}	-3.82	
	(1.26)	(1.25)	(1.26)	(1.25)	(1.89)	(2.41)	
5-20 km	1.52^{***}		1.54^{***}		1.73^{***}	2.12^{***}	1.36^{***}
	(0.45)		(0.45)		(0.51)	(0.70)	(0.52)
20-30 km	1.15^{**}		1.16^{**}		1.96^{***}	1.01	1.07^{*}
	(0.49)		(0.49)		(0.64)	(0.71)	(0.56)
30-40 km	2.87^{***}		2.87^{***}		2.80^{***}	2.59^{***}	2.68^{***}
	(0.43)		(0.43)		(0.53)	(0.72)	(0.49)
40-50 km	1.64^{***}		1.65^{***}		1.25^{**}	1.10	1.65^{***}
	(0.44)		(0.43)		(0.57)	(0.74)	(0.48)
50-60 km	0.83^{*}		0.83^{*}		0.67	1.31	1.19^{**}
	(0.46)		(0.46)		(0.59)	(0.86)	(0.51)
5-60 km		1.63^{***}		1.64^{***}			
		(0.24)		(0.24)			
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: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

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 within this extent 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.[27]

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



(a) Multiple Bandwidths **(b)** Two Bandwidths

Figure 2 displays coefficients and 95% confidence intervals from Table 3 model 3 (a) and model 4 (b).

Finally, firms modestly reduce investment 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.10 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.

[27] 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.7 and Figure A.7). Firm responses to conflict exposure are similar in high- and low-intensity conflicts.

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.[28] First, operating at the site of battles dramatically reduces investment ($\widehat{\zeta} = -4.06$). Second, operating in a buffer zone encourages investment by mining firms ($\widehat{\eta} = 1.63$). Finally, operating in a country with conflict deters investment, though the effect is minimal if a firm is far from the fighting ($\widehat{\theta} = -0.002$). 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	$\widehat{\zeta}$	−4.060	1.313	−6.633	−1.488
Buffer zone	$\widehat{\eta}$	1.636	0.257	1.132	2.140
Conflict-affected country	$\widehat{\theta}$	−0.002	0.001	−0.004	0.000

Table 4 estimates based on Table 3, model (2); standard errors computed via 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 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, because mines are tied to geological features; and, as a consequence, firms may increase spending on security and other services that the government can no longer provide, mitigating harms that might make other firms halt investment.

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

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

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.[29] 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.8). 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.9).

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 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.

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

[29] 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).

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.

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

	Dependent variable:						
	Log(Exploration Investment + 1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\mathbb{1}$(Conflicts > 0)	-0.65^{**}	-0.56					
	(0.32)	(0.34)					
$\mathbb{1}$(Conflicts = 1)			-0.41				
			(0.37)				
$\mathbb{1}$(Conflicts > 1)			-0.74^{**}				
			(0.37)				
$\mathbb{1}$(State-Based > 0)				-0.17			0.07
				(0.39)			(0.39)
$\mathbb{1}$(One-Sided > 0)					-0.87^{***}		-0.85^{***}
					(0.31)		(0.32)
$\mathbb{1}$(Non-State > 0)						-0.69	-0.62
						(0.45)	(0.46)
F-stat	4.15	2.72	2.11	0.2	7.86	2.32	3.23
p-value	0.04	0.1	0.12	0.65	0.01	0.13	0.02
$\overline{y_{ct}}$	9.98	12.19	9.98	9.98	9.98	9.98	9.98
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

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.

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 ($\overline{y_{ct}} = 9.98$). 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.10).

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. 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) 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.[30]

In Appendix F.1, we report analyses that separate "major" and multinational mining firms — firms that, by virtue of their size and visibility, may be especially concerned about their reputations (see Table A.12). We do not find a significantly different response among these subsets of firms. However, while the interaction term is not significant, major mining firms may respond more aversely to state-based armed conflict, which could reflect greater concern about their brands being associated with repressive governments (as suggested in Henisz 2017). This analysis does not, of course, rule out reputational effects — these could act on 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 fatal armed conflict. While untestable, we bolster this assumption by showing that investment does not change in anticipation of conflict. Figure A.12 plots the coefficients on the leads and lags of the indicator for a fatal armed conflict (see also Table A.11). 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 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).

[30] 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.

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.13). The latter, while sizable, is not significant.[31] 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 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

[31] 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.13) indicate that production for mines at conflict sites continues to decline three years after violence takes place.

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).[32] We also compile data from the World Mineral Statistics on annual production for roughly 100 minerals for nearly every country.[33] 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.14 (see also Table A.14), 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.[34] 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.

[32] 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.

[33] 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).

[34] The direct effects of conflict on taxation are included in all models in Table A.14 but are omitted from the table.

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.[35] 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.15). 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). While the UCDP data includes 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).[36] 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 find that the incidence of fatal armed conflict decreases assessments of government stability (model 4 of Table A.15): 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

[35] 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).

[36] The index is a composite of three items, measuring "government unity," "legislative strength," and "popular support."

among these firms helps us isolate the aggregate country-level effect, which we attribute to increased uncertainty around policy changes or reputation risk.[37] 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.16 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 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 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

[37] 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.

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 (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.

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 or three times the weight of bureaucratic quality. 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 individual conflict events near mining projects involve relatively few fatalities. Moreover, firms can employ private security or contract with state security forces 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 (or dimensions of risk) affect investment behavior.

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Supporting Information

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

Following text to be published online.

Contents

A. Systematic Review

Table A.1: Studies in Systematic Review

Author(s) & Year	Effect	Region[*]	Years	Unit of Analysis	Unit FE?[§]	Time FE?[**]	IV?[‖]
Koechlin (1992)	−	G (23)	1966-1985	country-2 year			
Perotti (1994)	−	G (26)	1960-1985	country			
Alesina and Perotti (1996)	−	G (71)	1960-1985	country			
Resnick (2001)	−	G (19)	1971-1993	country-year			
Globerman and Shapiro (2002)	−	G (144)	1995-1997	country			
Stasavage (2002)	−	G (74)	1971-1994	country-year	✓		
Sun, Tong and Yu (2002)	−	China (1)	1986-1998	province-year	✓		
Abadie and Gardeazabal (2003)	−	Spain (1)	1998-2000	firm-trading sessions[†]			
Fielding (2003*a*)	−	N. Ireland (1)	1960-1995	sector-year[≀]	✓		
Fielding (2003*b*)	−	Israel (1)	1988-1998	firm			
Asiedu (2006)	−	SSA (21)	1984-2000	country-year	✓		
Aysan et al. (2007)	−	MENA (33)	1970-2002	country-year			✓
Busse and Hefeker (2007)	−	G (83)	1984-2003	country-4 year[‡]	✓		
Daude and Stein (2007)	−	G (152)	1982-2002	country-year	✓		
Gani (2007)	−	G (17)	1996-2002	country-year	✓		
Malefane (2007)	−	Lesotho (1)	1973-2004	year			
Naudé and Krugell (2007)	−	Africa (43)	1970-1990	country-5 year	✓	✓	
Alfaro et al. (2008)[⊥]	−	G (34)	1984-1997	country-year			✓
Suliman and Mollick (2009)	−	SSA (29)	1980-2003	country-year	✓		
Bussmann (2010)	−	G (154)	1980-2000	country-year	✓		✓
Ramasamy and Yeung (2010)	−	G (23)	1980-2003	country-year	✓		
Baek and Qian (2011)	−	G (116)	1984-2003	country-year	✓		
Daniele and Marani (2011)	−	Italy (1)	2002-2006	province-year			
Mengistu and Adhikary (2011)	−	Asia (15)	1996-2007	country-year	✓		
Oh and Oetzel (2011)	−	G (101)	2001-2006	firm-country-year		✓	
Morrissey et al. (2012)	−	G (46)	1996-2009	country-year			
Powers and Choi (2012)	−	G (123)	1980-2008	country-year	✓		
Ramasamy et al. (2012)	−	G (59)	2006-2008	country-year			
Solomon and Ruiz (2012)	−	G (28)	1985-2004	country-year	✓		
Al-Khouri and Khalik (2013)	−	MENA (16)	1984-2011	country-year	✓		
Bandyopadhyay et al. (2013)	−	G (78)	1984-2008	country-year	✓	✓	
Hayakawa et al. (2013)	−	G (89)	1985-2007	country-3 year	✓	✓	
Singh (2013)	−	India (1)	1981-1990	district-year	✓	✓	
Burger et al. (2015)[⊥]	−	MENA (17)	2003-2012	country-year; country-quarter	✓		
Ezeoha and Ugwu (2015)	−	Africa (41)	1997-2012	country-year			✓
Mijiyawa (2015)	−	Africa (53)	1970-2009	country-5 year	✓	✓	
Lee (2016)	−	G (50)	1980-2006	country-year	✓	✓	
Lee (2017)	−	G (114)	1987-2006	country-year	✓		
Brown and Hibbert (2017)	−	G (65)	1997-2012	country-year	✓		
Cabral et al. (2019)	−	Mexico (1)	2005-2015	state-quarter	✓		

Studies in Systematic Review (Continued)

Author(s) & Year	Effect	Region[*]	Years	Unit of Analysis	Unit FE?[§]	Time FE?[**]	IV?[‖]
Brunetti and Weder (1998)	−/∼	G (60)	1974-1989	country			
Tuman and Emmert (1999)	−/∼	LAC (12)	1979-1992	country-year			
Bohn and Deacon (2000)	−/∼	G (125)	1955-1988	country-year			
Enders et al. (2006)	−/∼	G (69)	1989-1999	country-quarter	✓		
Li, Murshed and Tanna (2017)[∓]	−/∼	G (128)	2003-2012	country-sector-year	✓		
Carter et al. (2018)	−/∼	G (40)	1980-2010	country-year		✓	
Tuman and Emmert (2004)	−/+	LAC (15)	1979-1996	country-year			
Li (2006)[⊥]	−/∼/+	G (129)	1976-1996	country-year			
Wheeler and Mody (1992)[⊥]	∼	G (42)	1982-1988	country-year		✓	
Liu et al. (1997)	∼	China (1)	1983-1994	country-year	✓		
Feng (2001)	∼	G (40)	1978-1988	country			
Asiedu (2002)	∼	G (71)	1988-1997	country			
Globerman and Shapiro (2003)	∼	G (143)	1995-1997	country-year			
Bevan and Estrin (2004)	∼	Europe (11)	1994-2000	dyad-year			
Le (2004)	∼	G (25)	1975-1995	country-year	✓		
Biglaiser and DeRouen Jr. (2007)[⊥]	∼	G (126)	1966-2002	country-year	✓		
Kinda (2010)	∼	G (77)	2000-2006	country-sector-year			
Li and Vashchilko (2010)	∼	G (58)	1980-2000	dyad-year	✓		
Cleeve (2012)	∼	SSA (40)	1988-2008	country-year	✓		
Jadhav (2012)	∼	BRICS (5)	2000-2009	country-year			
Kolstad and Wiig (2012)	∼	G (142)	2003-2006	country			
Vadlamannati (2012)	∼	G (101)	1997-2007	country-year		✓	✓
Ashby and Ramos (2013)	∼	Mexico (1)	2004-2010	country-state-sector-year		✓	
Sissani and Belkacem (2014)	∼	Algeria (1)	1990-2012	year			
Kariuki (2015)	∼	Africa (35)	1984-2010	country-year	✓	✓	
Okafor (2015)	∼	SSA (23)	1996-2010	country-year			
Kim (2016)	∼	G (95)	1980-2000	country-year			
Shahzad et al. (2016)	∼	Pakistan (1)	1988-2010	quarter			
Williams (2017)	∼	G (68)	1975-2005	country-5 year			
Campos and Nugent (2003)	+	G (94)	1960-1985	country-5 year			✓
Biglaiser and DeRouen Jr. (2006)[⊥]	+	LAC (15)	1980-1996	country-year			✓
Guidolin and La Ferrara (2007)	+	Angola (1)	1998-2002	firm-day	✓		
Asiedu and Lien (2011)[⊥]	+	G (86)	1982-2007	country-4 year	✓		✓
Jadhav and Katti (2012)	+	BRICS (5)	2001-2010	country-year	✓		
Aziz and Khalid (2017)	+	G (60)	1990-2013	country-year	✓		

