Double-Edged Sword: The Hidden Civilian Toll of FDI*

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Word count: 9,055

Abstract

Conflict-affected countries attract over a fifth of global FDI, yet we know little about the political consequences of these capital flows. We argue that inflow of immobile FDI—such as extractive or industrial projects—reshapes warring parties' incentives and behavior. FDI generates rents that governments seek to secure through territorial control, amplifying civilian repression around investment sites. Rebels respond to growing territorial competition by escalating anti-civilian violence to undermine state authority. Using geocoded data on FDI and conflict across Africa (2003–2019), we address endogeneity concerns by comparing conflict areas with investments to those with future investments. Conflict events within 5 km of extractive and industrial FDI sites see 33–37% more civilian casualties, with no effect for services FDI. These findings reveal a troubling dynamic of private capital in fragile states: FDI can transform local power struggles with lasting consequences for civilian protection, conflict trajectories, and state-building.

Keywords: Foreign direct investment; globalization; conflict; civil war; spatial analysis

*Authors are listed in alphabetic order. The project was generously funded through a Research grant from the UCL Department of Political Science (Project number 502147). We are thankful to Inken von Borzyskowski, Tolga Sinmazdemir, Abbey Steele, Sigrid Weber, Blair Welsh, and Alon Yakter for valuable feedback on the project. An earlier draft of this research was presented at EPSA 2021, MPSA 2022, and at the UCL Conflict and Change 2022 workshop.

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1 Introduction

Foreign direct investment (FDI) is largely seen as an important driver of economic development and a force for political stability. With developing countries acquiring about half or more of all investments made in the last years, FDI is one of the most crucial sources of capital formation in the developing world.[1] Somehow less acknowledged, however, is the fact that some of the world's most violent conflict zones attract billions in foreign capital every year. According to UNCTAD data, countries in civil conflict (such as Nigeria, the DRC, Mozambique, or Colombia) have received a yearly median of 22% of global FDI since 2000.[2] What are the political consequences when FDI flows into unstable contexts, such as war zones?

Answering this question has important implications for our understanding of how foreign actors like multinational corporations (MNCs) shape politics and affect political stability. Policymakers tend to see FDI as beneficial for growth and stability, increasingly also promoting FDI into fragile countries due to growing funding gaps.[3] Scholars have long studied the effects of FDI on domestic politics, pointing to an ambivalent relationship. FDI often has heterogeneous effects on various outcomes (such as corruption, labor standards, and institutional quality), depending on host-country conditions and sectoral characteristics (Brazys et al. 2023; Malesky 2008; Malesky et al. 2015; Malesky and Mosley 2018; Pinto and Zhu 2016; Sandholtz and Gray 2003; Zhu 2017). Journalists and human rights advocates, in turn, often suggest that FDI fuels conflict, even accusing foreign investors to be complicit in violence.[4] But systematic evidence linking FDI to changing conflict dynamics remains absent. Prior research examines whether FDI promotes peace through economic interdependence or fuels instability through uneven distribution of costs and benefits (e.g., Barbieri and Reuveny 2005; Bussmann and Schneider 2007; Hartzell et al. 2010; Pinto and Zhu 2022; Tomashevskiy 2017). Yet these studies rely on aggregated national-level data and largely overlook that FDI frequently flows into active conflict zones, offering limited insight into micro-level dynamics of investment and violence.

We develop a disaggregated framework to study how FDI flows to conflict zones affects patterns of violence. We argue that inflows of immobile FDI (such as extractive or industrial projects) reshape the behavior and incentives of warring parties. Immobile FDI creates expectations of long-term economic rents for host governments (Zhu 2017). To secure these rents, governments reduce the political risks for investors by concentrating efforts to assert territorial control around FDI projects in conflict zones (Blair et al. 2022; Jensen et al. 2012; Pinto and Zhu 2022). Greater territorial competition with armed groups, however, increases anti-civilian violence at sites of investment by both warring parties.

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[1] UNCTAD, *World Investment Report 2025*. https://unctad.org/publication/world-investment-report-2025.

[2] Author's own calculation based on UNCTAD official data and UCDP statistics. See Figure A.5.

[3] World Bank, 2025,https://www.worldbank.org/en/news/video/2025/06/27/global-economic-prospects-foreign-direct-investment-fragile-conflict-settings-expert-answers.

[4] Amnesty International, *Investigate Shell for complicity in murder, rape and torture*. https://www.amnesty.org/en/latest/press-release/2017/11/investigate-shell-for-complicity-in-murder-rape-and-torture; David Smith, "Shell accused of fuelling violence in Nigeria by paying rival militant gangs." *The Guardian*. October 3, 2011. https://www.theguardian.com/world/2011/oct/03/shell-accused-of-fuelling-nigeria-conflict

Government forces increase civilian targeting near investment sites to eliminate or deter rebel support and secure territorial control, especially when they lack other means to ensure compliance and face limited accountability (e.g., Kalyvas 2006; Lyall 2009; Schwartz and Straus 2018; Stanton 2016; Wood 2010). At the same time, increased government presence in investment areas undermines rebel influence. This boosts the relative value of escalating civilian targeting close to investment sites for multiple reasons such as challenging governmental control, limiting government revenues, signaling strength and resolve, limiting civilian defection, or even mobilizing support for their cause (e.g., Asal et al. 2019; Polo and González 2020; Wood 2010, 2014b; Wood and Kathman 2015). We expect this dynamic especially when FDI is geographically immobile, meaning it is tied to a fixed location through sunk costs and physical infrastructure (as is typical for extractive industries or large-scale manufacturing, less so in services), which raises the government's willingness to fight over associated territory (Mesquita 2020).

We can see such dynamics play out in practice. To sustain foreign capital inflows, governments often promise foreign investors protection at investment sites, which affects warring strategies of involved parties. In Mozambique, TotalEnergies signed a security pact with the government to protect a $20 billion liquefied natural gas (LNG) project.[5] Similarly, since 2009 the Nigerian government hired thousands of former fighters to protect pipelines owned by multinational companies operating in the Niger Delta and has, at times, even diverted troops from the front lines with Boko Haram.[6] Consequent upticks in civilian casualties have been reported in both cases.[7]

We systematically test our argument combining geolocated data on FDI projects in Africa from 2003 to 2021 with data on conflict events from the Uppsala Conflict Data Program Georeferenced Event Dataset (UCDP GED). Our main empirical strategy removes endogeneity in FDI location by exploiting variation in timing and location of investment projects, following Knutsen et al. (2017). We compare the number of civilian casualties in conflict events occuring close to an existing foreign investment to those in areas that will experience an FDI in the future. This design rules out unobservable factors affecting FDI location choices by foreign investors, which might correlate with conflict characteristics.

We find that immobile FDI has a sizeable, localized effect on civilian casualties: Conflict events within 5 km of an existing extractive FDI see on average over 33% more civilian casualties than events around future extractive FDI sites. The effect is similar (37% increase) for industry FDI but insignificant for FDI in services. In additional analyses, which use Armed Conflict Location & Events Data (ACLED), we find that warring parties increase civilian victimization in different ways. Whereas rebels increase violence at the intensive margin, i.e. stage *deadlier attacks* against civilians around FDI sites, government forces increase violence at the extensive margin, i.e., perpetrate a *higher number of attacks* against civilians. We suggest that this indicates that

[5] TotalEnergies, *Total signs agreement with the Government of Mozambique regarding the security of Mozambique LNG project*. https://shar.es/agspGR.

[6] Drew Hinshaw, "'Niger Delta Avengers' Sabotage Oil Output." *The Wall Street Journal*. June 5, 2016. https://www.wsj.com/articles/niger-delta-avengers-sabotage-oil-output-1465165361.

[7] Samuel Tife, "Civilians flee army raids in Nigerian oil delta." *Reuters*. December 3, 2010. https://www.reuters.com/article/idUSLDE6B21NB/; Amnesty International. *"What I saw is death": War crimes in Mozambique's forgotten cape*. https://www.amnesty.org/en/documents/afr41/3545/2021/en/

rebels turn to terrorist attacks in face of heightened territorial competition close to FDI while the government relies more on civilian repression to secure control over FDI sites.

This article makes three important contributions to research in international political economy, conflict studies, and development. First, it extends scholarship on the domestic effects of FDI by examining how FDI inflows shape violence in conflict-affected settings. Prior work has shown that FDI influences institutional outcomes such as the rule of law, corruption and labor standards, though often with heterogeneous effects (Ahlquist 2006; Brazys and Kotsadam 2020; De Soysa and Oneal 1999; Malesky 2008; Malesky et al. 2015; Malesky and Mosley 2018; Pinto and Zhu 2016; Sandholtz and Gray 2003; Zhu 2017). We push this literature further by showing that FDI can also transform security dynamics: when FDI is geographically immobile (such as in extraction or manufacturing) it increases territorial competition and contributes to systematic increases in violence against civilians.

Second, we contribute to debates on globalization and political (in)stability. Whereas prior studies typically examine the relationship between FDI and the onset or prevalence of civil war at the national level, yielding conflicting results regarding whether foreign capital deters or fuels instability (Barbieri and Reuveny 2005; Bussmann and Schneider 2007; Hartzell et al. 2010; Mihalache-O'Keef 2018; Pinto and Zhu 2022; Tomashevskiy 2017), we move beyond binary conflict outcomes to explore how FDI in conflict zones affects how violence is deployed in ongoing wars. Our disaggregated framework reveals how external economic forces can reshape the incentives and capabilities of both governments and rebels at the subnational level, contributing to civilian targeting.

Third, these findings speak to an underexplored intersection between the political economy of development and the microdynamics of armed conflict. They highlight how global economic integration in contexts of weak institutions and active insurgency can have unintended consequences that undermine peacebuilding and civilian protection. We thus add to a growing literature on the drivers of civilian victimization and strategic violence in civil conflict (Balcells and Kalyvas 2014; De la Calle 2017; Eck and Hultman 2007; Fielding and Shortland 2012; Hultman 2007; Polo and González 2020; Schwartz and Straus 2018; Stanton 2016; Wood 2010, 2014a).

Our evidence also informs critical policy debates on the effects of FDI on development—especially at a time where we see renewed debate on aid effectiveness and a call for countries to acquire private financing to advance development goals. Undeniably, foreign capital is a major source of financing and revenue generation in the developing world, at times shaping economic and political development positively. However, we show that FDI into conflict areas comes at an unrecognized cost for civilians. This finding is worrisome, especially when such unintended effects of FDI are not considered by foreign investors or host governments but likely grow humanitarian needs and limit prospects for peace and development.

2 FDI and Patterns of Violence: A Localized Framework

A substantial body of literature in political science and economics investigates the consequences of global economic integration, often measured as trade or FDI. Scholars have drawn links to outcomes such as corruption, economic growth, democratization, and political stability, finding both positive and negative effects (e.g., Ahlquist 2006; De Soysa and Oneal 1999; Kosack and Tobin 2006; Malesky 2008; Malesky et al. 2015; Pinto and Zhu 2016).

A core premise is that FDI shapes domestic affairs by prompting governments to adjust policies in response to investors' sensitivity to political risk. Yet it remains unclear how governments respond to such pressures when FDI flows into conflict zones. As a long-term commitment, FDI requires investors to weigh potential long-run risks against benefits prior to project initiation (Jensen et al. 2012). But despite sunk costs, multinational companies can credibly threaten to disinvest if risks become too great, for example, due to escalating violence. This threat prompts governments to internalize the potential costs of disinvestment when making high-stakes decisions, such as how to allocate scarce resources to counterinsurgency.

Our argument relates to but also diverges from existing work on the political consequences of foreign investment. A long-standing literature in international political economy suggests that investors sensitivity to political risk incites a "race to the top" (Vogel 1997). For instance, governments will adopt better governance standards to retain or attract investments, such as rule-of-law promotion, property rights protection, or corruption control (e.g., De Soysa and Neumayer 2005; Malesky 2008; Sandholtz and Gray 2003). Similarly, the "capitalist peace" thesis posits that economic interdependence (often measured as FDI inflows) reduces conflict risk by raising the opportunity costs of violence and increasing incentives for peaceful dispute resolution (Barbieri and Reuveny 2005; Bussmann 2010; Gartzke et al. 2001). Yet, the effects of FDI on political outcomes are likely neither uniform nor obvious, particularly in conflict-affected countries.

Scholars have questioned the assumption that foreign investors are consistently deterred by conflict or that their preferences necessarily align with liberal peacebuilding goals. FDI in conflict settings, particularly in extractive sectors, generates significant rents and may benefit from weakened state oversight or subdued local resistance (Barry 2018; Billon 2001; Guidolin and La Ferrara 2007; Maher 2015). Moreover, sunk costs reshape investor behavior: once a project is underway, companies often tolerate high levels of insecurity, especially when violence occurs away from investments (Blair et al. 2022; Chen 2017; Mihalache-O'Keef and Vashchilko 2010). In such cases, disinvestment is less likely and in fact, investments are found to increase if ongoing violence occurs elsewhere in the country (Dai et al. 2017).

Our argument builds on and moves beyond existing findings by emphasizing the conditional and localized nature of investor influence in conflict zones. We argue that foreign capital can prompt governments to internalize the potential costs of disinvestment. In particular, we propose a localized effect and explore how governments respond in contexts where violence is proximate to investment sites and where the financial or strategic value of FDI is high (see also Barry 2018; Blair et al. 2022). Rather than assuming that FDI encourages pacification, we posit that expectations of future rents lead governments to prioritize short-term stability around FDI locations through coercive means at the expense of broader peace building, thus shaping *local* conflict dynamics in critical ways.

An example is offered by the province of Cabo Delgado in Mozambique, which has experienced an insurgency since 2017 but has also been the recipient of substantial foreign investments. Most prominently, in 2019 TotalEnergies started a $20 billion LNG project. To protect it against attacks by the rebel group Islamic State Mozambique (ISM)—previously also known as Ahlu-Sunna Wal-Jama'a (ASWJ) or Al-Shabaab—the government increased military pressures. Initially, the government contracted private military companies (first the Wagner Group, then Dyck), who departed by 2021 after incurring significant battlefield losses. Since then, the government has received extensive military assistance, training, and aid from various actors including Rwanda, the Southern African Development Community (SADC), the European Union, and the United States (notably after rising insurgent attacks led to a halt in construction of the LNG project in 2021).[8] Although military operations have made substantial territorial gains, the insurgency is yet to be defeated and areas close to investment sites record an immense civilian death toll, attributed to the government and the rebel group.[9] In the following, we explain why the inflow of FDI into conflict zones prompts warring parties to increase the use of civilian targeting in areas of investment.

2.1 Government Rationale

FDI often yields significant and durable revenues for host governments. Foreign corporations can mobilize larger capital than domestic firms (Wang and Wang 2015) and are less vulnerable to fragile domestic economic conditions (Chen 2011; Gubbi et al. 2010). We propose that FDI flows into conflict zones therefore motivates governments to reconfigure their counterinsurgency strategy to ease investor concerns and secure rents in the long term.

Governments that are engaged in armed conflict are often limited in their repertoire of actions to achieve peace due to important resource constraints such as military capability or fiscal capacity as well as the complex reality of what it means to achieve peace or broader stability. Military victory is often unlikely or extremely costly, especially if increased spending on defense undermines capacity for other important matters (such as social services) and may imperil political survival (Mukherjee 2014; Sexton et al. 2019). Similarly, settlement of hostilities can take years or decades of negotiations and may not hold in the long run (e.g., Matanock 2020). To ease investor concerns, governments are instead more likely to reallocate military resources toward zones hosting foreign investment projects, to assert control over these areas if under conflict and protect these assets.

Foreign investors, themselves, frequently request enhanced state protection, even when entering already conflict-affected areas (Rexer 2021). Governments often comply, deploying additional troops and at times diverting them away from other active fronts. For instance, in Nigeria the army has repeatedly diverted troops from the front against Boko Haram to secure oil infrastructure amidst militant threats of attacks.[10] In Mozambique, oil companies like Exxonmobil and TotalEnergies reportedly explicitly requested troops be deployed to the area of their investment

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[8] Sudarsan Raghavan. "ISIS fighters terrorize Mozambique, threaten gas supply amid Ukraine war." *The Washington Post*. October 20, 2022. https://www.washingtonpost.com/world/2022/10/18/mozambique-isis-cabo-delgado-gas/; see also the European Union Training Mission in Mozambique page: https://www.eeas.europa.eu/eutm-mozambique/about-european-union-training-mission-mozambique_en?s=4411.