[*] Regional acronyms include: G for global; LAC for Latin America & Caribbean; SSA for sub-Saharan Africa; MENA for Middle East and North Africa; and BRICS for Brazil, Russia, India, China and South Africa. The number of countries is included in parentheses.
− Finds a statistically significant negative relationship between conflict and investment.
∼ Finds no statistically significant relationship between conflict and investment.
+ Finds a statistically significant positive relationship between conflict and investment.
[§] Paper includes a unit fixed effects model as the main analysis.
[**] Paper includes a time fixed effects model as the main analysis.
[‖] Paper uses an instrumental variables model as the main analysis.
[†] "Trading sessions" represents total returns from stock market trading periods for Basque and Non-Basque firm portfolios.
[≀] "Sector-year": annual investment by sector in the sample period. In Fielding (2003*a*), sectors include: (1) food, drink and tobacco; (2) engineering; (3) transport equipment; (4) textiles; and (5) other. In Burger, Ianchovichina and Rijkers (2015), sectors include: (1) resources and energy; (2) non-resource manufacturing; (3) tradable services; and (4) non-tradables.
[‡] 4- or 5-year reflects temporal resolution of the panel.
[⊥] Found through independent search, not systematic review process.
[∓] Mixed findings come from separate models with DVs based on FDI in different sectors.

B. Descriptives

B.1 Exploration Investment and Mining Projects

Figure A.1: Relationship between Exploration Investment and Net FDI Inflows

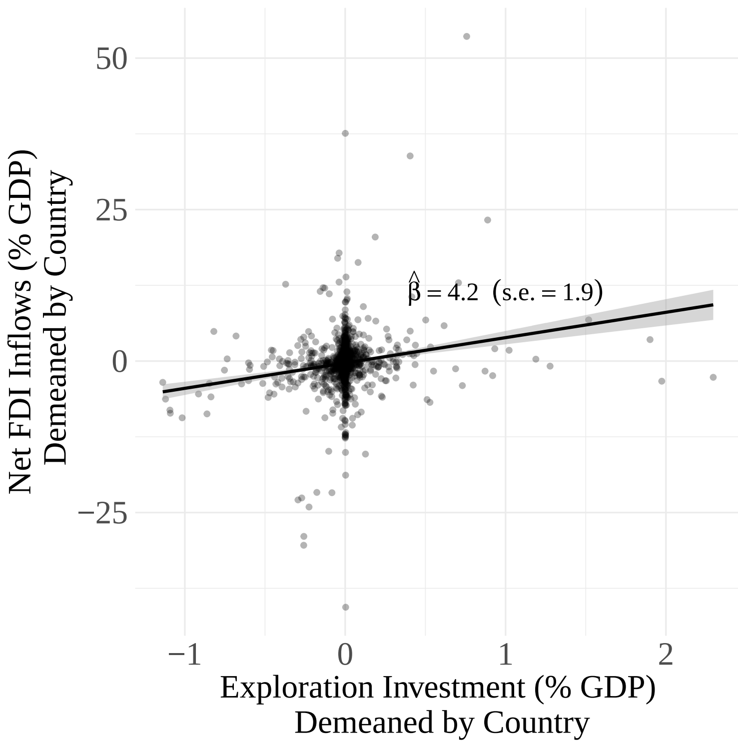


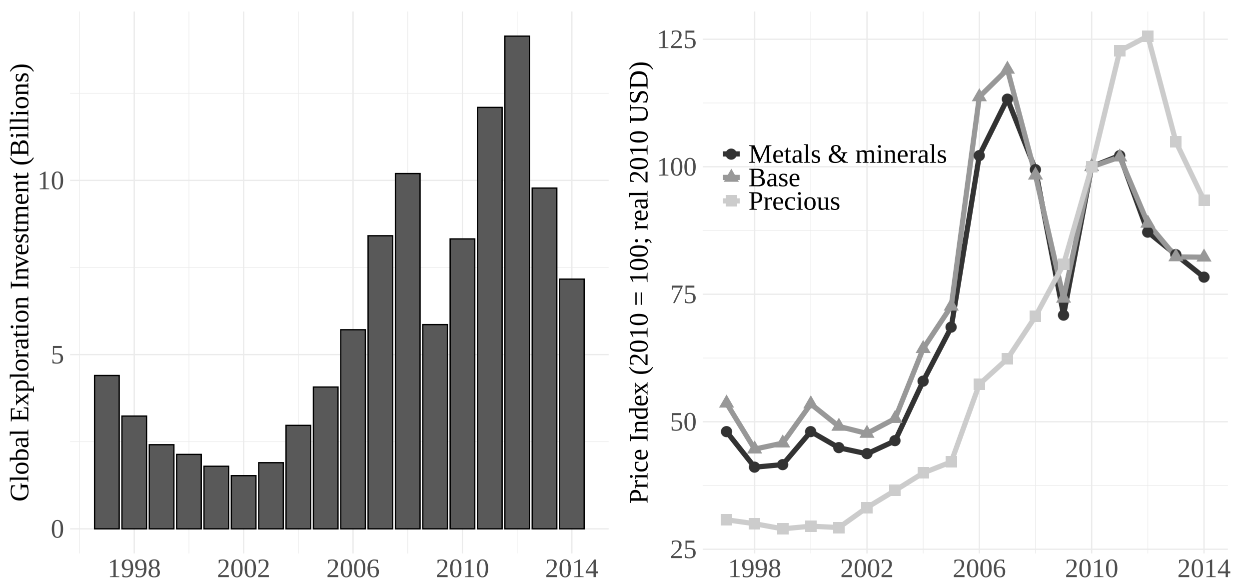
Figure A.1 displays the bivariate correlation between exploration investment and net FDI inflows. We normalize both exploration investment and net foreign direct investment (FDI) by GDP. We then demean both series (i.e., residualizing with country fixed effects) and plot the correlation. The OLS coefficient from regressing net FDI on exploration investment with country fixed effects is 5.28 and statistically significant, with standard errors clustered on country.

Table A.2: Mines and Investment by Region

Continent	Region	# Mines	Proportion of Mines Gold	Copper	Iron ore	Investment (USD)	Top countries by investment
Africa	Central	573	17	26	13	3.0B	D.R. Congo (62%); Angola (27%)
	East	1,145	41	18	3	3.3B	Tanzania (37%); Zambia (29%)
	North	188	35	11	7	0.3B	Morocco (39%); Egypt (30%)
	Southern	1,442	18	6	4	4.4B	South Africa (61%); Namibia (20%)
	West	1,505	71	1	8	5.3B	Ghana (25%); Burkina Faso (20%)
	Total	4,853	40	10	6	16.3B	South Africa (16%); D.R. Congo (11%)
Americas	Central	1,690	54	14	3	6.8B	Mexico (86%); Guatemala (4%)
	North	12,969	48	12	2	27.0B	Canada (68%); USA (31%)
	South	3,985	45	23	8	19.7B	Peru (27%); Chile (26%)
	Total	18,644	48	15	3	53.4B	Canada (34%); USA (16%)
Asia	Central	613	47	13	3	1.5B	Kazakhstan (57%); Kyrgyzstan (22%)
	East	2,643	43	16	12	4.8B	China (68%); Mongolia (29%)
	South	459	9	17	35	0.7B	India (52%); Iran (23%)
	Southeast	1,129	47	17	8	4.1B	Indonesia (54%); Philippines (31%)
	Western	410	49	25	6	1.0B	Turkey (56%); Saudi Arabia (22%)
	Total	5,254	42	17	12	12.2B	China (27%); Indonesia (18%)
Oceania	*Total*	5,111	39	15	12	17.0B	Australia (87%); Papua New Guinea (9%)
Europe	Eastern	1,156	38	13	16	4.7B	Russia (89%); Poland (4%)
	Western	873	28	16	6	2.6B	Finland (26%); Sweden (25%)
	Total	2,029	34	14	12	7.3B	Russia (57%); Finland (9%)
Total	*Total*	35,891	44	14	7	106.1B	Canada (17%); Australia (14%)

Table A.2 displays data on mining projects from the SNL Metals & Mining Group on the total number of mines by region (column 3); the proportion of mines in each region that extract gold (column 4), copper (column 5), and iron ore (column 6) which are the largest three commodities in terms of the number of mines globally; the total amount of investment in U.S. dollars in each region from 1997 to 2014 (column 7); and the top two host countries of investments in each region from 1997 to 2014 (column 8) along with the proportion of regional investment made in that country.

Figure A.2: Trends in Exploration Investment and Mineral Prices

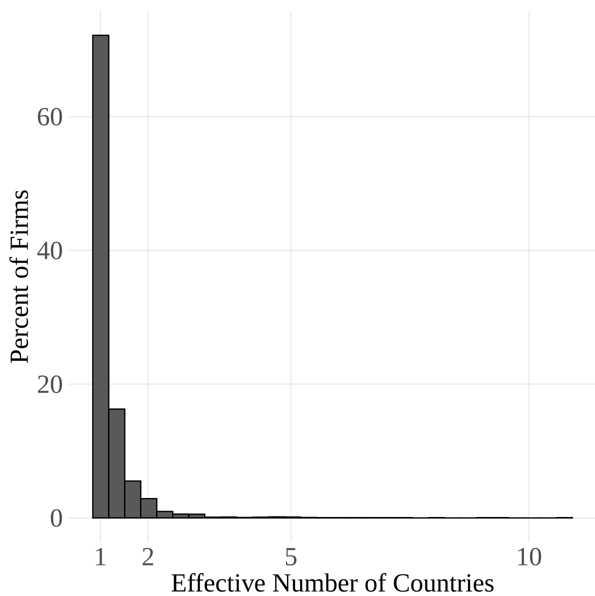


(a) Total Investment, 1997-2014

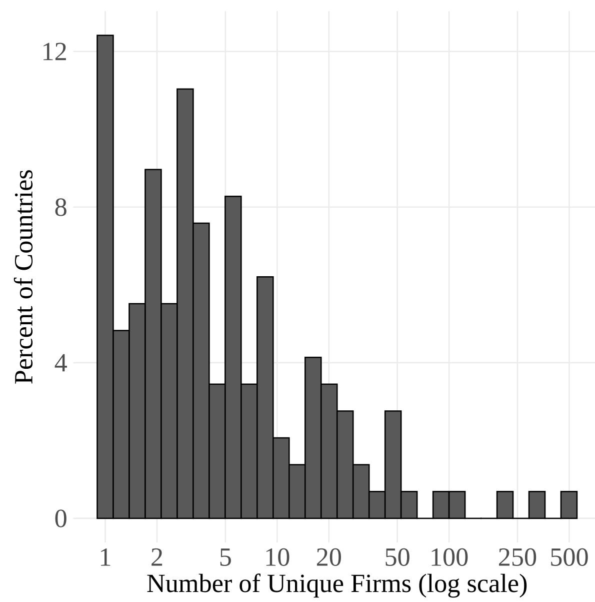
(b) Mineral Prices (indexed), 1997-2014

Figure A.2(a): we plot data on total levels of investment in exploration for minerals globally from SNL Metals & Mining Group. **Figure A.2(b)**: we plot annual price indices from the World Bank Commodity "Pink Sheet" for (a) metals and minerals; (b) base metals ("base"), excluding iron ore; and (c) precious metals ("precious").

Figure A.3: Firms Concentrate Investment in Small Number of Countries



(a) Effective Countries Invested by Firm



(b) Firms Invested by Country

Figure A.3(a): for each firm, we compute the effective number of countries that it invests in $(1/\sum_c s_{ic}^2)$ and average this measure across years. The figure plots the distribution of this measure. **Figure A.3(b)**: for each country, we determine the unique number of firms making positive investments and average this number across years. (We exclude country-years with no investment.) The figure plots the distribution of this measure.

Figure A.4: SNL Mining Projects

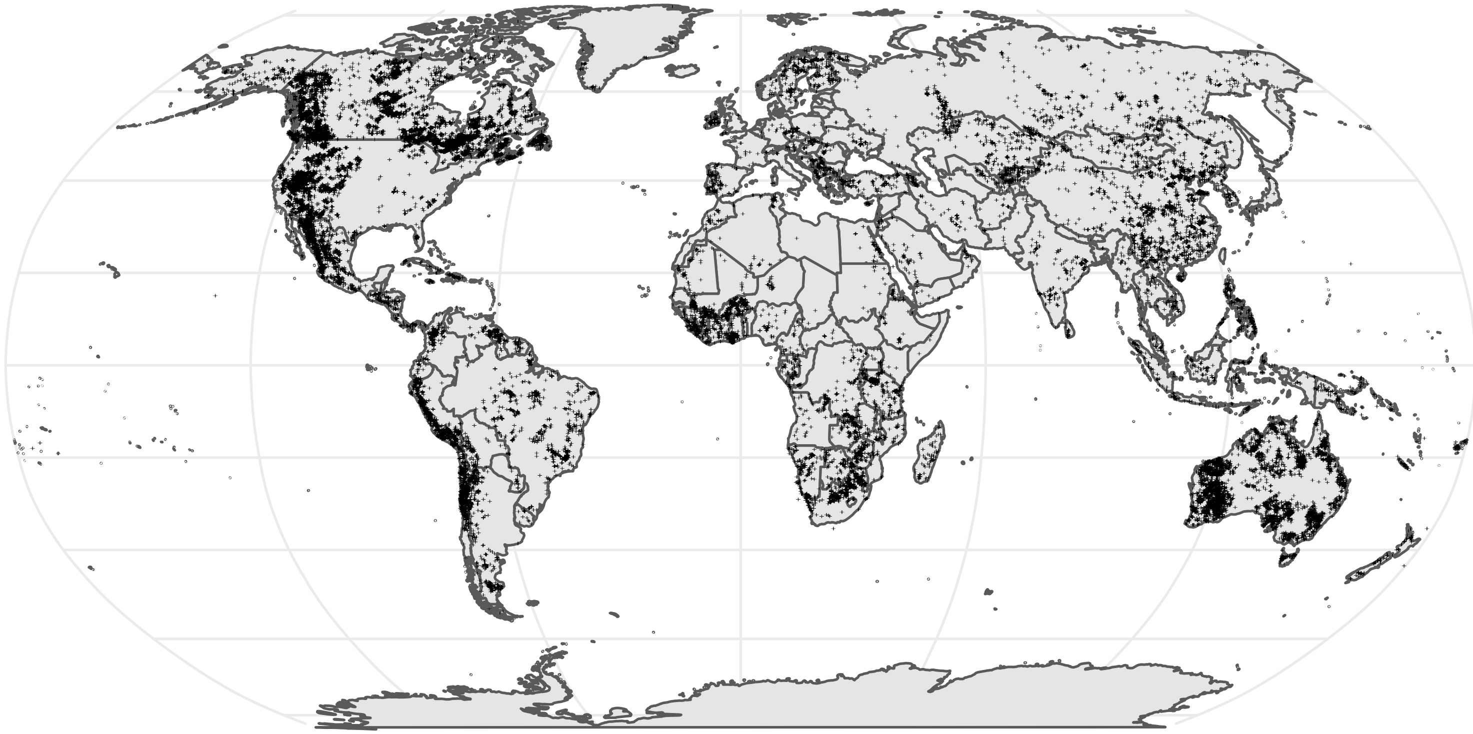


Figure A.4 maps the locations of mining projects in the SNL data.

B.2 Armed Conflict

Figure A.5: UCDP Conflict Events

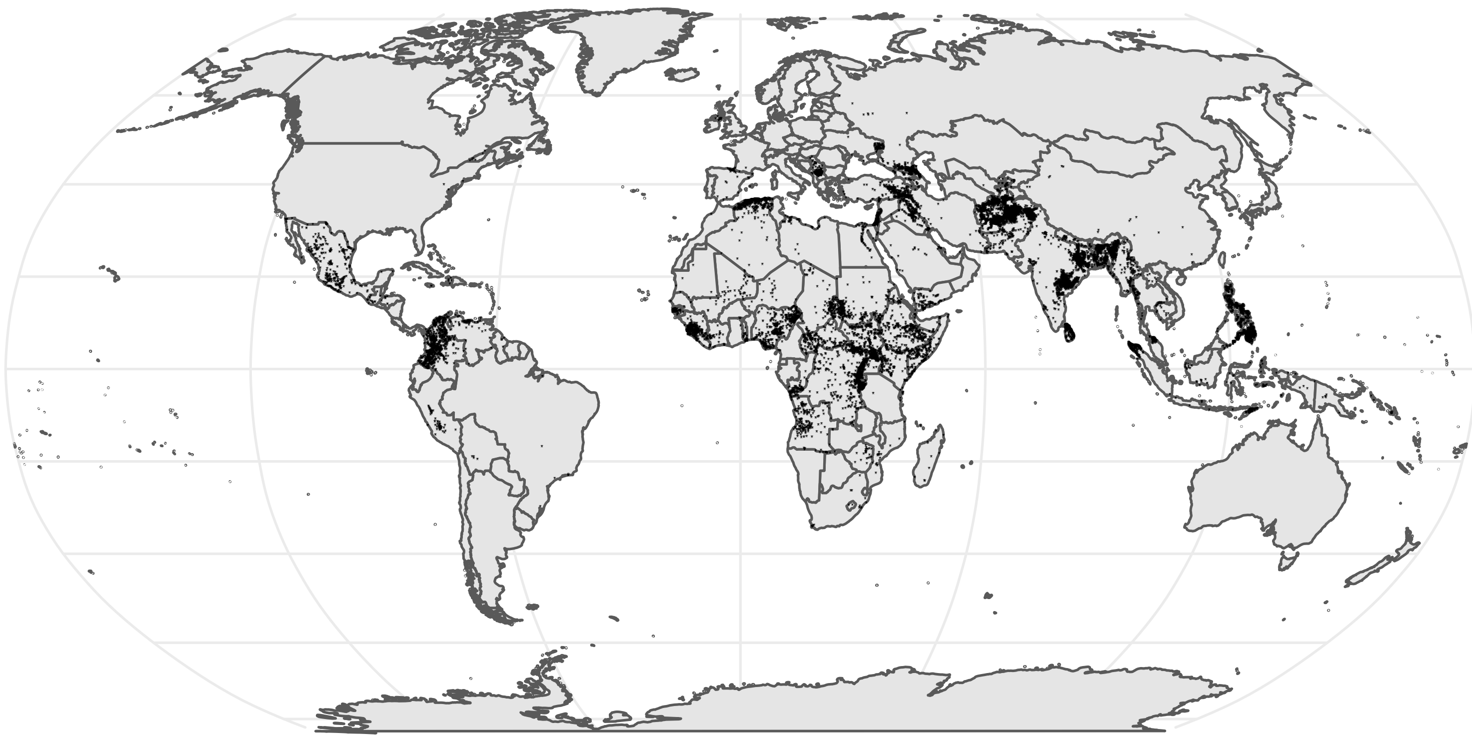


Figure A.5 maps the sites of conflict events in the UCDP data from 1997 to 2014. We only retain events geocoded based on the exact location or within 25 km of a known point.

Table A.3: Conflict by Region

Continent	Region	# Events	% Fatal	# Deaths	Event Type		
					% State	% Nonstate	% One-sided
Africa	Central	4,378	87	99,630	36	9	56
	East	7,962	79	162,471	64	16	21
	North	5,541	92	78,492	71	9	21
	Southern	85	49	236	14	69	16
	West	3,920	88	58,669	36	22	41
	Total	21,886	86	399,498	55	14	31
Americas	Central	1,813	82	16,860	1	95	4
	North	25	100	3,050	8	88	4
	South	4,388	93	21,436	66	7	27
	Total	6,226	90	41,346	47	33	20
Asia	Central	152	88	1,537	84	16	0
	East	29	72	292	38	10	52
	South	42,914	93	224,902	82	3	16
	Southeast	8,250	94	30,808	58	3	39
	Western	9,814	93	74,891	81	3	16
	Total	61,159	93	332,430	78	3	19
Oceania	*Total*	38	97	257	55	42	3
Europe	Eastern	3,401	80	23,459	93	0	7
	Western	139	78	449	86	0	14
	Total	3,540	79	23,908	93	0	7
Total		94,348	90	807,749	72	7	21

Table A.3 presents an overview of the UCDP conflict data by region. For each region, we provide the total number of conflict events, the percentage of those events that were fatal, the total number of deaths, and the percentage of events that were state-based, non-state-based, and one-sided.

Table A.4: Number of Firm-Country-Year Observations and Investment by Exposure to Conflict

	0-5	5-20	20-30	30-40	40-50	50-60	Beyond 60
Num. firm-country-years	18	243	143	178	176	156	2,316,088
Investment (million USD)	79	734	290	685	781	743	32,490

Table A.4 reports the total number of firm-country-years that experience conflict 0–5 km, 5–20 km, 20–30 km, 30–40 km, 40–50 km, 50–60 km, and more than 60 km from a mine. We also report the total exploration investment (in million USD) for firms with projects within these distance thresholds.

C. Defining Exposure to Conflict

D_{ict}^{k} is an indicator for whether a conflict occurred in bandwidth k for any of firm i's projects in country c and year t. Figure A.6 illustrates how the k^{th} bandwidth is constructed for the estimates displayed in Figure 2.

Figure A.6: How Bandwidths are Constructed around Mining Projects

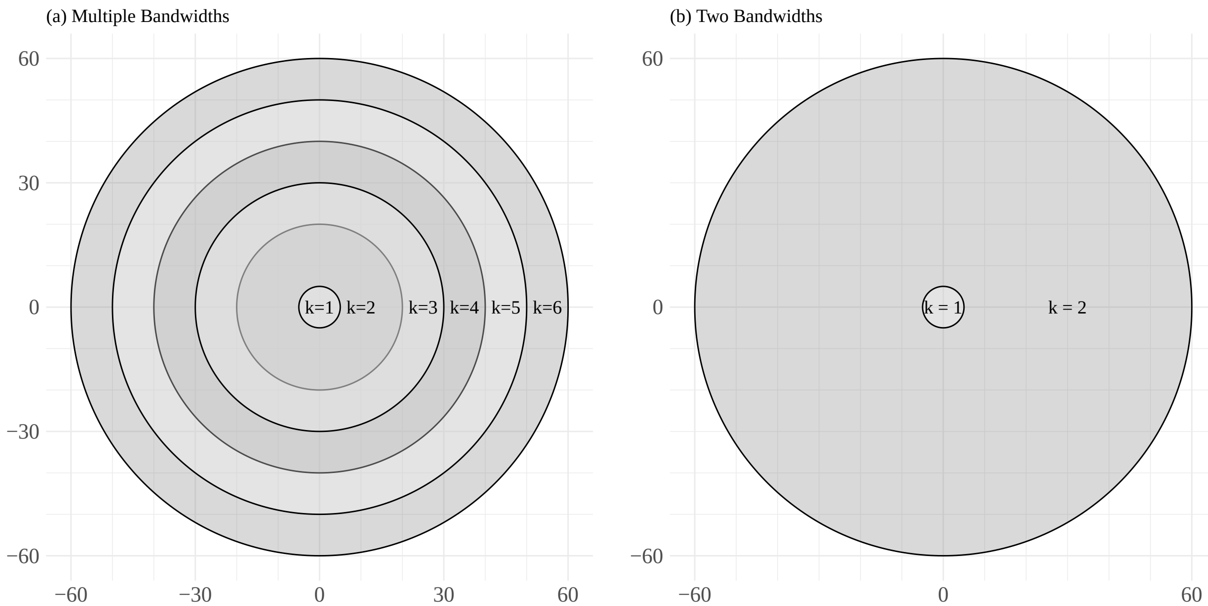


Figure A.6(a) illustrates how we construct the bandwidths for the estimates in Figure 2(a). **Figure A.6(b)** illustrates how we construct the bandwidths for the estimates in Figure 2(b). In both cases, the centroid of the circle represents a firm's mining project.