[9] For more details, see International Crisis Group. *Winning Peace in Mozambique's Embattled North*. February 10, 2022. https://www.crisisgroup.org/africa/southern-africa/mozambique/winning-peace-mozambiques-embattled-north and Amnesty International. *"What I saw is death": War crimes in Mozambique's forgotten cape*. March 2, 2021. https://www.amnesty.org/en/documents/afr41/3545/2021/en/.

[10] Drew Hinshaw, "'Niger Delta Avengers' Sabotage Oil Output." *The Wall Street Journal*. June 5, 2016. https://www.wsj.com/articles/niger-delta-avengers-sabotage-oil-output-1465165361.

in Cabo Delgado after militant attacks escalated.[11] Such changes in military strategy do not only reduce the risks associated with violent confrontation *for foreign investors*, they also have important consequences for patterns of violence observed at sites of investment.

Specifically, we expect the inflow of FDI to areas already embroiled in conflict to cause a localized increase in civilian victimization as government forces aim to secure territorial control. In this context, civilian victimization is a strategic choice used to weaken rebel support, punish collaboration, or deter future cooperation with insurgents when intelligence is lacking or time pressure is high (Fielding and Shortland 2012; Kalyvas 2006; Lyall 2009; Schwartz and Straus 2018). Governments often also lack the capacity or resources to pursue alternative strategies to elicit voluntary cooperation from civilians and secure territorial control, such as service provision or long-term development, further amplifying reliance on civilian targeting (Berman et al. 2011; Wood 2010).

Violence against civilians is not, however, intended to systematically kill a certain (identity) group. Reasons to perpetrate such type of violence deviate from those motivating anti-civilian violence in areas of investment, namely to secure control over FDI sites (for a review, see Valentino 2014).[12] Instead, we expect that the government will engage more heavily in violence against civilians to secure investment sites driven by a logic of strategic repression, to advance its interests while minimizing potential backlash for targeting civilians too heavily (Stanton 2016). And because repression does not have to be lethal to be effective (for instance arrests, torture or other forms of human rights abuses are commonly used to enforce control and discourage dissent) we expect to observe more *frequent* violence against civilians by government forces (or government-aligned militias) close to FDI, compared to other active conflict areas (Davenport 2007; DeMeritt 2016)

2.2 Rebel Rationale

Greater government presence and coercive capacity (e.g. larger deployment of security forces) near FDI projects tilts the local power balance in favor of the state and enhances the *quality of territorial competition*, with important consequences for rebel group's warring strategy. As we argued above, inward FDI affects where and how strongly the state projects authority, and likely expands the financial and logistical resources it can deploy to support its counterinsurgency strategy. This shift effectively reduces the rebels' capacity relative to the state. Yet, the strategic and symbolic value of investment locations motivates rebels to continue contesting the state even as the balance of power shifts.

Rebels respond to such differences in military power and a shifting battlefield environment by using different violent tactics, namely increased civilian targeting (e.g., Polo and Gleditsch 2016; Polo and González 2020). Although civilian targeting can be costly for rebels, who often depend on local support to retain territorial control, the inflow of FDI alters their calculus. Violence against civilians becomes a way to undermine the state's capacity to secure valuable territory (and associated rents), impose reputational and political costs, and retain a disruptive

[11] Kudzai Chimhangwa, "War in Mozambique: A Natural Gas Blessing, Turned Curse." *Open Democracy*. June 26, 2020. https://www.opendemocracy.net/en/oureconomy/war-mozambique-natural-gas-blessing-turned-curse/.

[12] Existing explanations emphasize ethnic ties to the opponent or a strong ideological/political allegiance as motivation for victimization (Balcells and Steele 2016; Steele 2011; Valentino et al. 2004).

presence while minimizing increasingly costly direct confrontation. This shift in rebel tactics typically involves reorganization into smaller, more mobile, and lightly armed groups that can blend into civilian populations (Kalyvas and Balcells 2010). Rather than prolonged battles, rebels resort to ambushes, town raids, and terrorist attacks; and specifically intensified civilian victimization (Balcells and Kalyvas 2014; Kalyvas and Balcells 2010).

By heightening violence against civilians, rebels impose costs on the government and effectively limit its capacity to control the area, while often also threatening and even limiting expected revenues from FDI investments. In that regard, rebels are able to leverage civilian vulnerability because controlling FDI sites is important to the government (and often also associated international actors). For instance, their ability to perpetrate such violence demonstrates that counter-insurgents cannot or will not credibly protect the population, reducing possible defection to the state and inducing compliance (Fielding and Shortland 2012; Hirose et al. 2017; Wood and Kathman 2015). Rebels may also use terrorist attacks (often targeting civilians) to incite a violent government response against the population and help stem erosion in support among their core constituency, potentially even mobilizing new supporters (Polo and González 2020).

These dynamics constitute a "weapon of the weak," particularly salient when territorial competition increases. But it is not necessarily weak rebel groups (at the conflict level) who engage in violence against civilians. Such actions are often strategic and vary geographically (e.g., Asal et al. 2019; Stanton 2016; Welsh 2023). Rebels are more likely to heavily target civilians (or engage in terrorism) as they face increased territorial competition, have lost territory, or incurred substantial battlefield losses (Hultman 2007; Polo and González 2020; Wood 2010). This is amplified when rebels cannot offer other incentives to induce civilian compliance, which is very likely the case close to FDI projects where the government is willing to invest substantially more resources than elsewhere to compete for territorial control and civilian support, which in turn further limits their "social embeddedness," reinforcing reliance on civilian targeting (Wood 2010, 2014b; Wood et al. 2012).

Although rebels still risk political costs—such as alienating targeted communities or civilians elsewhere (e.g., Fortna 2015)—continuing to challenge the state *at investment sites* is extremely valuable from a reputational perspective. First, by continuing to fight the state, rebels can signal strength and resolve, including to the government itself (Asal et al. 2019). Second, FDI projects often do not (immediately or ever) translate into development gains for local communities and in fact often carry negative consequences such as forced relocation, pollution, or corruption in fragile settings (e.g. Malesky et al. 2015; Obi 2014; Zhu 2017). Simultaneously, stronger military efforts add to negative effects (or at least unmet expectations) of FDI for the population. This offers rebels the opportunity to use terrorism to advertise their cause to a larger audience or even mobilize or maintain support from aggrieved communities at the periphery of investment sites and intense conflict (Polo and González 2020; Wood 2014b). Third, if rebels were to retreat, they would stand to loose support among population groups which were already supporting their violent campaign against the state *prior* to the start of an FDI project.

The current conflict in the North of Mozambique shows these dynamics. Historical marginalization is at the root of the emergence of the insurgency (although other factors, such as strong organization and regional support networks, were critical).[13] The rebel group, Islamic State Mozambique (ISM), leverages feelings of injustice among parts of the population, amplified by the influx of FDI, to expand their operations to areas where counterinsurgency efforts have been less substantial (Hendricks et al. 2023). Despite significant investments into development projects in the North over recent years, the population still largely feels left behind in terms of economic and political development and analysts suggest that widespread marginalization maintains a critical basis for insurgents to continue to gain support despite substantial counterinsurgency efforts.[14] At the same time, rebels deliberately maintain a violent presence close to investment sites, often strategically escalating violence against civilians and perpetrating attacks to halt operations linked to LNG projects.[15]

Increased rebel violence against civilians near investment sites is thus best understood as a strategic response to the opportunities and constraints created by FDI. Just as the state escalates its presence to protect investments, rebels adjust by intensifying violence against civilians in ways that can impose costs, signal strength, and undermine state control. Alongside the local population, these attacks may also target infrastructure, company personnel, or civilians associated with the investment project. We expect this to manifest in heightened civilian targeting that resembles terrorist attacks, meaning highly visible attacks intended to signal strength, undermine state authority, and deter foreign investment—often resulting in a higher number of casualties, though not following a logic of mass killings or ethnic cleansing (Stanton 2016; Valentino et al. 2004).

2.3 Sectoral Variation

The impact of FDI inflow on conflict dynamics varies by sector. Extractive and industry FDI are more likely to create these patterns than services FDI because they are geographically immobile and capital-intensive. These investments are tied to fixed locations through sunk costs and physical infrastructure, as is typical for extractive industries or large-scale manufacturing, less so for services. This immobility raises governments' willingness to fight over associated territory and concentrate military resources around investment sites to secure future rents.

This creates dual pressures. First, large sunk costs make foreign investors most sensitive to violence (Barry 2018). Second, controlling infrastructure and facilities is critical for securing material benefits, which heightens host governments' willingness to fight over investment areas (Mesquita 2020; Pinto and Pinto 2008; Zhu 2017). We thus expect a differential effect of FDI by sector, as activities in the extraction (e.g., oil, gas, or mining) or industry sector (e.g., manufacturing or construction) are usually more immobile than those in services (e.g., business services, sales, marketing, etc). Such expectations are also in line with other literature on FDI, which has shown the benefit of differentiating between sectors as these can carry varying political risk and government responses (Malesky et al. 2015; Pandya 2016; Pinto and Pinto 2008).

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[13] Peter Bofin. "Actor Profiles: Islamic State Mozambique (ISM)." *ACLED*. October 30, 2023. https://acleddata.com/2023/10/30/actor-profile-islamic-state-mozambique-ism/.

[14] "Special Report on Five Years of Conflict in Northern Mozambique." *Cabo Ligado Monthly*. November 23, 2022. https://www.caboligado.com/monthly-reports/cabo-ligado-monthly-october-2022.

[15] Johnston, Ian, Mark, Monica and Joseph Cotterill. "Total's risky bet on a natural gas megaproject in southern Africa", *Financial Times*. May 21, 2025. https://www.ft.com/content/c0629936-79c8-4528-9b00-7456d042b6c9

Confirming their immobility, extraction and industry sectors are also more capital-intensive. Our FDI data show that the capital investment for extractive FDI projects averages at $374 million, industry FDI projects at $169 million, and service FDI at $16 million (see Figure 1 for averages by activity). This aggravates the dynamics our argument puts forward as substantial rents make governments more accountable to foreign investors than to domestic constituencies, increasing their willingness to accept the political cost of civilian victimization and dedicate substantial resources to capturing territory tied to these rents (Mesquita 2020; Stanton 2016). Impunity heightens when governments lack sufficient capacity to swiftly secure territorial control and engage paramilitary groups or private military contractors (such as the Wagner Group), which is common when investor pressure mounts. Their involvement typically escalates violence against civilians (Carey et al. 2015; Carey and Mitchell 2017; Koren 2017; Serwat et al. 2022).[16]

Immobile FDI located in conflict-affected areas may also prompt increased military assistance, training, and aid—especially from countries or regions of origin of the investment (Kentor et al. 2023). Although such assistance can potentially restrain violence against civilians, significant foreign support is rare and its effects are likely only felt in the long run (DeMeritt 2015; Stanton 2016). In fact, substantial resources and capacity would be required to elicit voluntary cooperation or reduce insurgent support (e.g. Berman et al. 2011; Fielding and Shortland 2012) and even if the government increases development efforts, this could further entice insurgent action (Crost et al. 2014). Governments thus remain committed to gaining control over FDI-linked territory and rely on repression and specifically civilian targeting (DeMeritt 2016) to enforce control over investment sites, which reinforces rebels' escalation of anti-civilian violence.

2.4 Hypotheses

From this argument, we derive three testable empirical expectations

> **Hypothesis 1:** Conflict areas in the proximity of immobile FDI see more violence against civilians.
>
> **Hypothesis 2:** The number of fatalities in one-sided violent events against civilians perpetrated by rebel forces increases in the proximity of immobile FDI, compared to other conflict-affected areas.
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> **Hypothesis 3:** The number of one-sided violent events against civilians perpetrated by government forces increases in the proximity of immobile FDI, compared to other conflict-affected areas.

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[16]Only militias emerging out of targeted communities are less likely to victimize civilians due to their access to intelligence and local embeddedness (Lyall 2010; Stanton 2016).

3 Empirical Framework

3.1 Data

We build a dataset containing geolocated information on inward FDI projects and conflict events. We gather data on 11,689 FDI projects across Africa between 2003 and 2021 from *fDi Markets*, which reports greenfield foreign investment projects. Because our argument estimates the effect of a new FDI project on conflict, we only keep the 10,610 new projects. We exclude closed ones and expansions of existing sites. We use information about the location of projects—country, administrative region, and locality (city, town, or village)—to create a Google Maps API query and geolocate these data points. The majority (7,511, or 71%) of the projects report complete location information and can be geolocated.[17] This is the final group of FDI projects we consider.[18] Figure 1 breaks down their percentage over the total and average capital investment by activity. Similarly to Mihalache-O'Keef (2018), we distinguish three broad sectors: extraction, industry, and services, corresponding to the primary, secondary, and tertiary sectors. Although we estimate results separately for each sector, we classify extractive and industry FDI as relatively *immobile*. Investments in these sectors are more bound geographically and more capital-intensive, in contrast to FDI in services.

For our main analysis we rely on the UCDP GED version 20.1 (Sundberg and Melander 2013) to collect information on conflict events. UCDP GED records 225,385 geolocated violent events between 1989 and 2019. To match our FDI data availability, we only retain events occurring in Africa after 2003. Further, we exclude from the dataset events of "non-state conflicts" (violence between organized armed groups). We therefore consider only "state-based conflicts" (those where at least one of the two parties is the government of a state) and "one-sided violence" (targeted violence against civilians).[19] These selections leave us with 22,480 violent events between 2003 and 2019 in 37 African countries. We measure, for each conflict event i in year t, *Civilian Deaths*: the logged number of reported civilian casualties (+1).

Different sources of conflict data adopt coding conventions which might impact empirical results differently. Raleigh et al. (2023) compare two popular sources, UCDP and ACLED, and conclude that UCDP tends to precisely code a narrower set of events (see also Eck 2012), allowing greater internal reliability. Because we study a large set of events across time and countries, internal consistency is crucial to us, making UCDP a favored choice. Moreover, UCDP allows us to test our hypotheses about the effect of FDI on the *number of civilian deaths*, a quantity not coded by ACLED. However, UCDP data can be ill-suited to study complex dynamics of political violence. For instance, they do not capture attacks perpetrated by state-aligned militias and are less likely to reflect non-lethal violence against civilians, which we expect to be an important part of governments' repertoires of violence. We thus also conduct additional analy-

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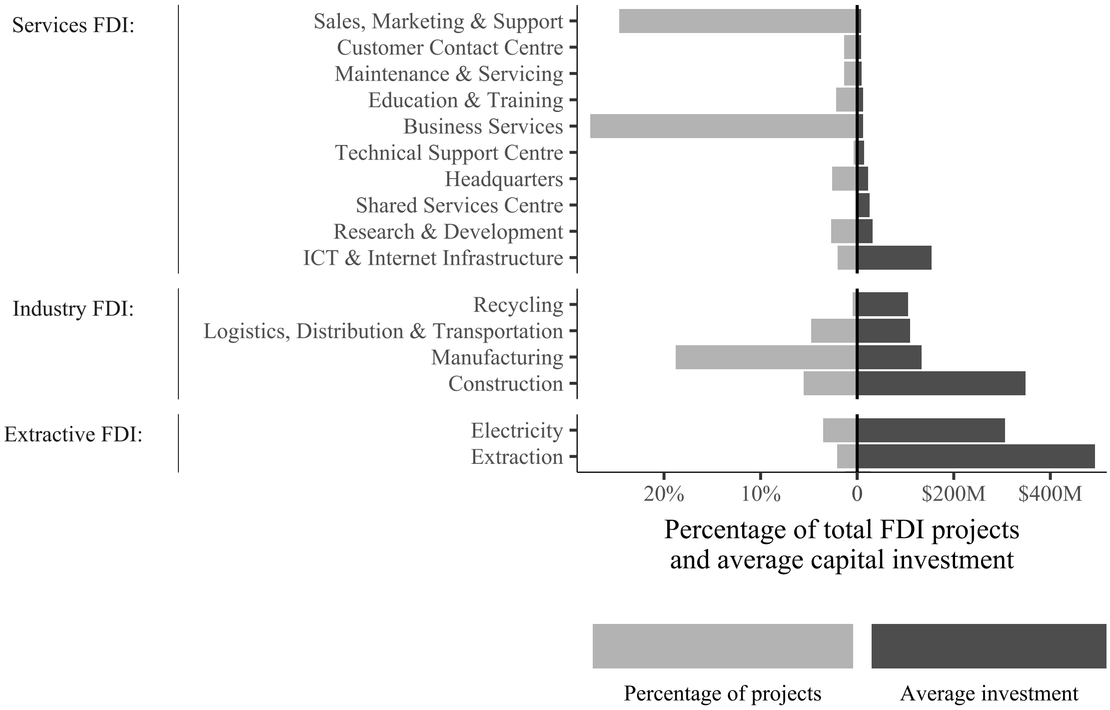
[17]In Supporting Information C we discuss possible selection bias in our estimates derived from not analyzing the remaining 29% of the FDI projects that do not report location information. We use observable covariates for all FDI projects to argue that selection bias likely leads us to *under*estimate the size of our target effect, which reassures us on the validity of the results.

[18]We cannot observe FDI projects before 2003, thus our analysis might suffer from left-truncation in treatment status. In Supporting Information B.2 we discuss why this is unlikely to threaten our inferences. We offer empirical tests to back our claim in E.3.

[19]Including also "non-state conflicts" does not significantly affect our results.

Figure 1: Distribution of FDI projects' activity by number (percentage of total) and average capital investment (millions of current US dollars)



ses using ACLED and measuring number of violent attacks against civilians—see Supporting Information (SI) G.

3.2 Research design

The non-random location of investment complicates a study of FDI's local effects. Foreign firms might decide (not) to invest in a certain area as a function of prospects of profit or stability. Because these factors likely correlate with conflict dynamics, observational studies risk erroneously attributing differences in conflict patterns to FDI, rather than to unobserved determinants of both. For instance, a negative association between presence of foreign investors and violence against civilians might mask investors' preferences for politically stable environments, which in turn might be associated with low-intensity violence.

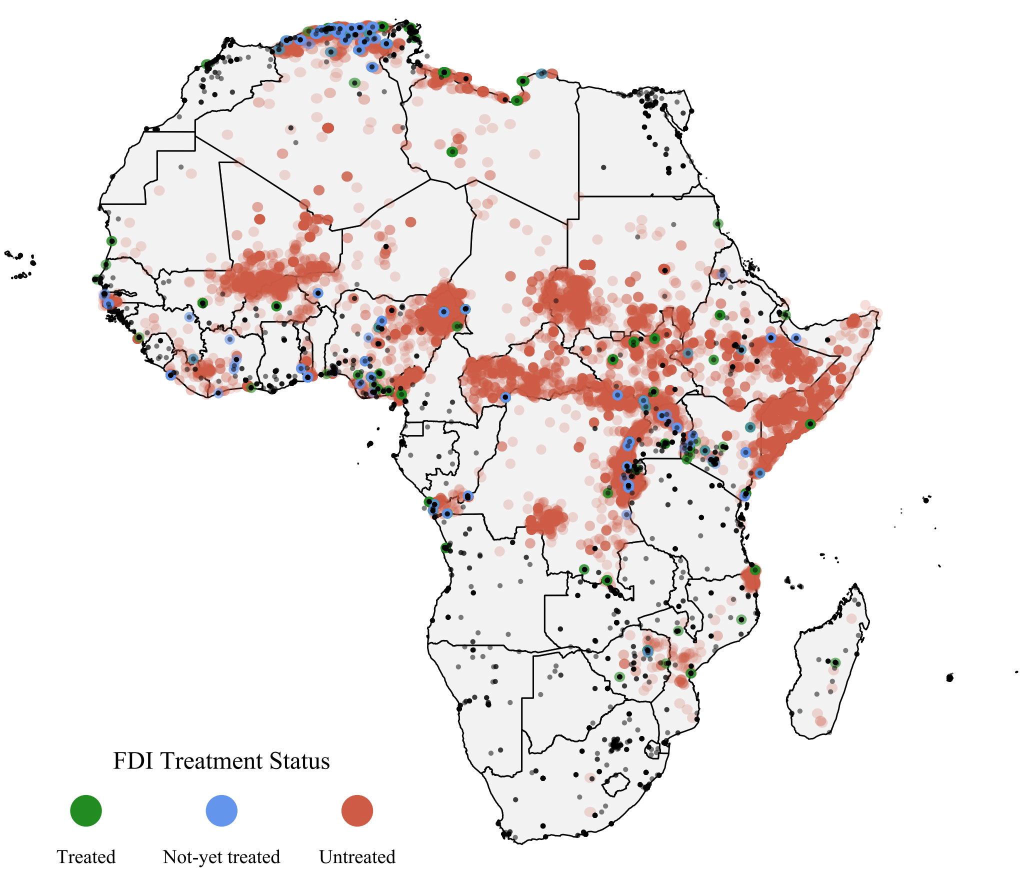
Our main identification strategy exploits spatial-temporal variation in the distribution of a "treatment" (an FDI project) to account for such selection bias. The design has been used to study local effects of mining (Knutsen et al. 2017; Kotsadam and Tolonen 2016), FDI (Brazys et al. 2023; Brazys and Kotsadam 2020; Rommel 2023), and foreign aid (Brazys and Jung 2024). In our application, the unit of analysis is a UCDP conflict event[20] i occurring in year t.

[20]Readers might be concerned that having conflict events as units selects on the dependent variable and prevents from studying how patterns of violence change as areas receive FDI. We acknowledge this point and, in SI G, we show that our results do not hinge on our unit choice: we obtain similar estimates when studying a global panel of African cell-years. However, we also argue that a precondition for measuring the number of civilian casualties is that a conflict event happens. Thus, conditioning on the occurrence of an event is a logical step given our interest.

We define a circular buffer of 5 km radius[21] around each conflict event and use geolocated FDI information to code each conflict event in one of three treatment groups: (1) conflict events within 5 km from at least one existing FDI project (*Treated*); (2) conflict events in no proximity of an existing FDI, but within 5 km of a future investment site (*Not-yet treated*); and (3) conflict events that are not within 5 km of an FDI project at any time point (*Untreated*). We estimate the localized effect of FDI on violence against civilians by comparing *Civilian Deaths* in treated and not-yet treated conflict events. We thus suppress a comparison between treated and untreated units, which is likely biased by factors determining selection into an FDI.

The design retrieves an unbiased estimate of the average treatment effect on the treated (ATT) conflict events under the assumption that, *absent the FDI*, attacks proximate to an investment would have had, on average, a similar number of civilian casualties as those in areas of future investment. We present this assumption formally in SI B, where we also defend its validity in our context. To support this assumption, SI D shows that the design removes significant observable differences in covariates that correlate with FDI and political violence.

Figure 2: FDI treatment status of UCDP GED violent events



Note: black dots represent geolocated FDI projects from fDi Markets. Circles represent buffer zones of 5 km radius defined around violent events reported from the UCDP GED. Circles are plotted with a 50 km radius in order to be visible, but their treatment status is defined based on a 5 km radius.

[21] In SI Figure E.1 we show similar results with alternative radius choices.

Figure 2 exemplifies our procedure. It plots all our conflict events, colored based on their treatment status. It also plots FDI projects as black dots.[22] Our comparison between treated (green) and not-yet treated (blue) conflict events removes important geographical differences. Consider Algeria. Here, foreign investment concentrates in the coastal Mediterranean area. This area likely differs from the southern region bordering Mali and Niger, which is affected by conflict but sees fewer FDI projects. Our design removes such differences by only comparing conflict events occurring in the proximity of present or future investments.

To strengthen the credibility of our identifying assumption, we compare only conflict events that are and will be treated by the same investment activity, be it extractive, industry, or services. This prevents us from comparing across investment types and using, say, events in areas of future services FDI as a counterfactual for those close to an existing extractive FDI.

We implement this design by estimating the linear model of *Civilian Deaths* in equation 1. In whichever version (extractive, industry, or services) the treatment is a three-level categorical indicator (for *treated*, *not-yet treated*, and *untreated*) using not-yet treated units as baseline, here represented as two binaries for *Treated* and *Untreated* units.[23] The ATT is estimated by β. Coefficient γ quantifies the selection bias that our design removes under the identifying assumption (it compares violence in areas that never see an FDI with those that will see an FDI in the future). We always include country and year fixed effects (FE) to remove time and country-invariant heterogeneity in FDI and political violence (α_c, δ_t). In SI, we show that our results are robust to making comparisons within narrower units with a grid-cell FE (Table E.1). All standard errors are clustered at the country level.[24]

$$CivilianDeaths_{ict} = \beta\, Treated_{it} + \gamma\, Untreated_{it} + \alpha_c + \delta_t + \varepsilon_{it} \tag{1}$$

In additional specifications, we control for local-level covariates that may affect decisions to invest and, simultaneously, patterns of violence—especially violence against civilians. Covariates are all drawn from PRIO-GRID data (Tollefsen et al. 2012), defined at the level of the 50×50 km cell each conflict event occurs in. Because PRIO-GRID covariates are not observed consistently over time we take, for each event occurring in cell p and year t, the latest observed value of a covariate in p before t. We control for the average proportion of mountainous terrain in the cell; for the number of politically excluded ethnic groups in a cell; for the percentage of the cell covered by urban area; and for the (logarithm of) population density.[25]

[22]Because the treatment status (and buffer color) is defined based on a 5 km radius but circles are represented with a 50 km radius in order to be visible, some buffers appear to be containing an investment (black dot) even though they are untreated (red).

[23]We thus depart from typical applications of the design which use *untreated* as a baseline, include dummies for treated and not-yet treated units, and estimate the ATT via an F-test on the difference of their coefficients (see Knutsen et al. 2017: 327). Our ATT estimates are numerically the same when we do that (not reported here) but we prefer this modification as it performs the comparison directly (without having to take a difference between coefficients), it estimates effect standard errors, and performs standard t-tests of hypotheses.

[24]In appendix Table E.2, we show results are robust to adopting Conley standard errors to account for spatial correlation between observations.

[25]Mountainous terrain is time-invariant. The number of excluded ethnic groups is observed until 2013, so we consider yearly values for events where $t < 2013$ and the 2013 value if $t \geq 2013$. The urban area is observed every decade so we take the 2000 value if $t \leq 2010$ and the 2010 value if $t > 2010$. Population density is measured every

Our approach is preferable over the alternative to divide African countries in arbitrary spatial units (e.g., grid-cells), observing them over time, and performing a panel data analysis. Such approach would introduce a *modifiable areal unit problem* where "data generated from [...] aggregate unit on any variable of interest [...] change as different border mappings are used" (Lee et al. 2025: 3). In our case, we would suffer both a "scale" and a "zoning" problem: Different arbitrary scale and zoning choices could cause different patterns of co-variation between treatment and outcome, introducing noise or, at worst, bias. Moreover, such arbitrary approach would be computationally expensive if performed at an equally fine-grained level of analysis as ours, given our vast geographical scope. We therefore maintain our research design as the preferable one. In Appendix G we show robustness to using this approach, anyway.

Although our approach is best suited to study the effect of FDI on the number of civilian casualties at a very fine-grained level, it does not allow us to test expectations about the effect of FDI on *number of events* of civilian victimization. Because our theory suggests potential differences in repertoires of violence between warring parties—i.e. increases in attacks (government) vs. deadlier attacks (rebels)—we also provide an additional analysis studying a panel of yearly PRIO-GRID cells, which allows us to use *number of attacks against civilians* as an alternative outcome. We present the approach in SI G.

4 Results

Table 1 reports our results. Models are divided in three groups depending on which treatment variable is included: extractive, industry, or services FDI. In all models, the first row quantifies the difference in *Civilian Deaths* between *Treated* and *Not-yet treated* conflict events for a given FDI type—our ATT estimate. For each FDI type, we first include only the treatment variable. The second model adds the four covariates. The final model adds a linear country-level time trend to account for country-specific temporal dynamics of FDI and conflict intensity.

Consistent with our expectation, we find that extractive and industry FDI (which is relatively immobile) significantly increases the number of civilian casualties in nearby conflict events; FDI in services elicits no such effect. Conflict events within 5 km of an extractive FDI experience an estimated[26] 33% higher number of *Civilian Deaths* than events in future extractive FDI sites (model 1). Estimates are still positive and sizeable when including covariates and time-trends (models 2 and 3). Similarly, FDI in industry increases the number of *Civilian Deaths* by 37% over the not-yet treated baseline (model 4), a result robust to the inclusion of covariates and linear trends (models 5 and 6). All these findings are distinguishable from zero at a 0.05 level of significance. We do not detect any effect for services FDI (models 7–9), whose estimates are small, noisy, and flip sign.

five years until 2010. We take the 2000 value if $t \leq 2005$, the 2005 value if $2005 < t \leq 2010$, and the 2010 value if $t > 2010$.

[26]Because these are log-linear models, we compute all percentage changes quantified by a $\hat{\beta}$ as $r = 100 \cdot (e^{\hat{\beta}} - 1)$.

Table 1: The local effect of FDI on civilian casualties

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.286***	0.238**	0.198**	0.312**	0.271*	0.224*	0.046	-0.011	-0.030
	(0.076)	(0.067)	(0.063)	(0.108)	(0.113)	(0.097)	(0.065)	(0.075)	(0.068)
Untreated *vs* Not-yet treated	0.054	0.090+	0.066*	0.171+	0.179+	0.136	0.066	0.085	0.052
	(0.042)	(0.044)	(0.031)	(0.090)	(0.102)	(0.087)	(0.064)	(0.073)	(0.067)
Mountainous terrain		-0.090	-0.093		-0.084	-0.087		-0.099	-0.100
		(0.089)	(0.099)		(0.093)	(0.103)		(0.092)	(0.103)
No. excluded ethnic groups		-0.052	-0.015		-0.050	-0.013		-0.055	-0.018
		(0.068)	(0.069)		(0.069)	(0.070)		(0.070)	(0.071)
Urban area		-0.023	-0.015		-0.024	-0.017		-0.018	-0.011
		(0.017)	(0.017)		(0.017)	(0.015)		(0.016)	(0.016)
Population density (log)		0.051*	0.035*		0.050*	0.034*		0.054*	0.039*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.129	0.152	0.133	0.130	0.153	0.132	0.129	0.152
R2 Adj.	0.131	0.126	0.148	0.131	0.127	0.148	0.130	0.126	0.148

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

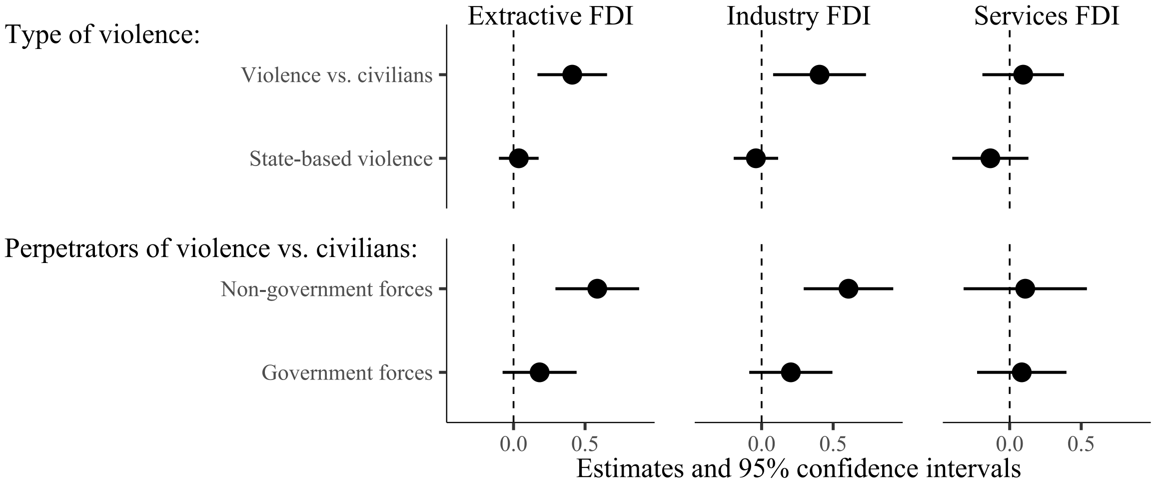
Notes: All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.