C.1 Path Distance between Conflicts and Mining Projects

We use Euclidean distance to construct the bandwidths in Figure A.6. To provide a better sense for the space and time that separates armed conflict events and mining projects, we also calculate the (weighted) path distance (i.e., the distance traveled along roads) between armed conflict events and mining projects that fall in the 5–20 kilometer buffer zone. Mining projects exposed to armed conflict are often located in rugged and rural parts of middle- and low-income countries, where infrastructure is limited. As such, the Euclidean distance understates how long one would have to travel to move between a conflict site and a mining project.

We use the gRoads data, which maps known roads across the world between 1980–2010. (We prefer this more historic data as contemporary maps may include roads that did not exist during our study period.) Let l_c be the location (i.e., coordinates) of a conflict and l_m be the location of a mine. Moreover, let v_c be the vertex (i.e., point) on any road network that is closest to l_c in terms of Euclidean distance; v_m, the vertex on any road network that is closest to l_m.

We first measure $d(l_c, v_c)$ and $d(l_m, v_m)$, where $d(\cdot)$ computes the Euclidean distance between two points. We then measure the shortest path distance (i.e., the shortest route along roads) between v_c and v_m: $pd(v_c, v_m)$. For 26 (of 594) conflict-mine pairs, we cannot compute pd because the roads closest to the conflict do not even connect to the roads closest to the mine. The (unweighted) path distance is from l_c to l_m is then: $d(l_m, v_m) + pd(v_m, v_c) + d(v_c, l_c)$.

We know that road quality affects travel costs. We use the dodgr package in R to assigns weights to different types of roads. These are best thought of as the relative costs of traveling 1 km along different types of roads. The package assigns travel along a motorway (e.g., freeway) a base weight of 1. Travel along a service road, for example, receives a weight of 2.5; unclassified roads receive a weight of 1.67. After weighting the segments of our road networks, we re-compute the shortest weighted path distance: $pd'(v_m, v_c)$. We further weight $d(l_c, v_c)$ and $d(l_m, v_m)$ by 2.5, which is equivalent to assuming that travel from the mine or conflict to the road network follows a perfectly straight service road. (This likely understates the cost of travel from the mine or conflict to the road network). The weighted path distance is then: $d(l_m, v_m) * 2.5 + pd'(v_m, v_c) + d(v_c, l_c) * 2.5$.

The average Euclidean distance between conflicts and mines in the 5–20 km buffer zone is 13.7 km, the average unweighted path distance is 39.2 km, and the average weighted path distance is 71.2 km. (The 26 mine-conflict pairs for which we cannot compute the path distance are dropped, which likely attenuates these averages.) Conflict sites and mining projects are separated by a "travel distance" that is equivalent to getting on a clear freeway and driving just over 71 km, which is five times the average crow-flies distance. These distance measure all positively correlated, the correlation between the Euclidean and unweighted path distance is 0.3; the correlation between the unweighted and weighted path distance measures exceeds 0.99.

D. Firm-Country-Year Results

Table A.5: Drop Observations with Excluded (Imprecisely Geocoded) Conflicts

	Dependent variable:			
	Log(Exploration Investment + 1)			
	(1)	(2)	(3)	(4)
0-5 km	-2.43^{*}	-2.80^{**}	-2.39^{*}	-2.75^{**}
	(1.26)	(1.24)	(1.25)	(1.24)
5-20 km	1.54^{***}	1.56^{***}		
	(0.45)	(0.47)		
20-30 km	1.16^{**}	1.18^{**}		
	(0.49)	(0.52)		
30-40 km	2.87^{***}	2.93^{***}		
	(0.43)	(0.46)		
40-50 km	1.65^{***}	1.36^{***}		
	(0.43)	(0.45)		
50-60 km	0.83^{*}	0.61		
	(0.46)	(0.50)		
5-60 km			1.64^{***}	1.56^{***}
			(0.24)	(0.26)
Firm-Country FE	768,888	768,888	768,888	768,888
Firm-Year FE	42,544	42,544	42,544	42,544
Observations	7,530,288	7,529,117	7,530,288	7,529,117

Table A.5 reports results from OLS models estimated using Equation 2. 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 (2) 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 (3) and (4), we employ only two bandwidths: 0–5 km or 5–60 km. In Models (2) and (3), we drop all firm-country-years where a firm operated in a project in an ADM2 (and year) with an event that can only be geocoded to the second-order administrative district (e.g., counties in the US). Significance: *p<0.1; **p<0.05; ***p<0.01.

Table A.6: Effect of Armed Conflict on Investment at the Firm-Country Level with Firm-Country and Year Fixed Effects

	Dependent variable:	
	Log(Exploration Investment + 1)	
	(1)	(2)
0-5 km	-2.49^{*}	-2.45^{*}
	(1.32)	(1.32)
5-20 km	1.54^{***}	
	(0.48)	
20-30 km	1.17^{**}	
	(0.52)	
30-40 km	2.88^{***}	
	(0.45)	
40-50 km	1.67^{***}	
	(0.46)	
50-60 km	0.82^{*}	
	(0.49)	
5-60 km		1.65^{***}
		(0.26)
Beyond 60 km	-0.002^{*}	-0.002^{*}
	(0.001)	(0.001)
Firm-Country FE	768,888	768,888
Year FE	18	18
Observations	7,530,288	7,530,288

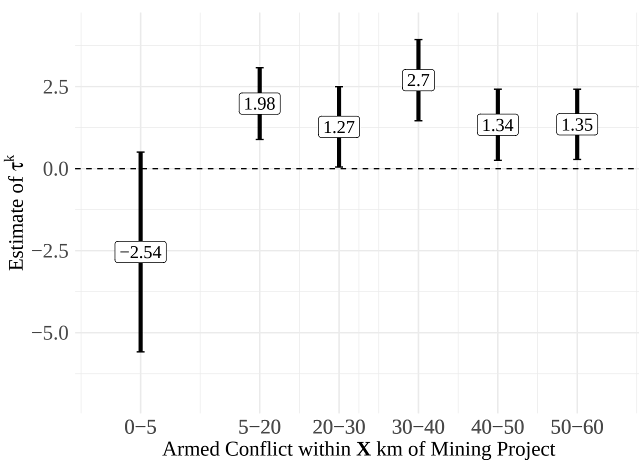
Table A.6 reports results from OLS models estimated using a modified version of Equation 2, which differs from Equation 2 in that year fixed effects are estimated instead of firm-year fixed effects. We cluster standard errors at the firm-year level, shown in parentheses. The dependent variable is exploration investment (logged plus one). The independent variable in model (1) 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, 50–60 km, or beyond 60 km from a mining project (see Figure A.6). In model (2), we employ two bandwidths: 0–5 km, 5–60 km, or beyond 60 km. Significance: *p<0.1; **p<0.05; ***p<0.01.

Table A.7: Effects of Conflict Exposure on Investment by Intensity of Conflict

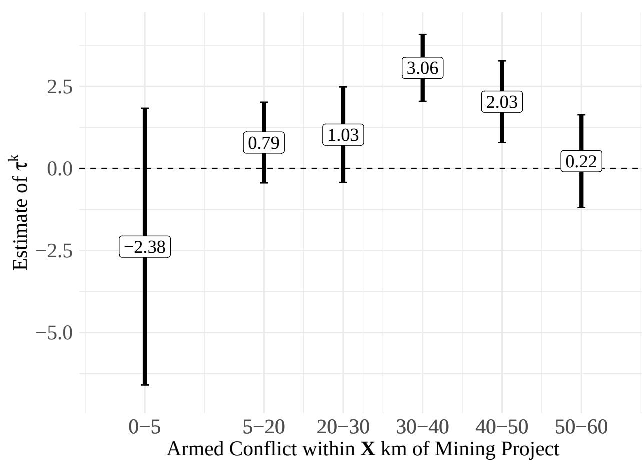
	Dependent variable:			
	Log(Exploration Investment + 1)			
	(1)	(2)	(3)	(4)
0-5 km	-2.39^{*}	-2.51	-2.43^{*}	-2.50
	(1.25)	(1.55)	(1.25)	(1.55)
0-5 km x Conflict intensity		0.31		0.18
		(2.65)		(2.64)
5-60 km	1.64^{***}	1.76^{***}	1.63^{***}	1.78^{***}
	(0.24)	(0.30)	(0.24)	(0.30)
5-60 km x Conflict intensity		-0.27		-0.34
		(0.38)		(0.38)
Beyond 60 km			-0.002^{*}	0.0001
			(0.001)	(0.001)
Beyond 60 x Conflict intensity				-0.02^{***}
				(0.002)
Firm-Country FE	768,888	768,888	768,888	768,888
Firm-Year FE	42,544	42,544	42,544	42,544
Country-Year FE	3,186	3,186	0	0
Observations	7,530,288	7,530,288	7,530,288	7,530,288

Table A.7 reports results from OLS models estimated using Equation 2. We cluster standard errors at the firm-year level, shown in parentheses. The dependent variable is exploration investment (logged plus one). The independent variable codes whether a conflict occurred in a given year (t) or in the year prior (t-1) between 0–5 km or 5–60 km. Models (1) and (3) replicate our results in Table 3. In models (2) and (4), we fully interact conflict with the intensity of the conflict in the country-year. High intensity is defined as more than 1,000 fatalities in a country-year using our GED conflict outcome data. Models (1) and (2) include country-year fixed effects, which absorbs the "Beyond 60 km" term included in models (3) and (4) (see Figure A.7). Significance: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure A.7: Effects of Conflict Exposure on Investment by Intensity of Conflict



(a) Low Intensity



(b) High Intensity

Figure A.7 displays effects from Equation 2 fully interacted with the intensity of the conflict in the country-year. High intensity is defined as more than 1,000 fatalities in a country-year using our GED conflict outcome data (see Table A.7).