We show robustness of these findings in SI. Results are robust to accounting for spatial correlation through Conley standard errors (Table E.2), including PRIO-GRID cell FE (Table E.1), and to two tests designed to account for left truncation in the FDI data (Tables E.3 and E.4). Results do not hinge on the arbitrary 5 km distance from an FDI project (Figure E.1) nor on the number of future time-points that define the not-yet treated group (Tables E.5 and E.6). Estimates are robust to controlling for the number of jobs created by FDI projects, to account for employment-induced local immigration which might simultaneously increase civilian casualties in violent attacks (Table E.7). Finally, we show that our estimates do not hinge on the chosen research design nor on the choice of UCDP data: we find similar effects on number of civilian casualties and number of deliberate attacks against civilians with various FE models on a panel of yearly PRIO-GRID cells, using UCDP or ACLED data (SI G).

4.1 Repertoires of civilian victimization by perpetrator

Our theoretical argument suggests that both rebels and the government will target civilians in the proximity of FDI more heavily but they may intensify violence in different ways. To recall, we expect rebels to increase violence at the intensive margin, resorting to more brutal forms of terrorism and indiscriminate violence, i.e. *deadlier attacks* against civilians. Meanwhile, government forces will rely more on victimization at the extensive margin, increasing *number of attacks* against civilians, indicative of heightened repression, which is not necessarily lethal. We test these expectations here and report findings in full in SI F and G.

Figure 3: The local effect of FDI on the number of civilian casualties by type of attacks and perpetrators. Sub-group analysis of results from Table 1



Note: the top panel splits the sample between one-sided violence against civilians and state-based violence; the bottom splits the sample of one-sided violence against civilians between attacks staged by rebels and those staged by government forces. The outcome variable is always the reported logged number of civilian casualties in a violent attack (+1). Estimates from models that include country and year FE. Full results in models 1, 4, and 7 of Tables F.1 and F.2—top panel—and of Tables F.3 and F.4—bottom.

Figure 3 summarizes two subgroup analyses that subset data in models 1, 4, and 7 of Table 1 by type of attack and perpetrator. First, we split the sample among events of deliberate violence against civilians (those where non-combatants are deliberate targets) and state-based ones (where civilian casualties are collateral damage). Second, we further split deliberate attacks against civilians based on whether the perpetrators were state forces or not.[27]

Deliberate attacks against civilians in the proximity of an existing extractive or industry FDI result in about 50% more civilian casualties than those in future investment sites (top results). We find no significant effect for state-based violence, in line with our argument that suggests violence against civilians is *strategic*; that is, civilian deaths do not increase around immobile FDI sites as a collateral. As suggested by our theory we also find that only rebels increase the *intensity* of civilian victimization (bottom results). Meanwhile, the government does not engage in deadlier attacks close to FDI than elsewhere. Yet, null results for violence by government forces at the intensive margin could hide an effect at the extensive margin, in terms of the *number* of attacks against civilians—in line with a logic of increased repression to maintain control.

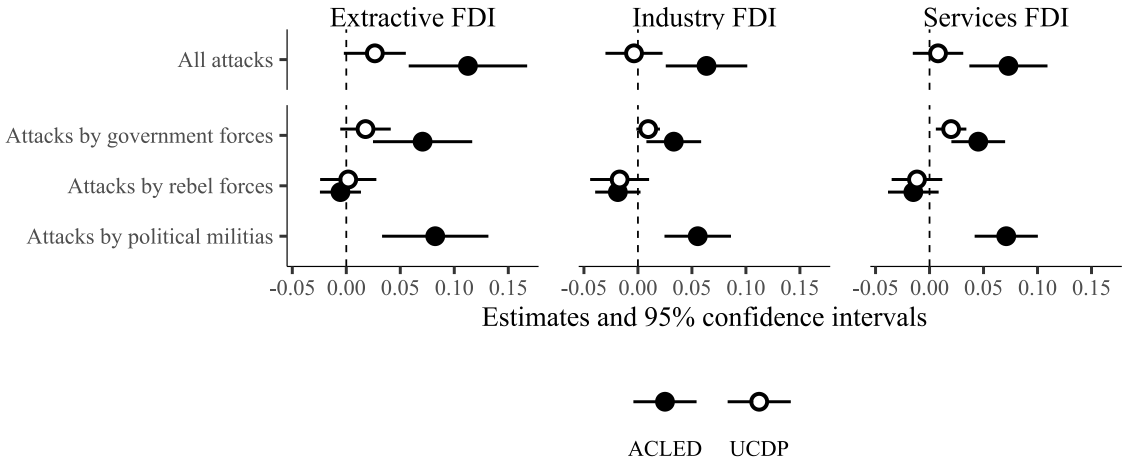
We therefore provide additional analyses studying the *number of attacks against civilians* with a two-way FE model on a panel of yearly 50×50 km PRIO-GRID cells (see SI G). Because here we do not study number of civilian casualties, we can also employ ACLED and thus

[27]Because UCDP GED does not code types of violent actors, we only distinguish among state and non-state perpetrators and leave it to a test below to differentiate types of non-state actors (rebels and state-aligned militias). However, 86% of the deliberate attacks by non-state forces in UCDP GED were initiated by an actor involved in confrontation with the government, according to UCDP data itself, suggesting that the vast majority of non-state perpetrators in the sample are in fact rebels.

study violence by pro-government militias, too. This is important because governments often outsource securitization of investment areas and civilian targeting to such actors, potentially creating victimization that should be attributed to government action (Carey et al. 2015; Carey and Mitchell 2017).

We build pairs of outcome variables measuring the (logged) number of deliberate attacks against civilians (+1) reported by UCDP and ACLED, respectively, in a cell-year. Importantly, these counts include many non-lethal events (particularly so those from ACLED[28]), a plausible indicator of repression. We also measure the number of such attacks initiated by government forces, rebel groups, and government-aligned political militias (this latter variable exists from ACLED information only).

Figure 4: The local effect of FDI on the number of attacks against civilians by type of perpetrator



Note: Results from two-way fixed effect models including a binary treatment variable and cell and year FEs. From top to bottom, full results of UCDP estimates are reported in SI Tables G.2, G.3, and G.4. For ACLED estimates, they are in Tables G.5, G.6, G.7, and G.8.

Figure 4 presents findings from linear models with cell and year FE and binary treatment variables relative to different FDI types. All FDI types increase the global number of deliberate attacks against civilians in a cell when looking at ACLED data—with the strongest increase recorded for extractive FDI (+12%). Using UCDP data, the result only holds for extractive projects (the most immobile and capital intensive, see Figure 1). This likely reflects the fact that ACLED accounts more heavily for non-lethal attacks, e.g., reports of torture or other human rights abuses, and thus may measure repression more broadly than UCDP. Consistent with our argument, government forces and state-aligned militias drive this effect: they conduct significantly more attacks (including non-lethal ones) against civilians following FDI onset in a given cell. The effect is insignificant when looking at rebel-initiated attacks (for both data sources). Rebels thus engage in deadlier attacks around FDI sites but not more frequently than in other conflict areas. Terrorism is costly and if rebels escalate violence against civilians too often this could create a backlash that will outweigh any strategic benefits anti-civilian violence has close to FDI (e.g., Polo and González 2020; Stanton 2016; Welsh 2023).

[28]ACLED reports many more non-lethal attacks against civilians: 9% of the deliberate attacks against civilians reported by UCDP in African countries between 2003 and 2019 had no fatality. When looking at ACLED, this percentage increases to 44%.

5 Conclusion

Scholars of international political economy have long debated the role of foreign actors, particularly multinational corporations, for political and economic development. This study reveals a troubling pattern: foreign direct investment (widely promoted by policy makers as a pathway to prosperity in developing countries) can fuel systematic violence against civilians when it flows into conflict zones. Our analysis of conflict events across Africa demonstrates that attacks near extractive and industry FDI see 33% and 37% more civilian casualties, respectively.

This is a substantial and previously unrecognized consequence of multinational corporate activity in conflict-affected regions. We suggest that extractive and industry FDI, compared to services FDI, are more likely to create these patterns because they are physically immobile and capital-intensive, making governments particularly sensitive to investor demands and willing to concentrate military resources around investment sites to secure rents into the future. Stronger territorial competition, in turn, creates a cycle of increased civilian targeting by both warring parties. Heightened targeting of civilians is strategic and operates through distinct mechanisms: governments perpetrate more frequent attacks to repress populations and secure control over investment sites, while rebels stage deadlier terrorist attacks to undermine state authority and signal resolve.

Our findings resonate with a growing literature that supports a nuanced take on FDI. Although foreign capital is a crucial source of finance, the effects of FDI on economic and political development hinge largely on institutional context (Brazys and Kotsadam 2020; Christensen 2019; Malesky et al. 2015; Pinto and Zhu 2016; Wright and Zhu 2018; Zhu 2017). Our analysis contributes to this literature by analyzing how global capital reshapes violence. Although other work has made important strides in disentangling links between FDI and conflict onset (Brazys et al. 2023; Mihalache-O'Keef 2018), we study the effect of FDI flowing *into* currently conflict-affected areas—a dynamic largely ignored by scholars and policy-makers despite being increasingly prevalent in today's conflict arenas.

Following other research which has demonstrated the usefulness of studying the political effects of FDI in a disaggregated manner (Malesky 2008; Malesky and Mosley 2018; Rommel 2023; Sorens and Ruger 2012; Tomashevskiy 2017), our empirical approach reveals micro-level patterns obscured in national-level analyses: violence increases close to FDI, is strategically motivated, and varies by FDI sector and perpetrator type. These findings echo a growing scholarship on the strategic logic behind anti-civilian violence; for instance to induce civilian compliance, deter defection, or encourage a backlash against the opponent (e.g., DeMeritt 2016; Polo and González 2020; Schwartz and Straus 2018; Wood 2010, 2014b). Although the available data does not allow us to directly test whether warring parties engage in selective or indiscriminate forms of violence, we detect an important trend: rebel groups engage in deadlier attacks around FDI sites whereas governments target civilians more frequently.

These findings inform an important debate on the effects of FDI on political stability by fundamentally challenging an assumptions underlying current development policy. As international actors increasingly promote private capital flows to fragile states in lieu of reduced development budgets, our evidence reveals substantial humanitarian costs linked to FDI. The mechanisms and effects we present have potential implications for armed conflict and stability beyond areas of investment, as they could drive other dynamics such as conflict diffusion, underdevelopment, or displacement. Future research should further theorize and test such related first or second-order effects of FDI.

This research also raises urgent questions for practitioners about the conditions under which development actors can promote FDI in fragile states. In an era of growing foreign investment in conflict-affected regions like Nigeria, Mozambique, or the DRC, understanding these dynamics has never been more critical. Thus, the challenge is not whether to promote FDI in fragile states, but how to structure investment relationships in contexts of weak institutions and active insurgency that allows genuine economic and political development.

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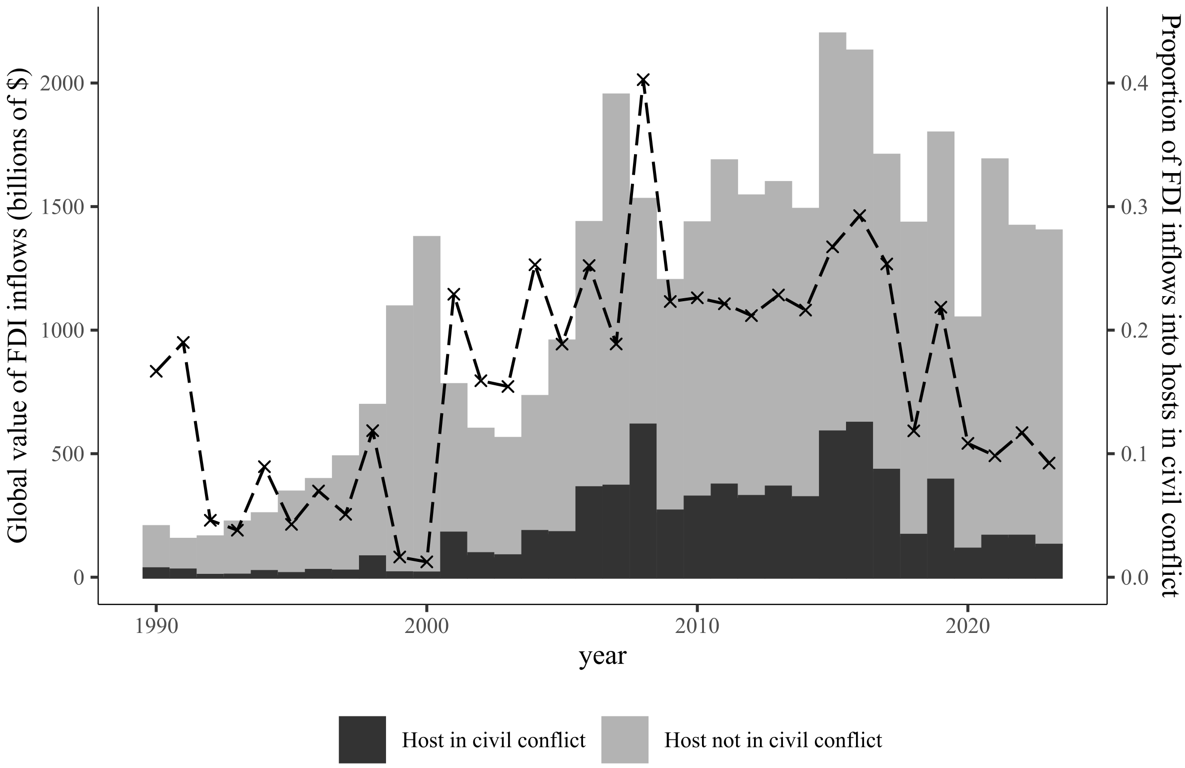
Double-Edged Sword: The Hidden Civilian Toll of FDI in Conflict Settings

Supporting Information

A FDI into conflict-affected countries

In Figure A.5, we present the yearly volume of FDI flows from UNCTAD data (histograms, left-hand side y-axis), distinguishing by whether the recipient is a country involved in civil conflict in that year or not (based on UCDP classification). We also report the proportion of FDI inflows into hosts in civil conflict over the total FDI in that year (dashed line, relative to the right-hand side y-axis). Starting from about the year 2000, global FDI has flowed more and more towards recipient countries who experienced civil conflict. Over the period 2000–2023, civil conflict recipients have received a median of 22% of global FDI flows.

Figure A.5: Conflict-Affected Countries Receive a Significant Share of Global FDI Inflows



Source: Annual FDI inflows data from UNCTAD. We follow the Uppsala Conflict Data Program (UCDP) to classify a country as experiencing a civil conflict. Note: Bars show the annual global value of FDI inflows (in billions of US dollars) to countries experiencing civil conflict (dark bars) and to countries not in civil conflict (light bars). The dashed line indicates the share of total global FDI inflows going to conflict-affected countries.

B Research design validity

B.1 Potential outcomes and identifying assumption

Here we derive and discuss the identifying assumption of our estimator. Let $i = 1, \ldots, N$ be our units (conflict events). Each unit can experience three treatment conditions based on its spatial and temporal proximity to an FDI project: $D_i = \{T, N, U\}$. $D_i = T$ indicates a "treated" conflict event, i.e. one that occurs in the proximity of an already existing FDI project. $D_i = N$ indicates a "not-yet treated" conflict event, one that occurs close to a future FDI site. $D_i = U$ indicates an "untreated" unit, one that is not proximate to any present or future FDI. Accordingly, each event i has three potential outcomes (PO) for the number of civilian fatalities: a treated PO, $Y_i(T)$; a not-yet treated PO, $Y_i(N)$; and an untreated PO, $Y_i(U)$. The fundamental problem of causal inference forces us to only observe the single PO realized by i's treatment assignment.

Our estimand is the average treatment effect on the treated (ATT) conflict events i.e., the average change in PO as treated conflict events move from the untreated to the treated condition:

$$\begin{aligned} \delta &= E[Y_i(T) - Y_i(U)|D_i = T] \\ &= E[Y_i(T)|D_i = T] - E[Y_i(U)|D_i = T] \end{aligned} \tag{B.1}$$

The first term of equation B.1—average treated PO of the treated—is observable, unlike the second—average untreated PO of the treated—which is the counterfactual. Thus, we can only estimate δ. A naive—and likely biased—estimator of δ is the difference in means:

$$\hat{\delta}_{DM} = E[Y_i(T)|D_i = T] - E[Y_i(U)|D_i = U] \tag{B.2}$$

This estimator returns a biased estimate of δ in case of treatment endogeneity, that is in case the untreated outcomes of untreated units differed from the counterfactual of the treated:

$$\begin{aligned} bias_{DM} &= \hat{\delta}_{DM} - \delta \\ &= E[Y_i(T)|D_i = T] - E[Y_i(U)|D_i = U] - E[Y_i(T)|D_i = T] + E[Y_i(U)|D_i = T] \\ &= E[Y_i(U)|D_i = T] - E[Y_i(U)|D_i = U] \end{aligned} \tag{B.3}$$

The $bias_{DM}$ is unlikely to be zero, here. Foreign investors are likely to self-select into locations with lower political risk to begin with (Jensen et al. 2012; Jensen 2003), as they prefer more stability. Thus, likely $E[Y_i(U)|D_i = U] > E[Y_i(U)|D_i = T]$. That is, conflict events in areas that never experience an FDI are probably more violent against civilians than those in areas that experience an FDI would have been, had the investment not started. The naive difference in means would likely *under*estimate the ATT of FDI on civilian fatalities in conflict events.

We instead adopt estimator $\hat{\delta}_{ST}$ proposed by Knutsen et al. (2017), which leverages the spatial-temporal variation in treatment assignment and compares the observed outcomes of treated units with those of not-yet treated ones:

$$\hat{\delta}_{ST} = E[Y_i(T)|D_i = T] - E[Y_i(N)|D_i = N] \tag{B.4}$$

We can make our identifying assumption explicit by deriving the difference between the estimator and the estimand, which we call $bias_{ST}$, and by imposing that $bias_{ST} = 0$:

$$\begin{aligned}
bias_{ST} &= \hat{\hat{\delta}}_{ST} - \delta \\
&= E[Y_i(T)|D_i = T] - E[Y_i(N)|D_i = N] - E[Y_i(T)|D_i = T] + E[Y_i(U)|D_i = T] \\
&= E[Y_i(U)|D_i = T] - E[Y_i(N)|D_i = N] \\
bias_{ST} &= 0 \\
&\Rightarrow E[Y_i(U)|D_i = T] = E[Y_i(N)|D_i = N]
\end{aligned} \tag{B.5}$$

Thus, $\hat{\hat{\delta}}_{ST}$ is an unbiased estimator of δ under the assumption that, *absent the FDI*, conflict events close to an existing investment would have had the same number of civilian casualties as events in an area of future investment, on average.

Our identifying assumption B.5 has two implications. First, it implies no treatment effect anticipation. If not-yet treated units anticipated the treatment effect, their observed outcomes would not approximate the counterfactual PO for the treated. However, because the direction of the treatment effect would be the same for the treated and (anticipating) not-yet treated units, in case of anticipation estimates from equation B.4 would just be driven towards the null.

Assuming no anticipation, a second implication of B.5 is that the *timing* of an FDI—which makes units in the $D_i = T$ group get the treatment before those in $D_i = N$—is exogenous to existing levels of violence against civilians in an area. This is a heavy assumption but in this context it is likely to hold in our favor. Because we hypothesize $\delta > 0$, we are concerned of violations of the assumption that cause us to *over*estimate δ: $\hat{\hat{\delta}}_{ST} > \delta \Rightarrow bias_{ST} > 0 \Rightarrow E[Y_i(U)|D_i = T] > E[Y_i(N)|D_i = N]$. That is, we would overestimate the effect of FDI on civilian casualties in conflict events only if the earlier-treated events, absent the treatment, would have been more violent against civilians than later-treated ones are. This seems like an implausible scenario that implies that foreign investors would *first* enter more violent areas and only later less violent ones. Self-selection of FDI first into more violent areas runs contrary to established literature on political risk (Jensen et al. 2012; Jensen 2003).

The identifying assumption implies that treated units are comparable to not-yet treated ones, were it not for the treatment. We take four steps to improve the credibility of this assumption. Because violence against civilians and FDI features can differ in time and between countries we employ, in all our analyses, country and year-FEs. Second, we make sure *not to* compare units treated with different types of FDI. We exclusively compare conflict events that are treated and not-yet treated by the same FDI type, be it extractive, industry, or services. Third, a conflict event is (not-yet) treated depending on its spatial distance from a present (future) FDI project. Larger distances include more events in these groups, increasing statistical power but introducing heterogeneity. We adopt a narrow 5 km distance to define treatment status. Finally, to remove heterogeneity between treated events and those that will be treated far in the future, in Tables E.5 and E.6 we look only at the next one and five years for defining a conflict event as not-yet treated. Some estimates are, however, driven towards the null when considering just one time-point in the future, which we interpret as a possible result of anticipation.

In Figures D.1, D.2, and D.3 we show that comparing treated and not-yet treated conflict events removes severe existing differences in covariates between treated and untreated conflict events.

This lends credibility to our identifying assumption that treated conflict events would have looked sufficiently similar to not-yet treated ones, had it not been for the FDI.

B.2 Truncation of FDI data

Here we discuss two ways left truncation of FDI data might affect our estimates. We code units' treatment status using data from fDi Markets which span from 2003 to 2021. A first problem occurs if *not-yet treated* events are in fact occurring in the proximity of an FDI established before 2003, which we do not observe. The second problem occurs if events coded as *untreated* are in fact occurring in the proximity of an FDI established before 2003. Before addressing them, we make two empirical considerations: FDI clusters in nearby areas (Figure 2). Thus, areas of pre-2003 investment are likely also represented in our data, a feature which should mitigate concerns. Moreover, we offer two empirical tests to mitigate concerns of left truncation of the treatment group and find our effects are robust (Tables E.3 and E.4).

Recalling equation B.5, the first case of truncation would bias our estimates if, by accounting for it, $E[Y_i(U)|D_i = T] > E[Y_i(N)|D_i = N]$. That is, if truncated events (treated before 2003) would have been systematically more violent against civilians, absent the treatment, than those treated in later time points. This scenario is as implausible as the one described in the previous section: it implies self-selection of FDI first into more violent areas. Truncation in the opposite direction would, instead, drive our estimates towards the null.

The second case of left truncation would bias our estimates only if, were we to code truncated untreated events as treated, these events would have had more civilian casualties, absent the treatment, than not-yet treated units. Because these truncated units, too, would be treated in previous time points, this again implies a scenario where FDI self selects first into areas that are more violent to begin with. We rule such a violation of our assumption as implausible.

C Descriptive statistics of geolocated and non-geolocated FDI projects

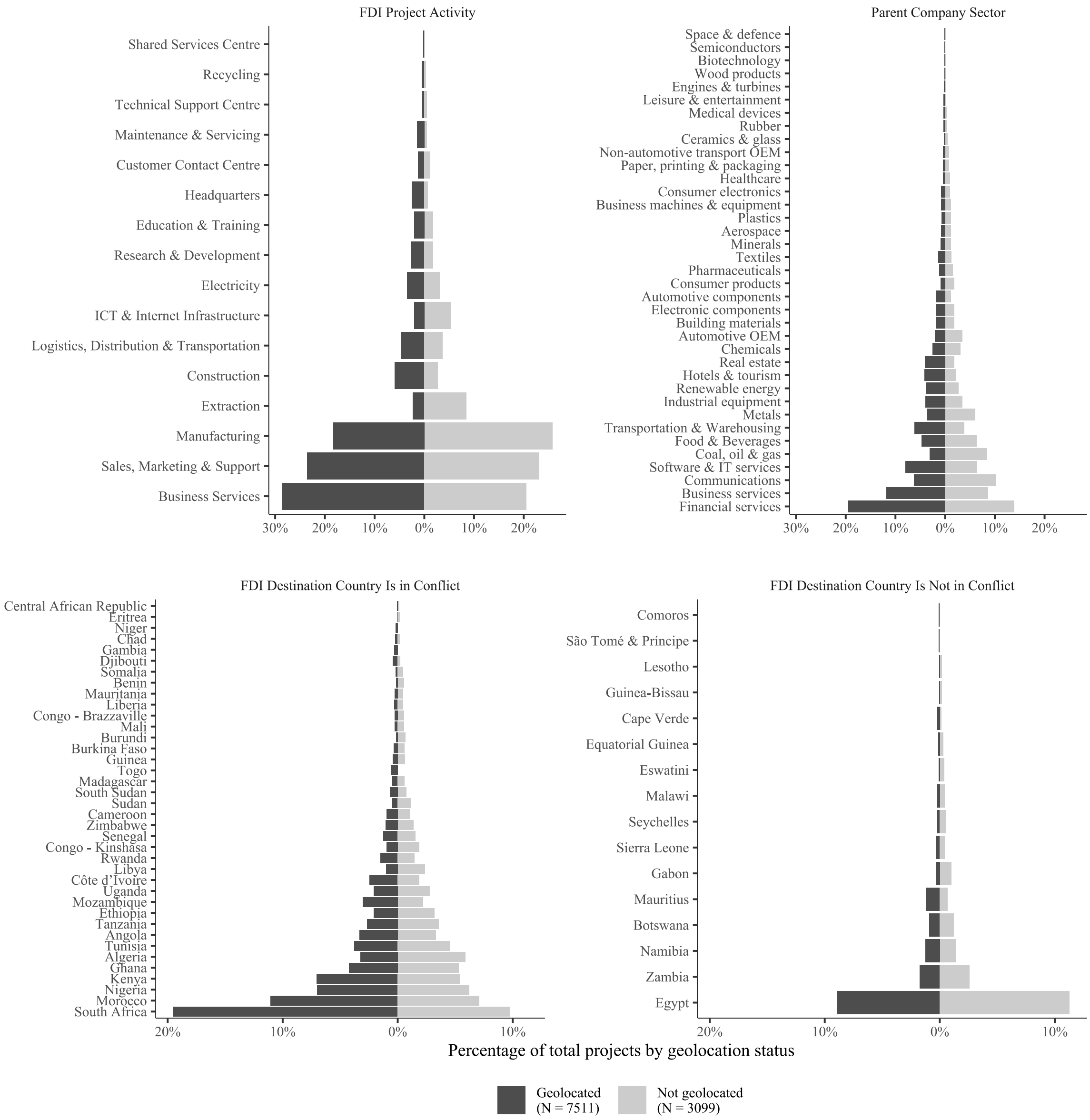
In Table C.1 we present average difference in covariates for FDI projects that report information on their location (7,511, or 71% of 10,610 total projects) and those that do not (3,099, 29%). Geolocated projects create significantly fewer jobs and invest smaller amounts of capital. Moreover, they are significantly less likely to have low-quality information on covariates, at least with respect to whether the "jobs created" figure is estimated. Figure C.1 plots the distribution of FDI activity, MNC sector, and destination country for these projects. The distributions of these variables are generally similar in the geolocated and non-geolocated groups. However, there are relatively more projects in "extraction," "manufacturing" (activity) and in "Coal, oil & gas" and "Metals" (sector) that are not geolocated.

Table C.1: Comparison of covariates for geolocated and non-geolocated FDI projects

	Geolocated (N=7511)		Not geolocated (N=3099)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Jobs Created	185.56	464.30	220.24	452.39	34.68***	9.73
Jobs Created Is Estimated	0.88	0.32	0.95	0.23	0.06***	0.01
Capital Investment	93.80	585.97	127.51	544.71	33.71***	11.89
Capital Investment Is Estimated	0.83	0.37	0.84	0.37	0.01	0.01

We can hypothesize how selection into geolocation biases our estimates. Our analyses are based on FDI projects that are smaller and under-represent extractive and manufacturing FDI. These more capital intensive and relatively more "immobile" projects would likely further heighten the conflict intensity in their proximity, were they included in the analysis (see Blair et al. 2022; Maher 2015; Mihalache-O'Keef and Vashchilko 2010; Rexer 2021). Thus we expect that, had non-geolocated FDI projects been provided with location information and included in our analysis, their effect would likely *increase* our observed positive effect.

Figure C.1: Comparison between geolocated and non-geolocated FDI projects by activity, sector, and destination country



D Descriptive statistics of treatment groups

D.1 Distribution of units across groups with different treatment definitions

Table D.1 reports the number of conflict events coded as "treated," "not-yet treated," and "untreated" by FDI type. It also illustrates how the size of the "not-yet treated" and "untreated" groups changes depending on the number of future time points considered when coding a conflict event as not-yet treated—which is relevant information for interpreting Tables E.5 and E.6.

To illustrate, consider the treatment condition of extractive FDI (top three rows). Out of 22,480 conflict events, 304 (1.35%) happen in the proximity (5 km) of an existing extractive FDI site. Among the events that do not occur in the proximity of an existing extractive FDI, 452 (2.01%) occur in areas that will see a proximate extractive FDI at any future time point in our data. This group shrinks when restricting it to consider only events that occur in areas that will see an extractive FDI over the next year (90 conflict events, 0.40% of the total) or over the next five years (356 events, 1.58% of the total). The remaining events are coded as untreated. Similar patterns can be observed for industry and services FDI.

Table D.1: Distribution of observations by treatment group and year baseline

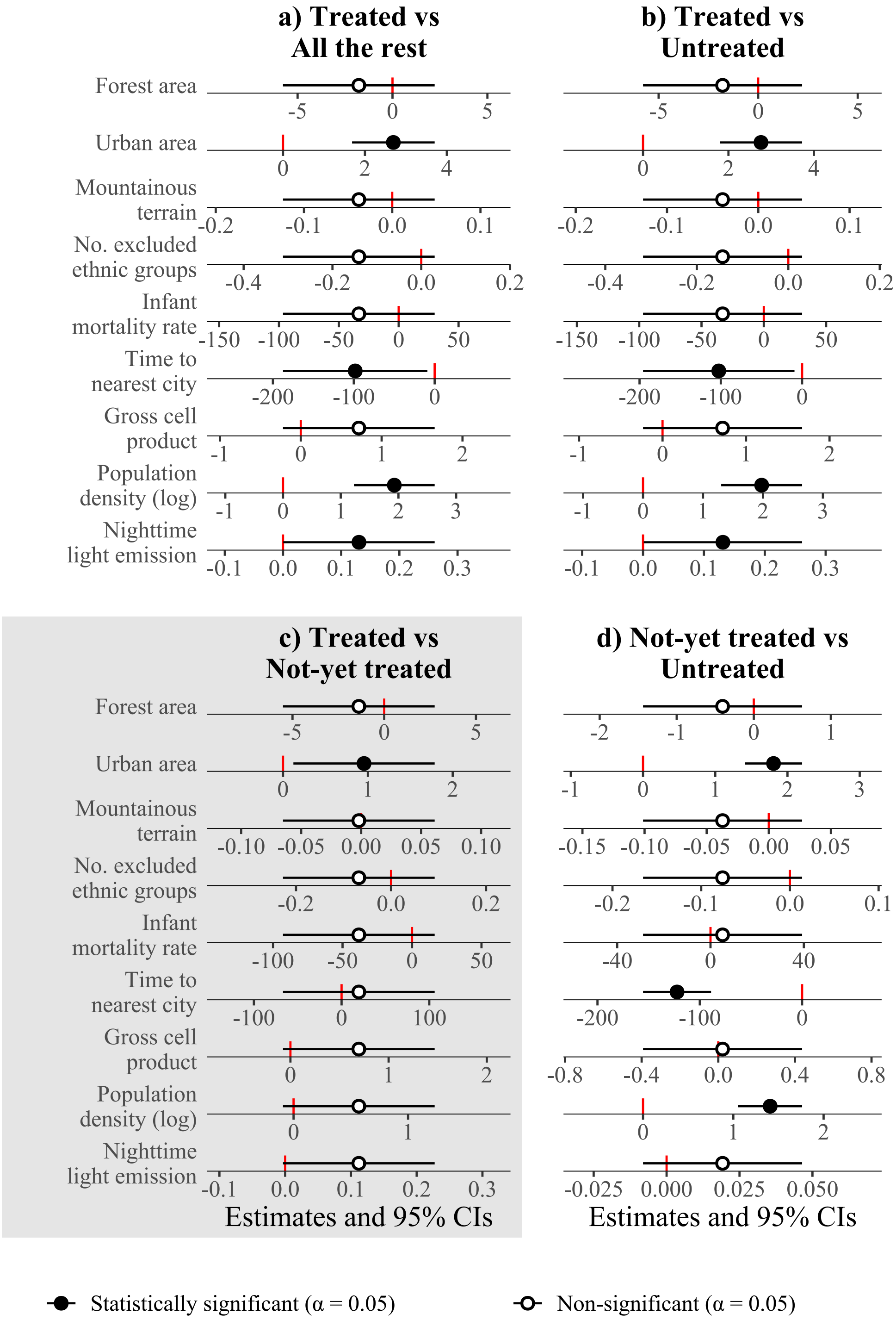
FDI treatment	Treated	Treated (%)	Not-yet treated	Not-yet treated (%)	Untreated	Untreated (%)
Extractive	304.00	(1.35%)	452.00	(2.01%)	21724.00	(96.64%)
Extractive (1y)	304.00	(1.35%)	90.00	(0.40%)	22086.00	(98.25%)
Extractive (5y)	304.00	(1.35%)	356.00	(1.58%)	21820.00	(97.06%)
Industry	771.00	(3.43%)	459.00	(2.04%)	21250.00	(94.53%)
Industry (1y)	771.00	(3.43%)	107.00	(0.48%)	21602.00	(96.09%)
Industry (5y)	771.00	(3.43%)	224.00	(1.00%)	21485.00	(95.57%)
Services	836.00	(3.72%)	289.00	(1.29%)	21355.00	(95.00%)
Services (1y)	836.00	(3.72%)	31.00	(0.14%)	21613.00	(96.14%)
Services (5y)	836.00	(3.72%)	207.00	(0.92%)	21437.00	(95.36%)