Figure A.8: Coefficient Stability when Excluding Multi-Country Firms

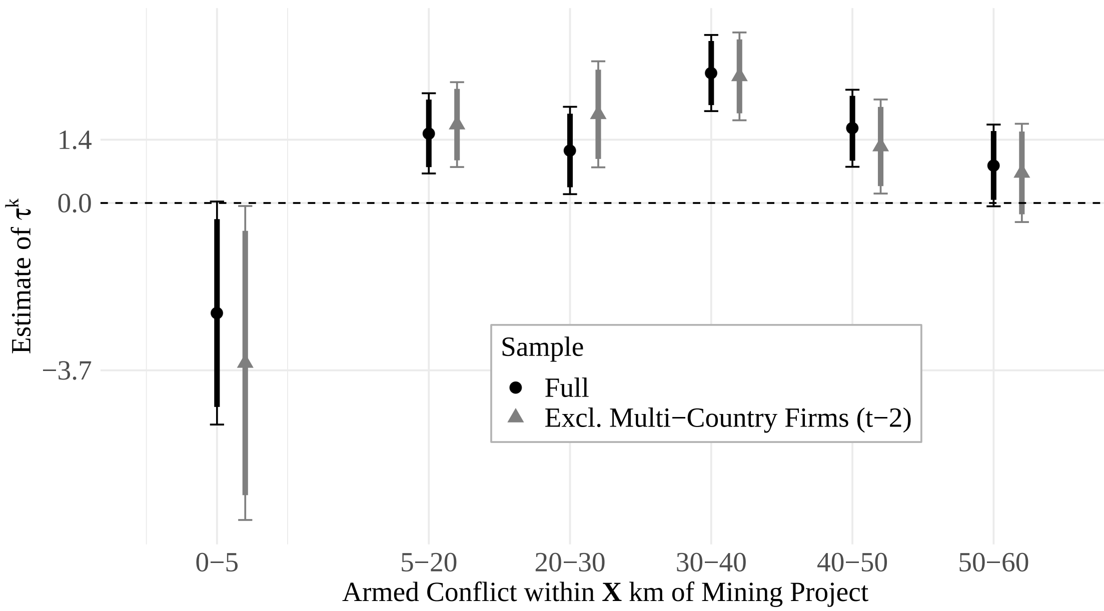


Figure A.8 displays effects from Equation 4, excluding firms that invest in multiple countries. In one specification, we include the full sample. In another specification, we drop firms with investments in multiple countries in $t-2$.

Figure A.9: Coefficient Stability when Excluding Multi-Project Firm-Countries

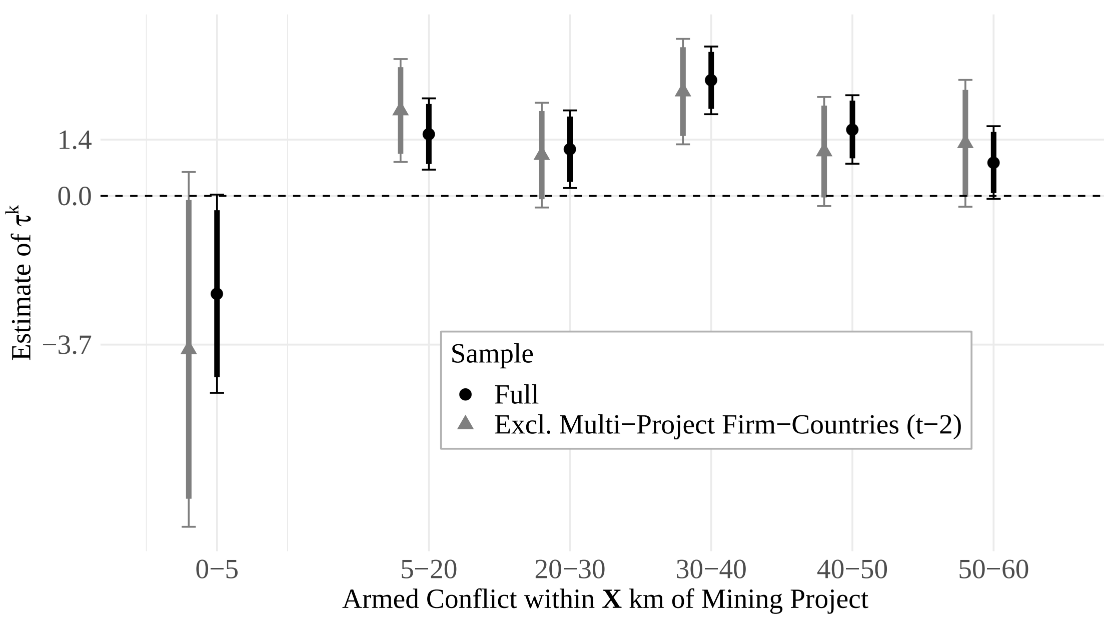


Figure A.9 displays effects from Equation 2, excluding firms that invest in multiple projects in the same country. In one specification, we include the full sample. In another specification, we drop observations when the firm had multiple projects in that country in $t-2$.

Table A.8: Dynamic Panel Estimates of Effects of Conflict Exposure in Buffer Zone on Investment

	Dependent variable:
	Log(Exploration Investment + 1)
0-5 km, lead 3	-1.49^{*}
	(0.86)
0-5 km, lead 2	-5.10^{*}
	(3.06)
0-5 km, lead 1	-0.03
	(0.84)
0-5 km, contemporaneous	-2.82
	(2.02)
0-5 km, lag 1	-2.69
	(2.07)
0-5 km, lag 2	-3.42
	(2.36)
0-5 km, lag 3	0.89
	(1.34)
5-60 km, lead 3	-0.37
	(0.40)
5-60 km, lead 2	-0.07
	(0.39)
5-60 km, lead 1	0.61
	(0.40)
5-60 km, contemporaneous	1.23^{***}
	(0.39)
5-60 km, lag 1	1.44^{***}
	(0.42)
5-60 km, lag 2	0.95^{**}
	(0.39)
5-60 km, lag 3	1.47^{***}
	(0.41)
Firm-Country FE	526,221
Firm-Year FE	19,214
Country-Year FE	2,124
Observations	3,400,878

Table A.8 reports results from OLS models estimated using a version of Equation 2 modified to include leads and lags. We cluster standard errors at the firm-year level, shown in parentheses. The dependent variable is exploration investment (logged plus one). The independent variable codes whether conflict occurred in a given year, in one of the three years prior, or in one of the three years after within a given distance of a firm's mining projects. We report the effects between 0–5 km and 5–60 km (see Figure A.10). Significance: *p<0.1; **p<0.05; ***p<0.01.

Figure A.10: Dynamic Panel: Effect of Exposure to Armed Conflict in Buffer Zone on Investment

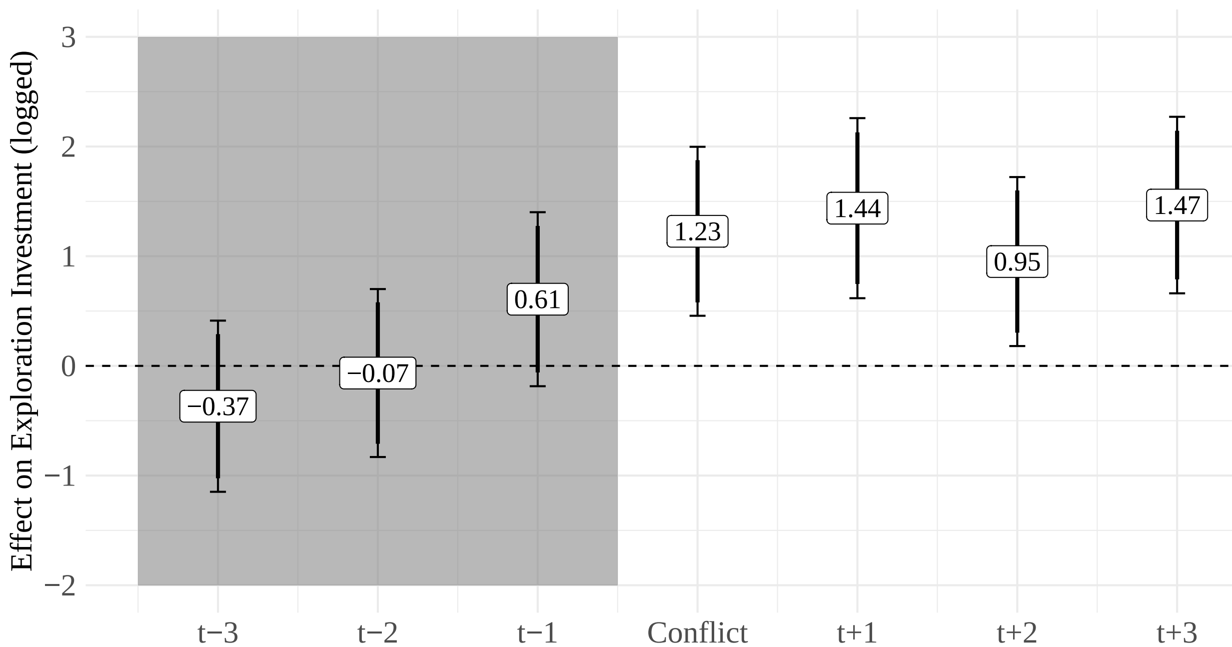


Figure A.10 displays dynamic panel estimates for the effects of exposure to conflict in the buffer zone around violence, defining the buffer zone as 5–60 km from conflict. We display contemporaneous effects and effects three years prior to and three years following conflict (see Table A.8).

E. Sector-Country-Year Results

In this section, we report on analyses of the effect of armed conflict on investment in sectors beyond mining. We construct data at the sector-country-year level using the fDi Markets (2019) dataset. The fDi Markets data records data about investment projects, including the total value and number of jobs anticipated to be created, initial year of investment, sector, owner firm, and location. We collapse the data to the sector-country-year level, calculating total investment value and number of projects.[38]

We conduct two analyses. First, we fit the following sector-country-year model:

$$y_{sct} = \omega_s + A_c + \Delta_t + \beta C_{ct} + \sum^{s} \zeta_s C_{ct} + \varepsilon_{sct} \tag{5}$$

where Y_{sct} is aggregate investment (logged) at the sector-country-year level, ω_s represents sector fixed effects, A_c represents country fixed effects, Δ_t year fixed effects, C_{ct} is an indicator for whether an armed conflict occurred in country c in year t or in the previous year $t-1$, and ζ_s is a sector-specific estimate of the effect of armed conflict (i.e., an interaction between the sector and armed conflict indicators). We cluster our standard errors on country.

In Figure A.11, we display a histogram of ζ_s, highlighting natural resource sectors. This analysis suggests that, while there is variation across sectors, the resource sectors (metals; minerals; coal, oil, and natural gas; and wood products) are not anomalous. Second, we fit the same model but combine the mining and metals sectors into a single sector. Table A.9 displays the regression coefficients for ω_s for mining and metals, β, and ζ_s for mining and metals. We, again, do not find evidence that the mining and metals sectors that we study differ significantly in their investment response to armed conflict.

[38] Given that investments in the fDi Markets data are lumpy and not presented as yearly flows, we cannot construct data comparable to our exploration investment outcome. This is an important difference — after a project has been launched, we cannot observe firms curtailing investments in response to armed conflict.

Figure A.11: Histogram of Sector-Armed Conflict Interactions

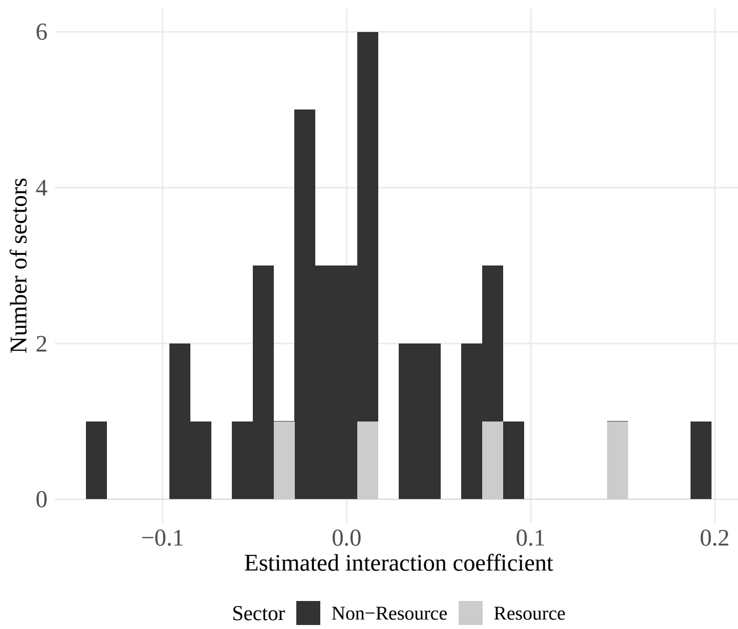


Figure A.11 displays coefficient estimates of the interaction between an armed conflict indicator and the sector of investment in a model fit on sector-country-year data. Natural resource sectors are highlighted, including metals; minerals; coal, oil, and natural gas; and wood products.