D.2 Distribution of covariates and outcomes by treatment group

Here we show that our research design removes large differences in covariates between conflict events that are treated and the rest. We consider covariates that likely correlate with FDI treatment status and conflict intensity, all drawn from version 2.0 of the PRIO-GRID and defined at the level of the cell where conflict event i occurs, at time t. We take the latest available value of that covariate before time t. We consider: percentage of cell area covered by forest, by urban area, by mountainous terrain; number of discriminated or powerless ethnic groups; infant mortality rate; average travel time to the nearest major city; gross cell product; (log of) population density; and calibrated average nighttime light emission.[1]

[1] Because our events span across 17 years and 55 countries, we make meaningful comparisons among them by subtracting from each covariate its average value at the country and year level ("entity demeaning") consistently with our research design that always includes country and year fixed effects.

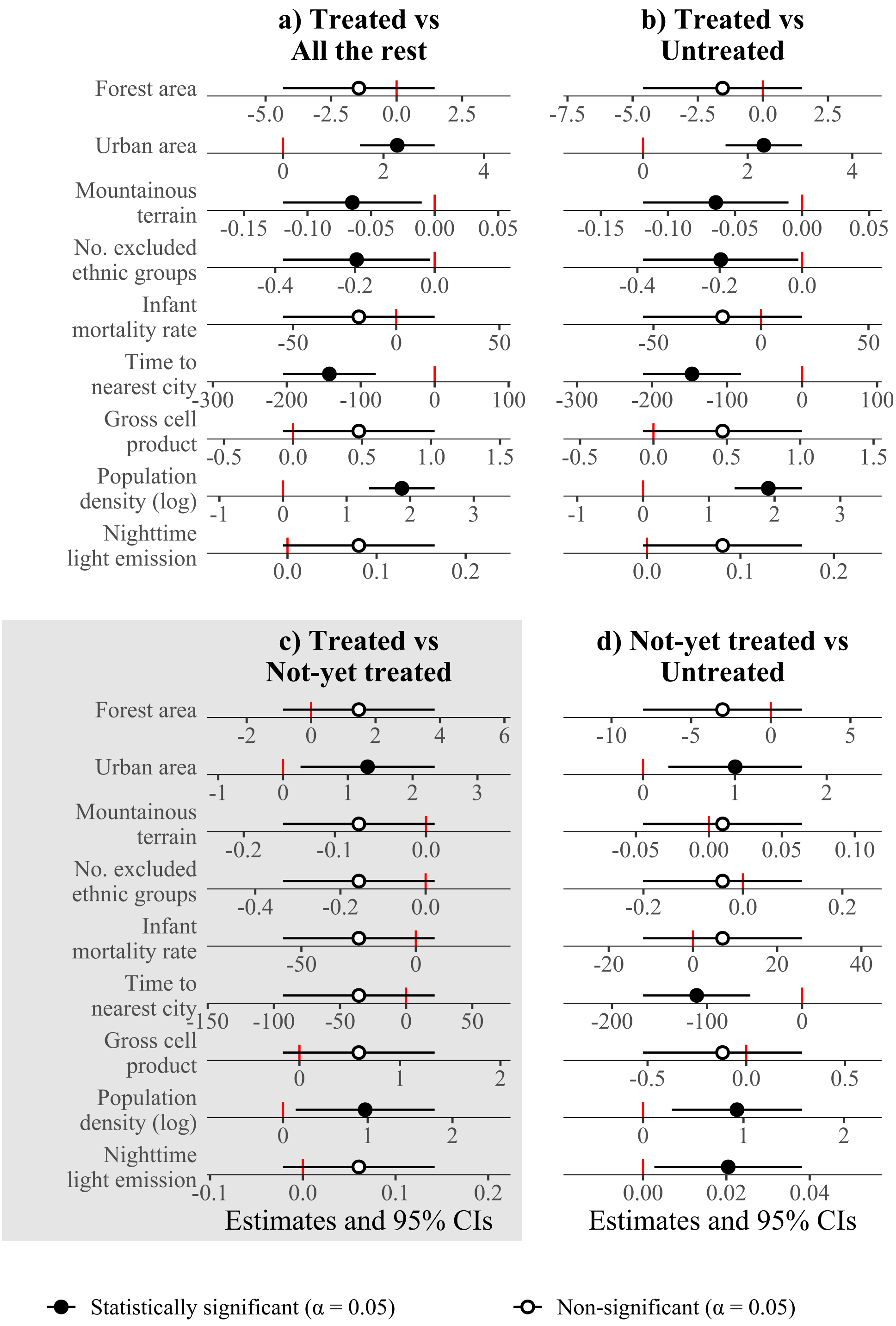
Figure D.1: Extractive FDI: Distribution of covariates for conflict events belonging to different treatment groups.



Note: Shaded panel reports the comparison used in the main analysis. Variables are de-meaned of country and year averages to remove heterogeneity at these levels

Figure D.1 reports the difference in mean of the covariates between units in different extractive FDI treatment statuses. Conflict events occurring 5 km from an existing extractive FDI happen, on average, in more urban and densely populated areas, closer to a major city and with stronger nighttime light emission than the others (panel a). These differences are driven by the unbalanced comparison between treated conflict events and untreated ones (panel b). Instead, a comparison between treated and not-yet treated units presents a more balanced distribution

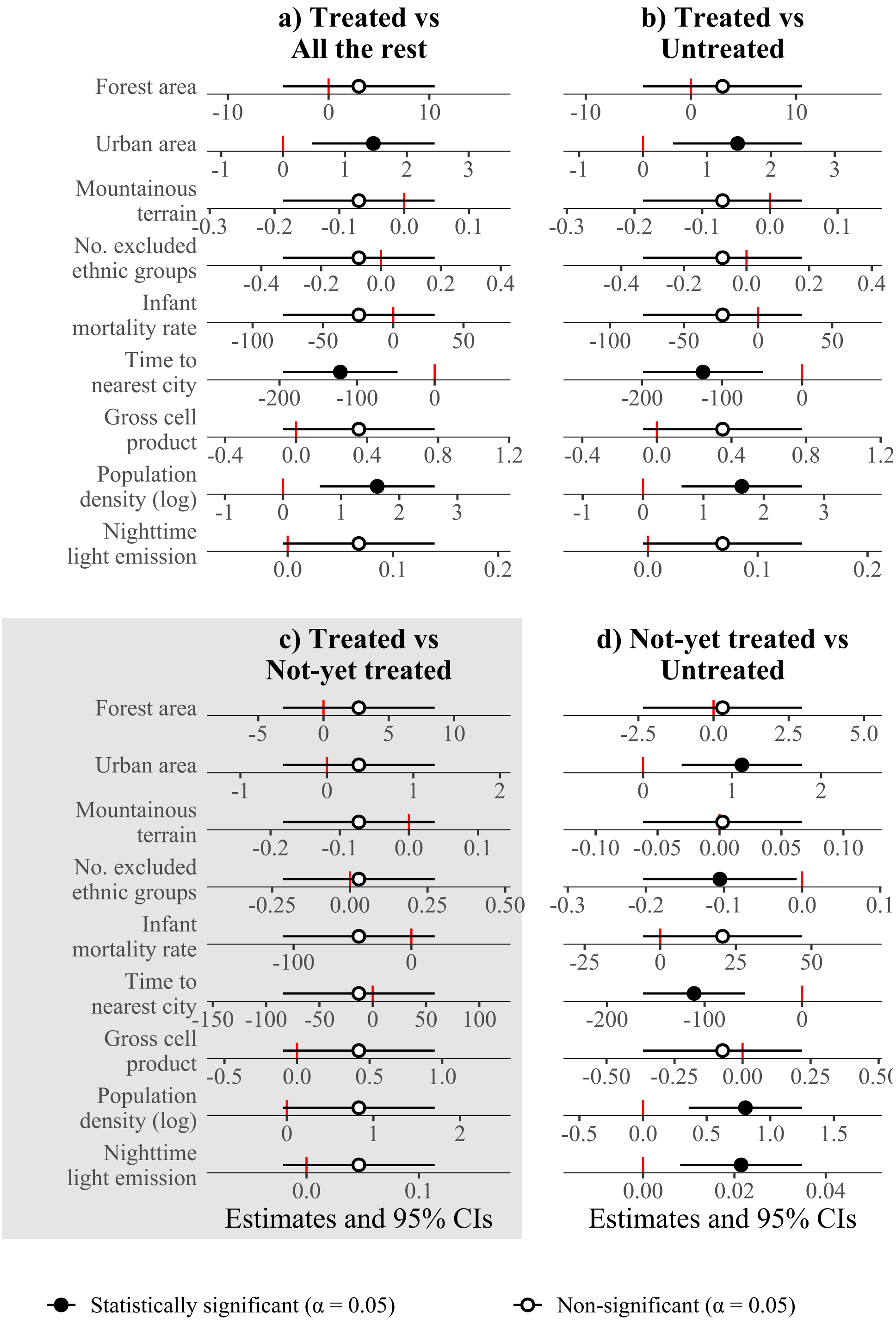
Figure D.2: Industry FDI: Distribution of covariates for conflict events belonging to different treatment groups.



Note: Shaded panel reports the comparison used in the main analysis. Variables are de-meaned of country and year averages to remove heterogeneity at these levels

of covariates (panel c), the only exception being the percentage of urban area in a cell, which we thus include as a control variable in our models. Panel d concludes by showing differences in covariates between the not-yet treated and the untreated groups. It illustrates the bias from self-selection of FDI into more densely populated and urbanized areas. We repeat the exercise, with similar conclusions, for industry FDI (Figure D.2) and services (Figure D.3).

Figure D.3: Services FDI: Distribution of covariates for conflict events belonging to different treatment groups.



Note: Shaded panel reports the comparison used in the main analysis. Variables are de-meaned of country and year averages to remove heterogeneity at these levels

In Figure D.4 we present differences in the outcome variable across treatment groups.[2] Consider extractive FDI. Treated conflict events—i.e., those occurring within 5 km of an existing extractive FDI—tend to be more violent for civilians than all the rest, as suggested by our argument. However, these differences can be confounded by the imbalances in covariates illustrated

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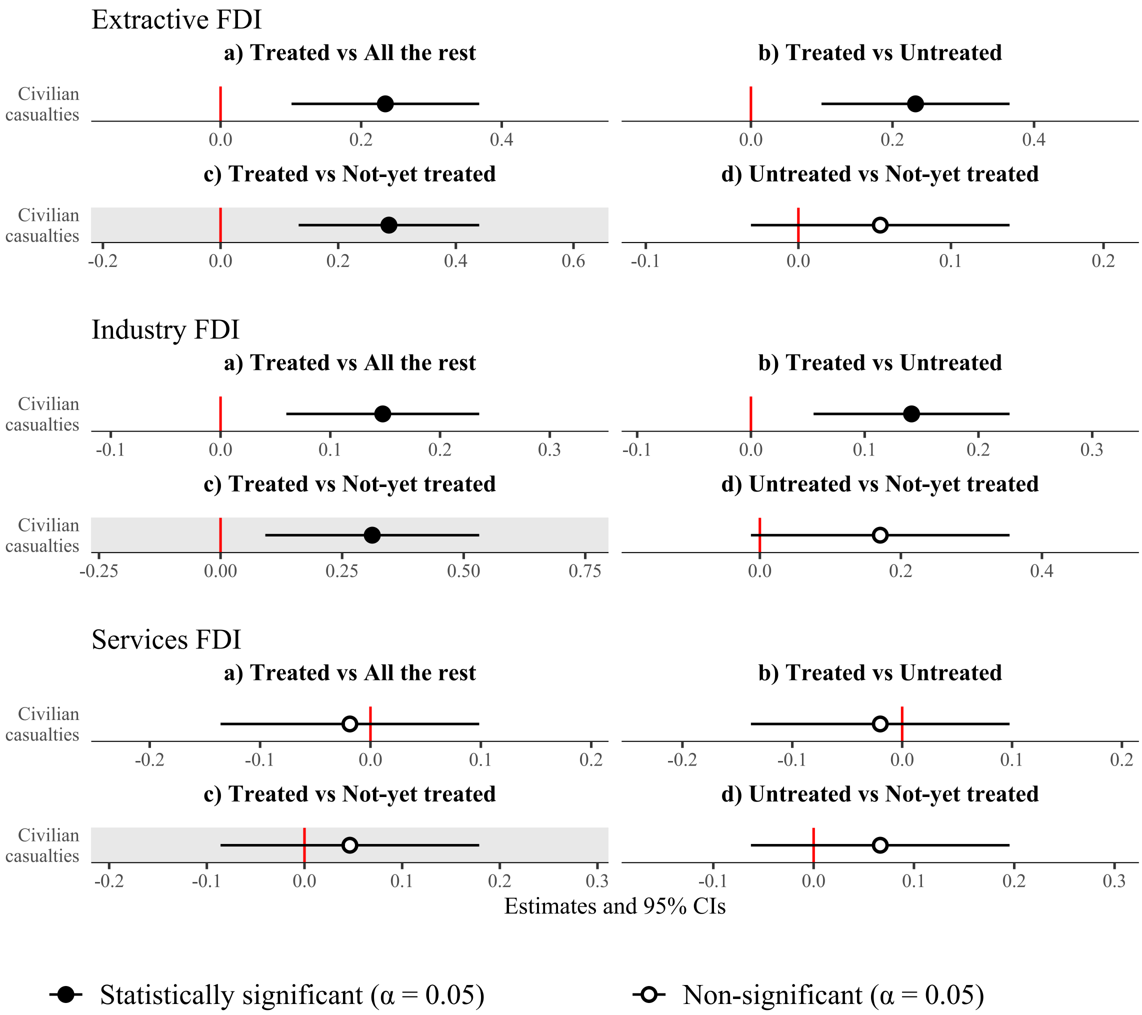
[2] As in the previous test, we de-mean the dependent variable by its own average by country and year.

in Figure D.1. To remove such selection bias, we compare treated and not-yet treated conflict events. A comparison between these groups still shows that events happening in the proximity of existing investments are more violent for civilians. The second and third panels replicate the exercise for FDI in the industry sector and services.

Figure D.4: Differences in casualties by treatment group and FDI type



Note: Shaded panel reports the comparison used in the main analysis. Variables are de-meaned of country and year averages to remove heterogeneity at these levels

E Robustness tests of main analysis

E.1 PRIO-GRID cell fixed effects

Table E.1: Results obtained with PRIO-GRID 50km cell fixed effects

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.280**	0.212*	0.212*	0.265+	0.250+	0.250+	0.025	-0.007	-0.008
	(0.090)	(0.088)	(0.089)	(0.147)	(0.141)	(0.141)	(0.091)	(0.100)	(0.102)
Untreated *vs* Not-yet treated	0.252***	0.193***	0.193***	0.207	0.196	0.196	0.050	0.044	0.043
	(0.058)	(0.050)	(0.050)	(0.165)	(0.145)	(0.145)	(0.086)	(0.098)	(0.098)
No. excluded ethnic groups		-0.053	-0.053		-0.053	-0.053		-0.054	-0.053
		(0.123)	(0.123)		(0.122)	(0.123)		(0.123)	(0.123)
Urban area		0.293*	0.293*		0.287*	0.287*		0.331*	0.331*
		(0.119)	(0.119)		(0.112)	(0.111)		(0.127)	(0.127)
Population density (log)		-1.293+	-1.308+		-1.318+	-1.327+		-1.289	-1.332+
		(0.762)	(0.704)		(0.751)	(0.686)		(0.770)	(0.696)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cell time-trend			Yes			Yes			Yes
Mean of outcome	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.279	0.282	0.282	0.279	0.282	0.282	0.278	0.282	0.282
R2 Adj.	0.219	0.216	0.216	0.219	0.216	0.216	0.218	0.216	0.216

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Notes: All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.

We re-estimate Table 1 and substitute country FE with PRIO-GRID cell FE (and cell-specific linear time trend) to completely remove all unobservable time-invariant confounders in conflict dynamics and FDI at the cell level. We do not include the mountainous terrain covariate in these models because it does not vary within-cell. Results, reported in Table E.1, confirm the positive and statistically significant effect of extractive FDI on the number of civilian casualties in conflict events, similar in magnitude to that found earlier. Although estimates are similar when looking at industry FDI, their standard errors are here larger, resulting in noisier estimates (albeit consistently positive and sizeable). We find no effect for services FDI.

E.2 Account for spatial correlation

We re-estimate Table 1 and substitute our country-clustered standard errors with Conley (1999) standard errors to account for spatial correlation between observations. We adopt a cutoff of 100km. Results, presented in Table E.2, are broadly consistent with those reported in the main text.

Table E.2: Results obtained with Conley (1999) standard errors

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.286***	0.238**	0.198*	0.312**	0.271*	0.224*	0.046	-0.011	-0.030
	(0.075)	(0.073)	(0.087)	(0.108)	(0.107)	(0.114)	(0.075)	(0.067)	(0.067)
Untreated *vs* Not-yet treated	0.054	0.090*	0.066	0.171*	0.179*	0.136	0.066	0.085	0.052
	(0.048)	(0.038)	(0.045)	(0.083)	(0.085)	(0.088)	(0.060)	(0.058)	(0.051)
Mountainous terrain		-0.090	-0.093		-0.084	-0.087		-0.099	-0.100
		(0.079)	(0.081)		(0.080)	(0.082)		(0.082)	(0.084)
No. excluded ethnic groups		-0.052	-0.015		-0.050	-0.013		-0.055	-0.018
		(0.042)	(0.039)		(0.043)	(0.040)		(0.042)	(0.040)
Urban area		-0.023	-0.015		-0.024+	-0.017		-0.018	-0.011
		(0.014)	(0.014)		(0.014)	(0.014)		(0.014)	(0.015)
Population density (log)		0.051**	0.035*		0.050**	0.034*		0.054**	0.039*
		(0.018)	(0.017)		(0.018)	(0.018)		(0.018)	(0.017)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.129	0.152	0.133	0.130	0.153	0.132	0.129	0.152
R2 Adj.	0.131	0.126	0.148	0.131	0.127	0.148	0.130	0.126	0.148

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. Conley (1999) standard errors with 100km cutoffs, in parentheses, to account for spatial correlation. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.

E.3 Account for left-truncation

Table E.3: Results when studying only post-2011 events

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.205*	0.128+	0.237***	0.434***	0.334***	0.390***	-0.106	-0.167+	-0.156
	(0.093)	(0.069)	(0.057)	(0.067)	(0.077)	(0.084)	(0.102)	(0.095)	(0.093)
Untreated *vs* Not-yet treated	0.032	0.090*	0.157**	0.311***	0.305***	0.355***	-0.068	-0.034	-0.043
	(0.041)	(0.036)	(0.046)	(0.061)	(0.062)	(0.065)	(0.100)	(0.094)	(0.092)
Mountainous terrain		-0.067	-0.029		-0.063	-0.023		-0.080	-0.039
		(0.120)	(0.120)		(0.123)	(0.123)		(0.127)	(0.128)
No. excluded ethnic groups		-0.104*	-0.085+		-0.102*	-0.082+		-0.108*	-0.089+
		(0.049)	(0.047)		(0.049)	(0.048)		(0.050)	(0.049)
Urban area		-0.006	0.001		-0.006	0.000		-0.004	0.003
		(0.019)	(0.019)		(0.018)	(0.018)		(0.020)	(0.020)
Population density (log)		0.047*	0.032		0.048*	0.033		0.053*	0.037+
		(0.022)	(0.021)		(0.022)	(0.022)		(0.022)	(0.020)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	0.627	0.627	0.627	0.627	0.627	0.627	0.627	0.627	0.627
Num.Obs.	15588	10072	10072	15588	10072	10072	15588	10072	10072
R2	0.120	0.096	0.109	0.122	0.097	0.110	0.120	0.097	0.109
R2 Adj.	0.118	0.092	0.103	0.119	0.093	0.104	0.117	0.093	0.103

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. Only events after 2011 considered. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.

In Table E.3 we offer a test to mitigate concerns about how left truncation of FDI data could bias our estimates (the issue is discussed in section B.2). We replicate our analysis after limiting our sample to post-2011 events (the median year in our 2003–2019 time series of conflict events), with the idea that left truncation should impact this analysis less than the one using the full data. We find consistent estimates to those presented earlier.

Table E.4: Results when considering areas of FDI expansion as treated

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.296***	0.262***	0.198**	0.306*	0.269*	0.216*	0.042	-0.012	-0.032
	(0.067)	(0.062)	(0.064)	(0.119)	(0.117)	(0.102)	(0.067)	(0.076)	(0.069)
Untreated *vs* Not-yet treated	0.056	0.092*	0.071*	0.186+	0.193+	0.143	0.065	0.085	0.052
	(0.040)	(0.043)	(0.028)	(0.099)	(0.105)	(0.090)	(0.067)	(0.073)	(0.067)
Mountainous terrain		-0.090	-0.092		-0.084	-0.088		-0.099	-0.100
		(0.090)	(0.100)		(0.092)	(0.103)		(0.092)	(0.103)
No. excluded ethnic groups		-0.051	-0.014		-0.051	-0.014		-0.055	-0.018
		(0.068)	(0.069)		(0.069)	(0.070)		(0.070)	(0.071)
Urban area		-0.024	-0.015		-0.024	-0.017		-0.018	-0.011
		(0.017)	(0.017)		(0.017)	(0.015)		(0.016)	(0.016)
Population density (log)		0.050*	0.035*		0.050*	0.035*		0.054*	0.039*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.129	0.153	0.133	0.130	0.153	0.132	0.129	0.152
R2 Adj.	0.131	0.126	0.148	0.131	0.127	0.148	0.130	0.126	0.148

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.

Next, we reconstruct our treatment variables by including in the "treated" group also "untreated" and "not-yet treated" conflict events that occur in the proximity of at least one FDI site coded as "expansions" or "co-locations" by fDi Markets, which indicates an FDI existed there previously—we distinguish extractive, industry, or services. Table E.4 reports results using these new treatment variables. The effects of extractive and industry FDI are still positive and sizeable, similar to those estimated in our main analysis.

E.4 Alternative definitions of treatment groups in time and space

Table E.5: Results when coding not-yet treated group based on FDI sites one year in the future

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated (1y)	-0.316	-0.341	-0.357	0.506***	0.470***	0.443***	0.033	-0.048	-0.035
	(0.279)	(0.275)	(0.278)	(0.093)	(0.101)	(0.101)	(0.158)	(0.147)	(0.144)
Untreated *vs* Not-yet treated (1y)	-0.553+	-0.513+	-0.509+	0.363***	0.376***	0.357***	0.052	0.045	0.045
	(0.297)	(0.283)	(0.278)	(0.068)	(0.066)	(0.068)	(0.162)	(0.151)	(0.144)
Mountainous terrain		-0.084	-0.088		-0.087	-0.089		-0.099	-0.100
		(0.092)	(0.102)		(0.092)	(0.102)		(0.092)	(0.103)
No. excluded ethnic groups		-0.051	-0.014		-0.049	-0.012		-0.054	-0.017
		(0.068)	(0.069)		(0.069)	(0.070)		(0.070)	(0.071)
Urban area		-0.029+	-0.021		-0.024	-0.017		-0.019	-0.012
		(0.016)	(0.014)		(0.017)	(0.015)		(0.016)	(0.016)
Population density (log)		0.050*	0.034*		0.050*	0.034*		0.054*	0.039*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.134	0.130	0.154	0.134	0.130	0.153	0.132	0.129	0.152
R2 Adj.	0.132	0.127	0.149	0.131	0.127	0.148	0.130	0.126	0.148

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened over the next year.

In Tables E.5 and E.6 we restrict the number of future time points used to code an event as not-yet treated, respectively to one and five years. The effect of existing extractive FDI on civilian casualties is not significant when compared to events in areas that will see an extractive investment over the next year. This might be driven by anticipation of incoming investment, which should drive the estimate towards the null (see Section B.1). However, the null might simply be due to the extremely small number of not-yet treated units in this model (see Table D.1). We find significant positive effects when considering the next five years, instead. The effect of industry FDI is consistently positive and significant. No effect is found for services.

Table E.6: Results when coding not-yet treated group based on FDI sites five years in the future

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated (5y)	0.266**	0.226**	0.193*	0.357**	0.330**	0.290*	-0.006	-0.068	-0.082
	(0.075)	(0.070)	(0.080)	(0.127)	(0.120)	(0.114)	(0.083)	(0.097)	(0.085)
Untreated *vs* Not-yet treated (5y)	0.033	0.074*	0.060+	0.215*	0.237*	0.204*	0.013	0.025	-0.002
	(0.031)	(0.028)	(0.034)	(0.105)	(0.107)	(0.098)	(0.086)	(0.091)	(0.082)
Mountainous terrain		-0.090	-0.093		-0.085	-0.088		-0.099	-0.100
		(0.089)	(0.100)		(0.092)	(0.102)		(0.092)	(0.103)
No. excluded ethnic groups		-0.051	-0.014		-0.050	-0.013		-0.054	-0.017
		(0.068)	(0.069)		(0.069)	(0.070)		(0.070)	(0.071)
Urban area		-0.024	-0.016		-0.024	-0.017		-0.018	-0.012
		(0.017)	(0.016)		(0.017)	(0.015)		(0.016)	(0.016)
Population density (log)		0.051*	0.035*		0.050*	0.035*		0.054*	0.039*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.129	0.152	0.133	0.130	0.153	0.132	0.129	0.152
R2 Adj.	0.131	0.126	0.148	0.131	0.127	0.148	0.130	0.126	0.148

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

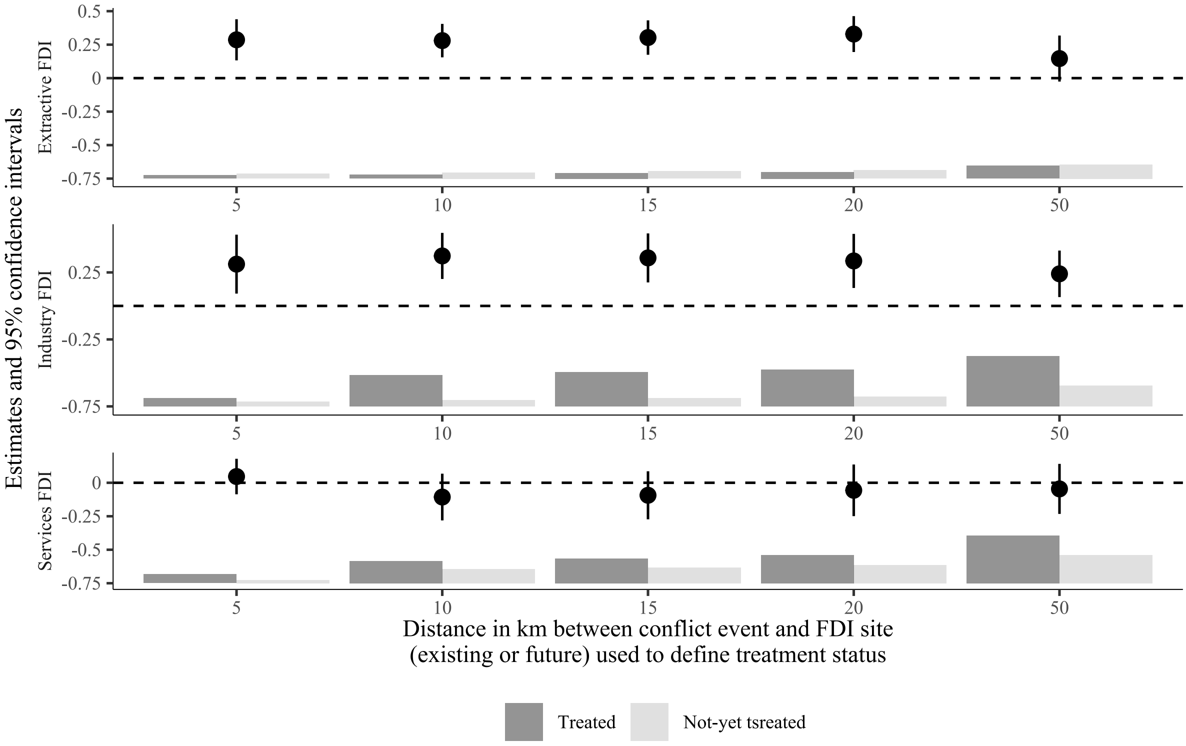
All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened over the next five years.

In Figure E.1 we vary the distance from FDI sites used to defined treatment groups to show that our main findings are not driven by the arbitrary choice of 5 km, nor that they are biased by measurement error in our geolocation of FDI projects. We consider radii of 5, 10, 15, 20, and 50 km. The effect is positive and significant for extractive and industry-sector FDI across the board. No significant effect is found for FDI in services. Estimates obtained with 50 km radii become noisier, particularly so for extractive FDI.

E.5 Control for number of jobs created by FDI

We show that our results are robust to controlling for the total number of jobs created by existing FDI in the proximity of a conflict event. This control rules out an alternative explanation: that a foreign investment offers jobs which make an area more populous (as workers and their families relocate there), thus increasing the number of civilian casualties in nearby violent attacks—*vis-à-vis* areas that are yet-to see an investment—purely because of this influx of non-combatants. We control for the number of jobs created by the investment—a quantity which is typically decided at the planning stage of an investment, therefore pre-treatment. We replicate Table 1 after including, respectively, the number of jobs created by existing extractive, industry, and services FDI within 5 km of the event. Even after holding constant the size of population influx attracted by foreign jobs, we find that extractive and industry FDI increase the number of civilian casualties, with effects comparable to our previous analysis in size and significance.

Figure E.1: Estimates obtained when varying sizes of buffer radii around conflict events



Note: estimates are relative to the comparison between *Treated* and *Not-yet treated* buffers using country and year fixed effects. Top panel reports results when defining treatment status based on extractive FDI; middle panel focuses on industry FDI; bottom panel on services. Histograms report distribution of Treated and Not-yet treated units per specification.