Table A.9: Differential Effects of Conflict on Aggregate Investment by Sector

	Dependent variable:		
	Log(N Investments + 1)	Log(Total Investment + 1)	Log(N Firms + 1)
	(1)	(2)	(3)
$\mathbb{1}$(Conflicts > 0) (C_{ct})	−0.002	−0.03	−0.004
	(0.01)	(0.03)	(0.01)
Minerals sector	0.14***	0.61***	0.14***
	(0.02)	(0.09)	(0.02)
Minerals sector $\cdot$ C_{ct}	0.04	0.16	0.04
	(0.03)	(0.12)	(0.03)
F-stat	13.4	18.28	13.88
p-value	0.00	0.00	0.00
$\overline{y_{sct}}$	0.41	1.22	0.39
Sector FE	37	37	37
Country FE	160	160	160
Year FE	12	12	12
Observations	74,880	74,880	74,880

Table A.9 reports results from OLS models estimated using Equation 5. We cluster standard errors at the country level, shown in parentheses. The three models report on three dependent variables: the number of foreign direct investments (logged plus one) in a sector-country year; the total value of those investments (logged plus one); and the number of firms making investments (logged plus one). The independent variables code whether conflict occurred in a given year or the preceding year, whether the sector is metals and minerals (i.e., reporting one of the sector fixed effects), and the interaction. The model includes sector, country, and year fixed effects. Significance: *p<0.1; **p<0.05; ***p<0.01.

F. Country-Year Results

Table A.10: Effect of Armed Conflict on the Number of Firms Investing at the Country Level

	Dependent variable:						
	Log(N Firms + 1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\mathbb{1}$(Conflicts $>$ 0)	-0.13^{**}	-0.11^{**}					
	(0.05)	(0.05)					
$\mathbb{1}$(Conflicts = 1)			-0.12^{**}				
			(0.05)				
$\mathbb{1}$(Conflicts $>$ 1)			-0.13^{**}				
			(0.06)				
$\mathbb{1}$(State-Based $>$ 0)				-0.08			-0.05
				(0.05)			(0.05)
$\mathbb{1}$(One-Sided $>$ 0)					-0.12^{**}		-0.10^{*}
					(0.06)		(0.06)
$\mathbb{1}$(Non-State $>$ 0)						-0.09	-0.08
						(0.07)	(0.07)
F-stat	5.90	4.41	3.75	2.26	4.14	1.82	1.99
p-value	0.02	0.04	0.02	0.13	0.04	0.18	0.11
$\overline{y_{ct}}$	1.26	1.54	1.26	1.26	1.26	1.26	1.26
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
Observations	3,186	2,610	3,186	3,186	3,186	3,186	3,186

Table A.10 reports results from OLS models estimated using Equation 4. We cluster standard errors at the country level, shown in parentheses. The dependent variable is the total number of firms (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.

Figure A.12: Dynamic Panel Estimates: Country-level Effect of Conflict on Investment

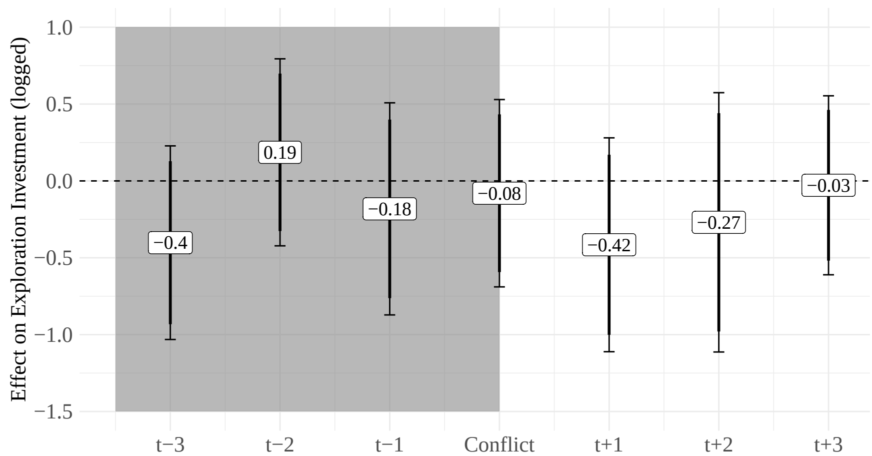


Figure A.12 displays point estimates and 95% confidence intervals (thicker bars: 90% CIs) on the leads and lags of armed conflict incidence. Equation 4 is only amended to include these leads and lags and then estimated using OLS. The independent variable codes whether conflict occurred in a given year, in one of the three years prior, or in one of the three years after within a given distance of a firm's mining projects. Standard errors are clustered on country (see Table A.11).

Table A.11: Dynamic Panel Estimates of Effect of Armed Conflict on Investment at the Country Level

	Dependent variable:
	Log(Exploration Investment + 1)
Lead 3	−0.40
	(0.32)
Lead 2	0.19
	(0.31)
Lead 1	−0.18
	(0.35)
Contemporaneous	−0.08
	(0.31)
Lag 1	−0.42
	(0.35)
Lag 2	−0.27
	(0.43)
Lag 3	−0.03
	(0.30)
F-stat	0.91
p-value	0.5
$\overline{y_{ct}}$	9.75
Country FE	177
Year FE	12
Observations	2,124

Table A.11 reports 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 independent variable codes whether conflict occurred in a country in a given year, in one of the three years prior, or in one of the three years after (see Figure A.12). Significance: *p<0.1; **p<0.05; ***p<0.01.

F.1 Heterogeneous Effects by Type of Firm

We have limited information on the characteristics of firms in our sample. To assess whether reputational risks explain the investment response of firms operating in conflict-affected countries, we identify two types of firms that we expect to be more concerned about their reputations. First, we code firms as mining "majors" or "juniors" based on market capitalization using data from 2014, at the end of our sample period, from `mineweb`'s list of firms which we match by hand to our firm names. While there is no consensus definition of mining majors, we code the top 100 by market capitalization as majors. Second, we code firms with investments in more than three countries as multinationals.

We separately aggregate investment to the country-year by firm type. We then fit a version of Equation 4, where we interact our conflict variable with our indicator for firm type. This allows us to assess whether the effects of armed conflict on exploration investment differ by firm type.

We present the results in Table A.12. We do not detect significant differences in how different types of firms respond to armed conflict (see models 1 and 3). While our estimates are not significant, our point estimates suggest (model 2) that major firms pull back more sharply in response to state-based conflicts (while junior firms react more strongly to one-sided and non-state conflicts). If association with a repressive state poses a larger reputational risk for major firms (as suggested in Henisz 2017), then these results are consistent with those firms acting to more aggressively limit that risk.

Table A.12: Differential Effects of Conflict on Aggregate Investment by Firm Type

	Dependent variable:			
	Log(Exploration Investment + 1)			
	Majors		Multinationals	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Conflicts > 0)	-0.54^{*}		-0.56	
	(0.32)		(0.53)	
$\mathbb{1}$(State-Based > 0)		-0.03		-0.19
		(0.44)		(0.56)
$\mathbb{1}$(One-Sided > 0)		-0.73^{**}		-1.03^{*}
		(0.30)		(0.57)
$\mathbb{1}$(Non-State > 0)		-0.61		-0.30
		(0.44)		(0.49)
Large firm · $\mathbb{1}$(Conflicts > 0)	0.003		-0.09	
	(0.52)		(0.51)	
Large firm · $\mathbb{1}$(State-Based > 0)		-0.37		-0.01
		(0.49)		(0.60)
Large firm · $\mathbb{1}$(One-Sided > 0)		0.50		0.29
		(0.51)		(0.67)
Large firm · $\mathbb{1}$(Non-State > 0)		0.56		0.16
		(0.66)		(0.64)
F-stat	0.52	8.5	1.13	5.03
p-value	1.00	0.00	0.04	0.00
$\overline{y_{ct}}$	7.5	7.5	8.06	8.06
Country FE	177	177	177	177
Year FE	18	18	18	18
Observations	6,372	6,372	6,372	6,372

Table A.12 reports the results from OLS models estimated using Equation 4, fit on identical stacked data with two different outcomes and an indicator for which outcome was used in order to test the hypothesis that effects differ between the two outcomes. In Model 1, the two outcomes are exploration investment of mining major firms (top 100 firms by market capitalization) and exploration investment of junior mining firms. In Model 3, by multinational firms (investment in > 3 countries) and non-multinational firms. Models 2 and 4 are identical except with different predictors. 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), in Models 1 and 3. Models 2 and 4 report estimates from Equation 4 using alternative measures of conflict by conflict type. Significance: *p<0.1; **p<0.05; ***p<0.01.

G. Evidence on Mechanisms

G.1 Disrupted Production

Table A.13: Effect of Armed Conflict on Production at the Project-Level (Africa Only)

	Dependent variable:			
	$\mathbb{1}$(Production $>$ 0)	Log(Production + 1)	$\mathbb{1}$(Production $>$ 0)	Log(Production + 1)
	(1)	(2)	(3)	(4)
0-5 km	-0.19^{*}	-2.12	-0.20^{*}	-2.13
	(0.11)	(1.31)	(0.11)	(1.32)
5-60 km	-0.03	-0.05		
	(0.03)	(0.33)		
5-20 km			-0.04	-0.10
			(0.05)	(0.54)
20-30 km			-0.04	-0.09
			(0.06)	(0.67)
30-40 km			0.02	0.51
			(0.04)	(0.45)
40-50 km			-0.06	-0.41
			(0.06)	(0.79)
50-60 km			-0.01	-0.23
			(0.06)	(0.64)
F-stat	2.01	1.32	0.88	0.75
p-value	0.13	0.27	0.51	0.61
$\overline{y_{ict}}$	0.88	9.9	0.88	9.9
Project FE	605	605	605	605
Mineral FE	35	35	35	35
Year FE	23	23	23	23
Observations	7,926	7,926	7,926	7,926

Table A.13 reports results from OLS models with project, year, and mineral fixed effects. We cluster standard errors on project, shown in parentheses. The dependent variable is annual mineral production, measured as both a dummy variable for positive production and the total production (logged plus one). In models (1) and (2) the independent variable codes whether conflict occurred in the one of the three years prior (t-1, t-2, and/or t-3) within 0–5 km or 5–60 km from firm's mine. Models (3) and (4) further subdivide these geographic bandwidths (see Figure A.6). Data availability is limited to mining projects in Africa. Significance: *p<0.1; **p<0.05; ***p<0.01.

Figure A.13: Dynamic Panel: Effect of Armed Conflict within 5 km of a Mine on Production

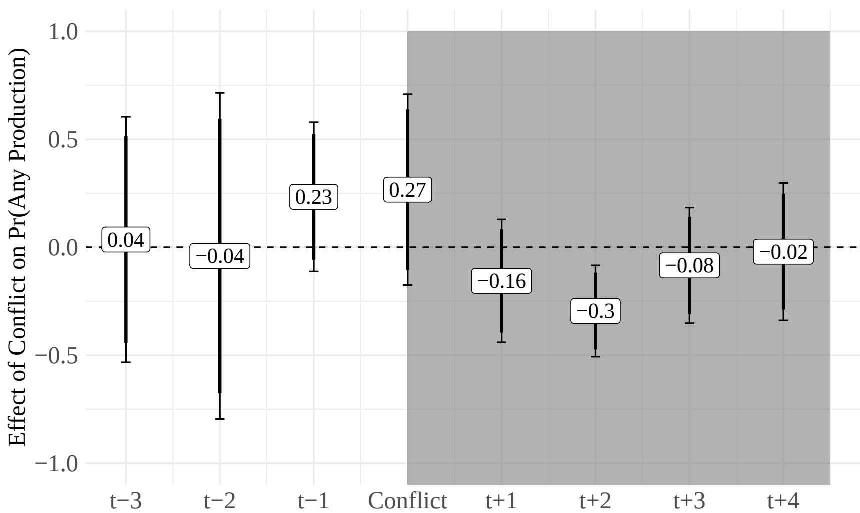


Figure A.13 reports point estimates and 95% confidence intervals (thicker bars: 90% CIs) on the leads and lags of armed conflict incidence within 5 km of a mining site. Estimates based on a linear probability model with project, year, and mineral fixed effects. We cluster standard errors on project. Data availability is limited to mining projects in Africa.

G.2 State Capacity

Figure A.14: Elasticity of Resource Production and Taxes amid Conflict

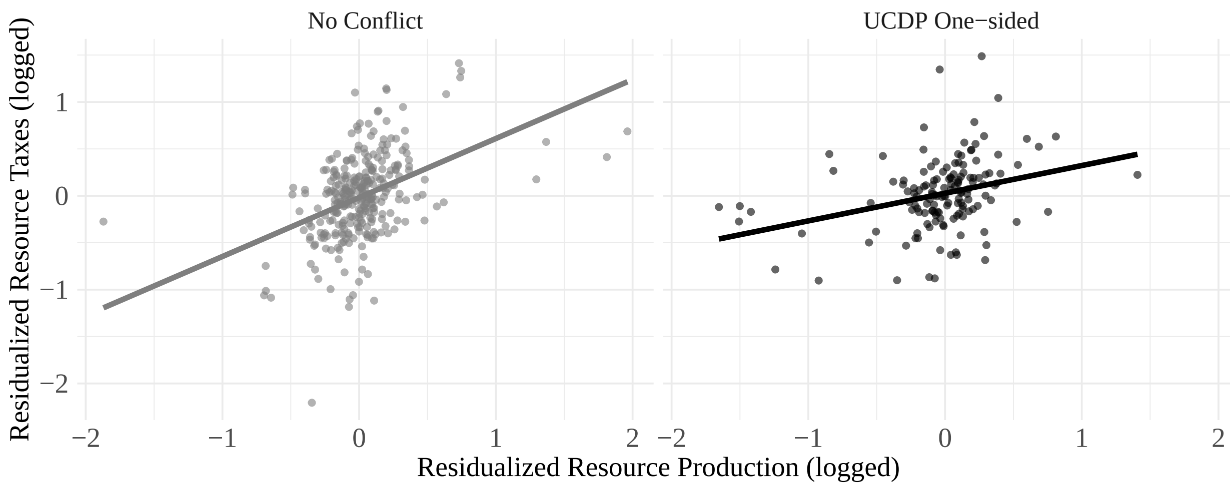


Figure A.14 displays the elasticity between natural resource production and resource tax revenues in countries without conflict (left) and with a one-sided conflict (right). Both series are logged to compute the elasticity; we also residualize using country fixed effects (see Table A.14).

Table A.14: Elasticity of Resource Production and Taxes as a Function of Conflict

	Dependent variable:			
	Log(Resource Tax Revenues + 1)			
	(1)	(2)	(3)	(4)
P_{t-1}	0.562^{***}	0.485^{***}	0.534^{***}	0.453^{***}
	(0.112)	(0.127)	(0.117)	(0.123)
$P_{t-1} \times \mathbb{1}(\text{One-sided} > 0)$	-0.088^{**}			
	(0.037)			
$P_{t-1} \times \mathbb{1}(\text{State-based} > 0)$		-0.045		
		(0.036)		
$P_{t-1} \times \mathbb{1}(\text{State-based} \mid \text{One-sided} > 0)$			-0.062^{*}	
			(0.033)	
$P_{t-1} \times \mathbb{1}(\text{Non-state} > 0)$				0.089
				(0.064)
F-stat	8.63	5.39	7.73	6.41
p-value	0.00	0.00	0.00	0.00
$\overline{y_{ct}}$	21.98	21.98	21.98	21.98
Country FE	32	32	32	32
Region-Year FE	75	75	75	75
Observations	441	441	441	441

Table A.14 reports results from OLS models with country and region-by-year fixed effects. We use log-log specifications to estimate the elasticity, in which resource tax revenues are the dependent variable, and mineral production value interacted with conflict incidence is the independent variable. We cluster standard errors on country, shown in parentheses (see Figure A.14). Significance: *p<0.1; **p<0.05; ***p<0.01.

G.3 Policy Change

Table A.15: Country-level Effect of Armed Conflict on Government Stability

	Dependent variable:					
	Internal Conflict Index			Government Stability Index		
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(Conflicts > 0)	-0.49^{***}			-0.20^{*}		
	(0.15)			(0.11)		
$\mathbb{1}$(Conflicts = 1)		-0.19			-0.23^{*}	
		(0.15)			(0.12)	
$\mathbb{1}$(Conflicts > 1)		-0.62^{***}			-0.18	
		(0.16)			(0.13)	
$\mathbb{1}$(State-Based > 0)			-0.54^{***}			0.20
			(0.15)			(0.13)
$\mathbb{1}$(One-Sided > 0)			-0.41^{***}			-0.24^{*}
			(0.13)			(0.13)
$\mathbb{1}$(Non-State > 0)			-0.37^{**}			-0.45^{***}
			(0.17)			(0.13)
F-stat	11.21	7.81	9.41	3.1	2.18	6.67
p-value	0.00	0.00	0.00	0.08	0.11	0.00
$\overline{y_{ct}}$	9.2	9.2	9.2	8.56	8.56	8.56
Country FE	134	134	134	134	134	134
Year FE	18	18	18	18	18	18
Observations	2,394	2,394	2,394	2,394	2,394	2,394

Table A.15 reports the results from OLS models estimated using Equation 4. We cluster standard errors at the country level, shown in parentheses. The dependent variables come from ICRG: models (1)–(3), Internal Conflict Index; models (4)–(6), Government Stability Index. The main independent variable codes whether conflict occurred in a given year (t) or in the year prior (t-1). Significance: *p<0.1; **p<0.05; ***p<0.01.

Table A.16: Effect of Armed Conflict on Investment by New Entrants

	Dependent variable:				
	Log(Exploration Investment + 1) by New Entrants				
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Conflicts > 0)	-0.86^{**}				
	(0.42)				
$\mathbb{1}$(State-Based > 0)		-0.77^{**}			-0.55
		(0.39)			(0.40)
$\mathbb{1}$(One-Sided > 0)			-0.90^{*}		-0.73
			(0.48)		(0.49)
$\mathbb{1}$(Non-State > 0)				-0.82^{*}	-0.69
				(0.45)	(0.44)
F-stat	4.30	4.00	3.51	3.35	2.81
p-value	0.04	0.05	0.06	0.07	0.04
$\overline{y_{ct}}$	6.48	6.48	6.48	6.48	6.48
Country FE	177	177	177	177	177
Year FE	18	18	18	18	18
Observations	3,186	3,186	3,186	3,186	3,186

Table A.16 reports results from OLS models estimated using Equation 4. We cluster standard errors on country, shown in parentheses. We restrict the dependent variable to exploration investment (logged plus one) by new entrants, firms that had not previously invested in a given country. In model (1) the independent variable codes whether conflict occurred in a given year (t) or in the year prior (t-1). Models (2)-(4) evaluate different types of conflict, as classified by UCDP. Model (5) includes indicators for all the different types of conflict. Significance: *p<0.1; **p<0.05; ***p<0.01.

H. Details of Systematic Review

The aim of a systematic review is to "identify, appraise and synthesize all the empirical evidence that meets pre-specified eligibility criteria to answer a specific research question" (Higgins and Green 2011). In this appendix, we present additional details on how we conducted the systematic review as well as our PRISMA systematic review reporting statement.

H.1 Coding Rules for Systematic Review

Measure of Violent Events We exclusively measure counts or incidences of violent events. Where papers measure additional factors related to conflict, we consider this an aggregate risk measure.

Model Selection In order to characterize this literature, we first determine which model we will evaluate from the papers that met our filtering criteria. Based on the table the author(s) highlight as their main empirical results, we select the model that uses unit fixed effects at the same level as their cross-sectional unit of analysis. If unit fixed effects (FE) are not used in the paper, or are not at the same level as the paper's cross-sectional unit of analysis, we select the model that uses an instrumental variable (IV) to instrument for conflict/instability.

In situations where the author(s) neither use FE nor an IV approach, we use their preferred model specification as the main model, if it is favored for reasons that enhance the credibility of the causal inference (i.e., they justify why they have to control for an important confounder). In the absence of author preference, we select the simplest model that relates conflict to investment. When analyses use both an aggregated and a disaggregated measure of conflict/risk, we select the aggregate measure.

We consider this model the main model of the paper. We use this model to characterize the studies in our systematic review.

Study Characteristics Following the selection of the main model, we code a range of characteristics from each paper, which we include in Table A.1. These include: (1) effect; (2) region/countries; (3) years; (4) geographic and time unit of analysis; (5) fixed effects; and (6) instrumental variable. The rationale behind these coding choices is included below:

(A) Effect

$+$, $-$, $\sim$ (null), or mixed (includes at least two of the three above)

We code the effect of conflict on investment based on both the sign and statistical significance (at any level) of the point estimate of the main model (detailed above). Based on the relevant model, we identify the effect variable in three different ways: (1) from a single main model with a single measure of conflict (e.g., the International Country Risk Guide (ICRG) political instability index); (2) from a single main model with multiple measures of conflict (e.g., revolutions and assassinations); and (3) from multiple main models, with different dependent variables, and a single measure of conflict (e.g., investment by sectors). The latter two strategies may lead to a "mixed" effect of conflict on investment, as some coefficients may be significantly positive or significantly negative while others may have no effect. By contrast, the first strategy will only lead to an effect of conflict on investment that is significantly positive, significantly negative, or null.

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.

(B) Region/countries

G (global), LAC, SSA, MENA, BRICS, or country name

We identify the regional focus of the research from the main text of the paper. In addition, we include a variable for the number of countries evaluated in the paper. We code the region(s) and the number of countries used in the analyses based on the countries that receive investments in the data, rather than the number where investment originates (for example, if the data represent FDI from 37 OECD countries in China we would code that as a single country in Asia). Where possible, we use the number of countries listed in the main model. When this information is unavailable, we code the number of countries the author(s) reference in the main text of the paper.

(C) Years

We code years based on the year range provided in main text of the paper. On rare occasions, a paper may subset further down for analysis purposes. If this is the case, we code the more restricted year range if it is used in the main model and available in the main text of the paper.

(D) Geographic and Time Unit of Analysis

We code the geographic and time unit of analysis based on the most disaggregated level of data used in the model (e.g., country-year, sector-month). We identify the unit of analysis based on the subscript in the model equation, when available. Otherwise, we infer this information from the main text of the paper.

(E) Fixed Effects

We identify if a paper uses both unit and/or time fixed effects. We code unit fixed effects if the paper utilizes unit fixed effects at their cross-sectional unit of analysis (e.g., country for country-year panels). We code time fixed effects if the author(s) use time fixed effects at the temporal level of the panel data (e.g., year for country-year panels). As such, we do not code fixed effects if those used in the paper are based on aggregated time periods (i.e. five-year periods rather than years) or geographic units (i.e. continents rather than countries). We identify whether the author(s) use unit and/or time fixed effects based on the model equation (when available) or from the table of the main model.

(F) Instrumental Variables

We code a study as having used instrumental variables if the author(s) specify that they are instrumenting for conflict/instability. We identify whether the author(s) use an instrumental variable from the table of the main model, when possible, or from the main text of the paper.

H.2 PRISMA Checklist for Systematic Reviews

The Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) is a standard checklist for reporting on systematic reviews (Liberati et al. 2009). We document our response to each item on the checklist below.

(1) Title (p. 4)

Identify the report as a systematic review, meta-analysis, or both.

- See text.

(2) Structured Summary (p. 4-5)

Provide a structured summary including, as applicable: background; objectives; data sources; study eligibility criteria, participants, and interventions; study appraisal and synthesis methods; results; limitations; conclusions and implications of key findings; systematic review registration number.

- See text. We do not provide information about participants, interventions, or a systematic review registration number.

(3) Rationale (p. 4)

Describe the rationale for the review in the context of what is already known.

- See text.

(4) Objectives (p. 4)

Provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS).

- See text.

(5) Protocol and registration (p. 4, Appendix H.1)

Indicate if a review protocol exists, if and where it can be accessed (e.g., Web address), and, if available, provide registration information including registration number.

- See text for review protocol. We do not register our systematic review.

(6) Eligibility criteria (pg. 4 and Table A.1)

Specify study characteristics (e.g., PICOS, length of follow-up) and report characteristics (e.g., years considered, language, publication status) used as criteria for eligibility, giving rationale.

- Study characteristics are provided in Table A.1. The rationale for our eligibility criteria is as follows: (1) *published in 1990 or later* – given advances made in quantitative social science, we restrict our analysis to post-1990; (2) *published in a peer-reviewed social science or business journal or by a university press* – the peer review filter serves as a quality control; (3) *examines the relationship between conflict and foreign investment* – this filter ensures that the papers in our review focus on the same IV and DV we evaluate; and (4) *includes a point estimate* – a point estimate restricts the papers to quantitative social science research and serves as the basis on which we code the effect direction.

(7) Information source (p. 4)

Describe all information sources (e.g., databases with dates of coverage, contact with study authors to identify additional studies) in the search and date last searched.

- We use Google Scholar to compile a database of articles. Our Google Scholar search occurred on September 11, 2018, where we pulled 950 articles that met our keyword criteria. We also included three additional relevant articles. We then conduct a "spider" search in Google Scholar of the articles that made it through all pre-specified inclusion filters. This means that we compiled and assessed all studies that cite any of the articles from the first search (42 total). We conducted the "spider" search from February 9 to February 11, 2019 and on January 17, 2020.

(8) Search (p. 4)

Present full electronic search strategy for at least one database, including any limits used, such that it could be repeated.

- Our search takes the form: (conflict OR violence OR coups OR revolutions OR assassinations OR political risk OR war OR political instability) AND (investment OR firms).

(9) Study selection (p. 4)

State the process for selecting studies (i.e., screening, eligibility, included in systematic review, and, if applicable, included in the meta-analysis).

- Prior to evaluating the full list of articles from Google Scholar, we specify eligibility criteria for inclusion in the systematic review (see item #6). We then employ a combination of automated review (e.g., the correct years) and manual review to implement the remainder of the filtering process.

(10) Data collection process (p. 4)

Describe method of data extraction from reports (e.g., piloted forms, independently, in duplicate) and any processes for obtaining and confirming data from investigators.

- We extract the sign and statistical significance of the coefficient on conflict/instability in regressions where investment is the dependent variable. When a study reports more than one relevant model, we favor the instrumental variable or fixed effects model results if available, given stronger claims of causal identification. However, for one article, we report results from multiple models due to slightly different dependent variables. These are noted in Table A.1. We then review each article to identify other relevant characteristics of the studies, which we present in Table A.1. We do not collect original data from these papers.

(11) Data items (Table A.1)

List and define all variables for which data were sought (e.g., PICOS, funding sources) and any assumptions and simplifications made.

- We code the following features of each article: effect direction and significance, use of fixed effects, use of instrumental variables, unit of analysis, year range, use of composite risk score, number of countries, region of focus, authors, journal, and year of publication.

(12) Risk of bias in individual studies (Table A.1)

Describe methods used for assessing risk of bias of individual studies (including specification of whether this was done at the study or outcome level), and how this information is to be used in any data synthesis.

- Where applicable, we note which studies utilize a fixed effects or an instrumental variable research design. In these cases, we report the effect directions with statistical significance presented in those models. If a study does not employ FE or IV, then we focus on their primary/preferred specification. We also exclude non-peer reviewed working papers from our review. In our summary Table 1, we provide details on how many studies use either fixed effects or instrumental variables, organized by effect direction.

(13) Summary measures (p. 4-5 and Table A.1)

State the principal summary measures (e.g., risk ratio, difference in means).

- The principle summary measure is the coefficient sign and statistical significance (significantly positive, unable to reject the null, and significantly negative) for variables that measure conflict or instability in regressions where the dependent variable is investment. We only extract coefficients that evaluate the effect of conflict/instability on investment.

(14) Synthesis of results (N/A)

Describe the methods of handling data and combining results of studies, if done, including measures of consistency (e.g., I^2) for each meta-analysis.

(15) Risk of bias across studies (N/A)

Specify any assessment of risk of bias that may affect the cumulative evidence (e.g., publication bias, selective reporting within studies).

(16) Additional analyses (N/A)

Describe methods of additional analyses (e.g., sensitivity or subgroup analyses, meta-regression), if done, indicating which were pre-specified.

(17) Study selection (pg. 4 and Figure A.15)

Give numbers of studies screened, assessed for eligibility, and included in the review, with reasons for exclusions at each stage, ideally with a flow diagram.

- See the Flow Diagram presented in Figure A.15. We exclude studies at each stage because they fail to satisfy the inclusion criteria outlined in advance of the systematic review.

(18) Study characteristics (Table A.1)

For each study, present characteristics for which data were extracted (e.g., study size, PICOS, follow-up period) and provide the citations.

- We provide select study characteristics to demonstrate the geographic scope and temporal focus of this body of research.

(19) Risk of bias within studies (Table A.1)

Present data on risk of bias of each study and, if available, any outcome level assessment (see item 12).

- We note which studies we consider less prone to bias based on their use of a fixed effects or an instrumental variable design (Table A.1).

(20) Results of individual studies (Table A.1)

For all outcomes considered (benefits or harms), present, for each study: (a) simple summary data for each intervention group (b) effect estimates and confidence intervals, ideally with a forest plot.

- We report three types of effect directions (significantly positive, unable to reject the null, significantly negative) from the main models in each individual study.

(21) Synthesis of results (N/A)

Present results of each meta-analysis done, including confidence intervals and measures of consistency.

(22) Risk of bias across studies (N/A)

Present results of any assessment of risk of bias across studies (see Item 15).

(23) Additional analyses (N/A)

Give results of additional analyses, if done (e.g., sensitivity or subgroup analyses, meta-regression [see Item 16])

(24) Summary of evidence (Table 1)

Summarize the main findings including the strength of evidence for each main outcome; consider their relevance to key groups (e.g., healthcare providers, users, and policy makers).

- See Table 1.

(25) Limitations (pg. 5)

Discuss limitations at study and outcome level (e.g., risk of bias), and at review-level (e.g., incomplete retrieval of identified research, reporting bias).

- See text. Google Scholar restricts our first search to the top 950 articles. As a result, we did a second Google Scholar search to collect all the papers that cited articles from the first search that met our filter requirements.

(26) Conclusions (pg. 5)

Provide a general interpretation of the results in the context of other evidence, and implications for future research.

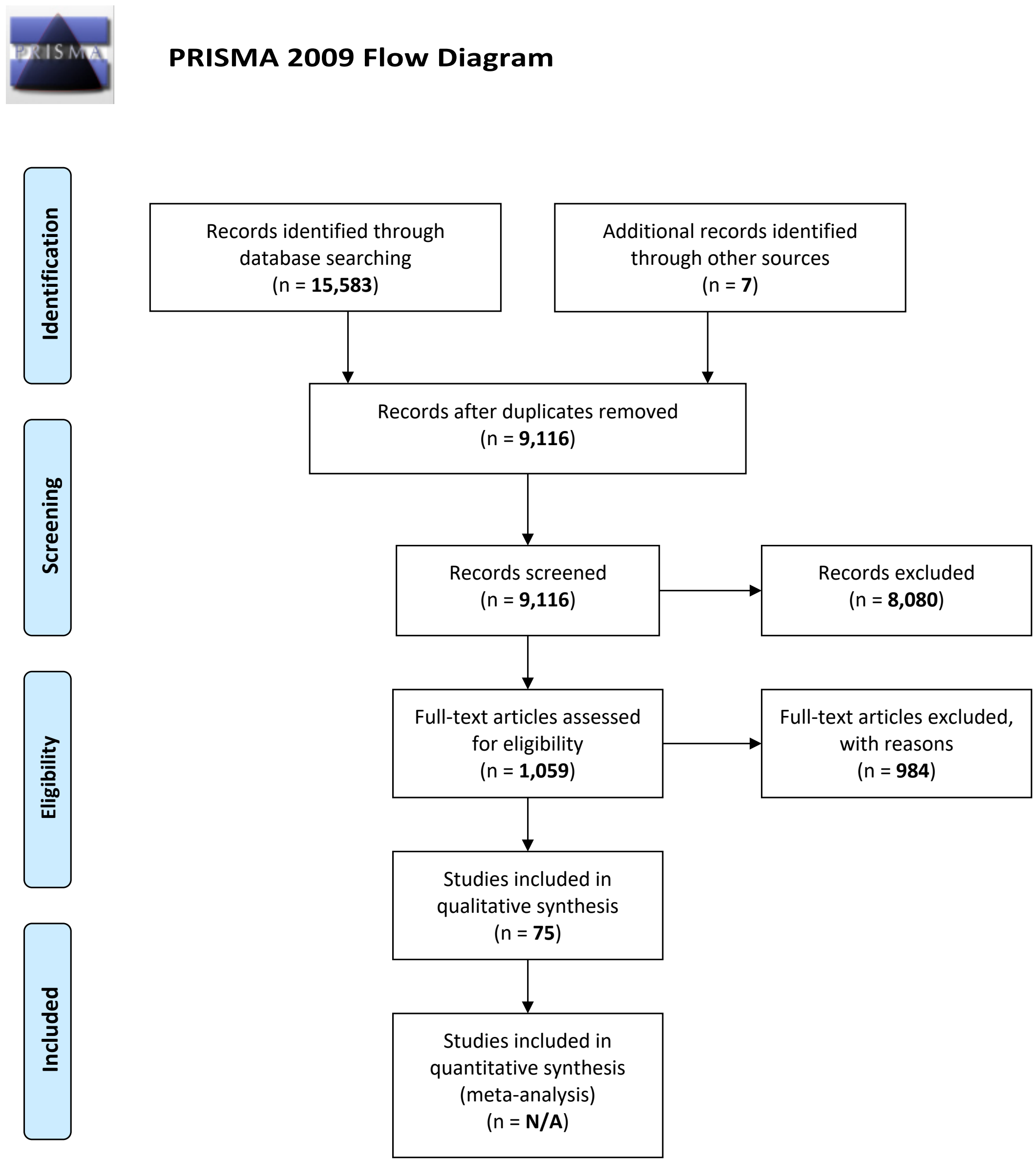
- Results form this country-level research motivate our focus on the firm-level.

(27) Funding (N/A)

Describe sources of funding for the systematic review and other support (e.g., supply of data); role of funders for the systematic review.

- We did not receive any funding to conduct this systematic review.

Figure A.15: PRISMA Flow Diagram



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