Table E.7: Results when controlling for the number of jobs created

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.423***	0.370***	0.345***	0.288*	0.262*	0.205+	0.009	-0.037	-0.065
	(0.065)	(0.083)	(0.046)	(0.119)	(0.124)	(0.117)	(0.074)	(0.078)	(0.075)
Untreated *vs* Not-yet treated	0.051	0.087+	0.066*	0.172+	0.179+	0.136	0.065	0.081	0.048
	(0.042)	(0.044)	(0.031)	(0.090)	(0.102)	(0.087)	(0.064)	(0.073)	(0.068)
FDI jobs created	-0.304***	-0.291***	-0.312***	0.010+	0.004	0.009	0.046***	0.032**	0.046***
	(0.033)	(0.046)	(0.046)	(0.006)	(0.009)	(0.012)	(0.011)	(0.011)	(0.011)
Mountainous terrain		-0.094	-0.097		-0.084	-0.088		-0.101	-0.103
		(0.092)	(0.102)		(0.093)	(0.103)		(0.092)	(0.102)
No. excluded ethnic groups		-0.052	-0.015		-0.049	-0.012		-0.053	-0.013
		(0.069)	(0.070)		(0.069)	(0.069)		(0.070)	(0.070)
Urban area		-0.023	-0.015		-0.024	-0.018		-0.020	-0.014
		(0.018)	(0.017)		(0.017)	(0.016)		(0.016)	(0.017)
Population density (log)		0.051*	0.035*		0.050*	0.034*		0.053*	0.037*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648	0.648
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.130	0.153	0.134	0.130	0.153	0.133	0.129	0.153
R2 Adj.	0.131	0.127	0.148	0.131	0.127	0.148	0.131	0.126	0.148

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.

F Subgroup analysis: type of attacks and perpetrators

We perform subgroup analyses by limiting data in Table 1 models to one-sided violence against civilians (Table F.1) and state-based violence (Table F.2). The positive effect of extractive and industry FDI on civilian casualties is detected only for events of deliberate violence against civilians. Finally, we further split deliberate attacks against civilian by perpetrator, distinguishing cases initiated by government forces (Table F.4) and by non-government forces (Table F.3). The positive and significant effect of extractive and industry FDI is detected only for cases of one-sided violence against civilians initiated by non-governmental forces.

Table F.1: Subgroup analysis: one-sided violent events (civilian targeting) only

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.423***	0.370***	0.345***	0.288*	0.262*	0.205+	0.009	-0.037	-0.065
	(0.065)	(0.083)	(0.046)	(0.119)	(0.124)	(0.117)	(0.074)	(0.078)	(0.075)
Untreated *vs* Not-yet treated	0.051	0.087+	0.066*	0.172+	0.179+	0.136	0.065	0.081	0.048
	(0.042)	(0.044)	(0.031)	(0.090)	(0.102)	(0.087)	(0.064)	(0.073)	(0.068)
FDI jobs created	-0.304***	-0.291***	-0.312***	0.010+	0.004	0.009	0.046***	0.032**	0.046***
	(0.033)	(0.046)	(0.046)	(0.006)	(0.009)	(0.012)	(0.011)	(0.011)	(0.011)
Mountainous terrain		-0.094	-0.097		-0.084	-0.088		-0.101	-0.103
		(0.092)	(0.102)		(0.093)	(0.103)		(0.092)	(0.102)
No. excluded ethnic groups		-0.052	-0.015		-0.049	-0.012		-0.053	-0.013
		(0.069)	(0.070)		(0.069)	(0.069)		(0.070)	(0.070)
Urban area		-0.023	-0.015		-0.024	-0.018		-0.020	-0.014
		(0.018)	(0.017)		(0.017)	(0.016)		(0.016)	(0.017)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	1.332	1.332	1.332	1.332	1.332	1.332	1.332	1.332	1.332
Population density (log)		0.051*	0.035*		0.050*	0.034*		0.053*	0.037*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.130	0.153	0.134	0.130	0.153	0.133	0.129	0.153
R2 Adj.	0.131	0.127	0.148	0.131	0.127	0.148	0.131	0.126	0.148

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.Data limited to one-sided violence against civilians.

Table F.2: Subgroup analysis: state-based violent events only

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.423***	0.370***	0.345***	0.288*	0.262*	0.205+	0.009	-0.037	-0.065
	(0.065)	(0.083)	(0.046)	(0.119)	(0.124)	(0.117)	(0.074)	(0.078)	(0.075)
Untreated *vs* Not-yet treated	0.051	0.087+	0.066*	0.172+	0.179+	0.136	0.065	0.081	0.048
	(0.042)	(0.044)	(0.031)	(0.090)	(0.102)	(0.087)	(0.064)	(0.073)	(0.068)
FDI jobs created	-0.304***	-0.291***	-0.312***	0.010+	0.004	0.009	0.046***	0.032**	0.046***
	(0.033)	(0.046)	(0.046)	(0.006)	(0.009)	(0.012)	(0.011)	(0.011)	(0.011)
Mountainous terrain		-0.094	-0.097		-0.084	-0.088		-0.101	-0.103
		(0.092)	(0.102)		(0.093)	(0.103)		(0.092)	(0.102)
No. excluded ethnic groups		-0.052	-0.015		-0.049	-0.012		-0.053	-0.013
		(0.069)	(0.070)		(0.069)	(0.069)		(0.070)	(0.070)
Urban area		-0.023	-0.015		-0.024	-0.018		-0.020	-0.014
		(0.018)	(0.017)		(0.017)	(0.016)		(0.016)	(0.017)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	0.211	0.211	0.211	0.211	0.211	0.211	0.211	0.211	0.211
Population density (log)		0.051*	0.035*		0.050*	0.034*		0.053*	0.037*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.130	0.153	0.134	0.130	0.153	0.133	0.129	0.153
R2 Adj.	0.131	0.127	0.148	0.131	0.127	0.148	0.131	0.126	0.148

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.Data limited to state-based violence.

Table F.3: Subgroup analysis: one-sided violent events initiated by non-state forces

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.423***	0.370***	0.345***	0.288*	0.262*	0.205+	0.009	-0.037	-0.065
	(0.065)	(0.083)	(0.046)	(0.119)	(0.124)	(0.117)	(0.074)	(0.078)	(0.075)
Untreated *vs* Not-yet treated	0.051	0.087+	0.066*	0.172+	0.179+	0.136	0.065	0.081	0.048
	(0.042)	(0.044)	(0.031)	(0.090)	(0.102)	(0.087)	(0.064)	(0.073)	(0.068)
FDI jobs created	-0.304***	-0.291***	-0.312***	0.010+	0.004	0.009	0.046***	0.032**	0.046***
	(0.033)	(0.046)	(0.046)	(0.006)	(0.009)	(0.012)	(0.011)	(0.011)	(0.011)
Mountainous terrain		-0.094	-0.097		-0.084	-0.088		-0.101	-0.103
		(0.092)	(0.102)		(0.093)	(0.103)		(0.092)	(0.102)
No. excluded ethnic groups		-0.052	-0.015		-0.049	-0.012		-0.053	-0.013
		(0.069)	(0.070)		(0.069)	(0.069)		(0.070)	(0.070)
Urban area		-0.023	-0.015		-0.024	-0.018		-0.020	-0.014
		(0.018)	(0.017)		(0.017)	(0.016)		(0.016)	(0.017)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	1.427	1.427	1.427	1.427	1.427	1.427	1.427	1.427	1.427
Population density (log)		0.051*	0.035*		0.050*	0.034*		0.053*	0.037*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.130	0.153	0.134	0.130	0.153	0.133	0.129	0.153
R2 Adj.	0.131	0.127	0.148	0.131	0.127	0.148	0.131	0.126	0.148

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001
All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.Data limited to one-sided violence against civilians initiated by non-state forces.

Table F.4: Subgroup analysis: one-sided violent events initiated by state forces

	Extractive FDI			Industry FDI			Services FDI		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Treated *vs* Not-yet treated	0.423***	0.370***	0.345***	0.288*	0.262*	0.205+	0.009	-0.037	-0.065
	(0.065)	(0.083)	(0.046)	(0.119)	(0.124)	(0.117)	(0.074)	(0.078)	(0.075)
Untreated *vs* Not-yet treated	0.051	0.087+	0.066*	0.172+	0.179+	0.136	0.065	0.081	0.048
	(0.042)	(0.044)	(0.031)	(0.090)	(0.102)	(0.087)	(0.064)	(0.073)	(0.068)
FDI jobs created	-0.304***	-0.291***	-0.312***	0.010+	0.004	0.009	0.046***	0.032**	0.046***
	(0.033)	(0.046)	(0.046)	(0.006)	(0.009)	(0.012)	(0.011)	(0.011)	(0.011)
Mountainous terrain		-0.094	-0.097		-0.084	-0.088		-0.101	-0.103
		(0.092)	(0.102)		(0.093)	(0.103)		(0.092)	(0.102)
No. excluded ethnic groups		-0.052	-0.015		-0.049	-0.012		-0.053	-0.013
		(0.069)	(0.070)		(0.069)	(0.069)		(0.070)	(0.070)
Urban area		-0.023	-0.015		-0.024	-0.018		-0.020	-0.014
		(0.018)	(0.017)		(0.017)	(0.016)		(0.016)	(0.017)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country time-trend			Yes			Yes			Yes
Mean of outcome	1.123	1.123	1.123	1.123	1.123	1.123	1.123	1.123	1.123
Population density (log)		0.051*	0.035*		0.050*	0.034*		0.053*	0.037*
		(0.020)	(0.016)		(0.020)	(0.017)		(0.020)	(0.016)
Num.Obs.	22480	15503	15503	22480	15503	15503	22480	15503	15503
R2	0.133	0.130	0.153	0.134	0.130	0.153	0.133	0.129	0.153
R2 Adj.	0.131	0.127	0.148	0.131	0.127	0.148	0.131	0.126	0.148

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

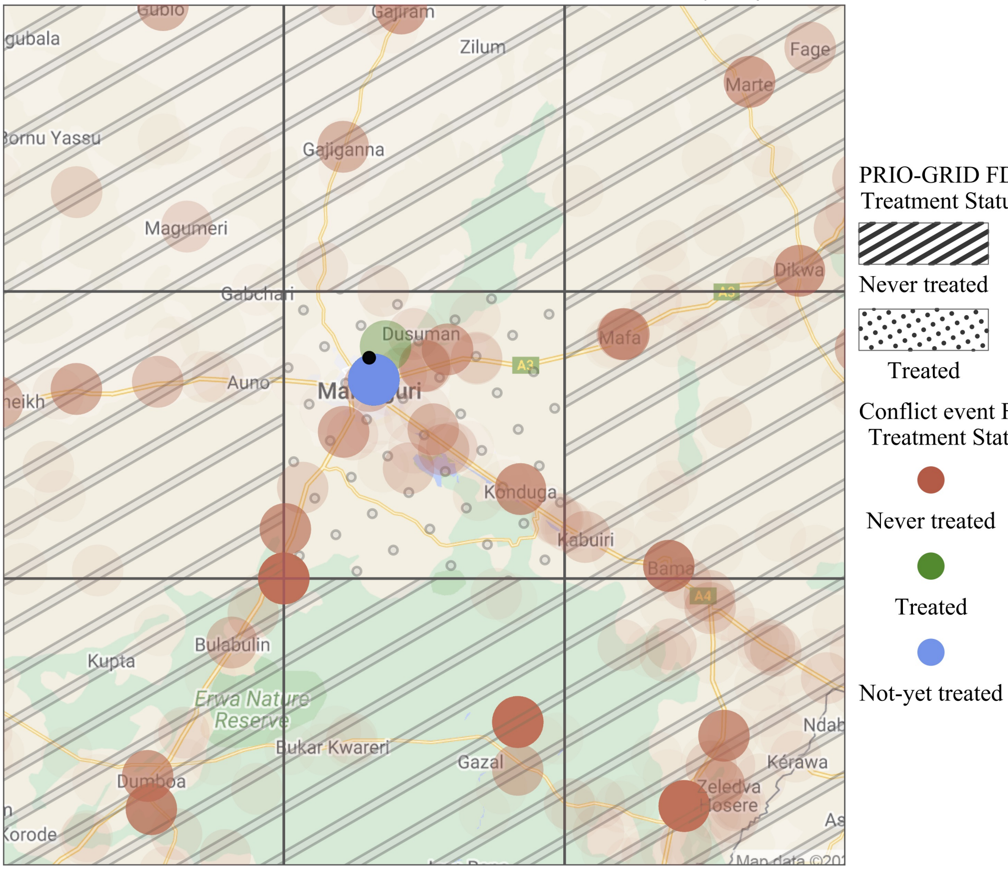
All models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Unit of analysis is a geolocated conflict event in a given year. All standard errors are clustered at the country level. A unit is defined as treated under a given FDI type if there is an FDI of that type within 5 km of it, at present. Not-yet treated units are defined considering the same distance in space but look at whether an FDI of that type will be opened at any point in the future in our data.Data limited to one-sided violence against civilians initiated by state forces.

G Alternative research design and data source: panel of PRIO-GRID cells

We show that we can obtain similar estimates for the effects of FDI on violence against civilians even if we aggregate information in a different data structure, modelled in a different research design, at a different (lower) level of resolution, and even by using alternative data sources. We construct a panel of PRIO-GRID 50×50 km cells observed yearly between 2003 and 2019. We leverage this analysis for making three points. First, that our findings do not hinge on the data structure, research design, level of resolution, and data source (including their coding conventions) that we adopt in our main analysis. Second we note that, by using cells as statistical units, we observe how receiving an FDI changes patterns of violence in places that did not experience any violence in previous time points. Finally we note that, from the point of view of spatial precision, this dataset is defined at a much more highly aggregated unit than that used by our main analysis—which looks at a 5 km radius around a conflict event. Thus, this analysis allows us to reduce measurement error introduced when geolocating FDI projects, if that procedure had a lower resolution than 5 km (akin to the test in Figure E.1 for larger radii).

Figure G.1 exemplifies the difference in resolution for the two designs by representing both in the case of the Nigerian city of Maiduguri, capital of the north-eastern Borno State. We overlay the map of the area with: the 5 km radius buffers around conflict events, colored as

Figure G.1: City of Maiduguri, North-East Nigeria. Comparison of resolutions of our main analysis at the level of 5 km buffers around conflict events and of the 50×50 km PRIO-GRID cell-year panel data.



Note: Colored bubbles are the 5 km radius buffers around conflict events used in our main analysis. They are colored by FDI treatment status. Black dots report geolocated FDI projects. Overlayed are 50×50 km PRIO-GRID cells, with filling paterns based on whether they are treated with an FDI project.

Source: authors' compilation.

in Figure 2; the FDI project in the area from fDi Markets data—black dot; and the PRIO-GRID cells, with filling patterns defined based on whether the cell is ever treated in this new design. The research design in our main analysis considers a much more fine-grained set of geographical units as treated and not-yet treated: many conflict events in the "treated" cell are in fact untreated when we consider buffers of 5 km radius.

We build a cell-year panel comprising the universe of 10,671 African PRIO-GRID cells observed yearly between 2003 and 2019 (observations: 181,407). We merge these cells with our geolocated data on FDI projects. We code three binary treatment variables (distinguishing extractive, industry, and services FDI) taking value 1 if and after a cell experiences at least one FDI of a given type, 0 otherwise.

Next, we build a series of dependent variables measuring violence against civilians. All variables are sums and we take the logarithm of the sum +1, consistently with our main analysis. Our first dependent variable is the sum of civilian casualties in conflict events reported by UCDP GED in a given cell-year. We include this variable for showing that we can obtain similar estimates as those in our main analysis if we aggregate data differently and model them in a different design.

We also build several dependent variables that measure *number of deliberate attacks* against civilians. To this end, we also draw on a different conflict data source—the Armed Conlict Location & Event Data (ACLED)—to show that our findings do not hinge on our main source of conflict data—UCDP GED.[3] ACLED data, moreover, conveniently offer a nuanced characterization of who initiates attacks—unlike UCDP GED—which we exploit below. We draw from ACLED information on conflict events happening in Africa between 2003 and 2019. We keep only cases of political violence (we exclude protests), in particular deliberate attacks targeting civilians. Finally, we keep only events coded by ACLED as having the most precise information on time and geolocation. Drawing on spatial and temporal information, we assign the remaining 24,239 ACLED data points to a specific PRIO-GRID cell-year.

We aggregate UCDP and ACLED data at the cell-year level and measure four pairs of dependent variables counting number of deliberate attacks against civilians (all logarithms of counts + 1), using UCDP and ACLED respectively:

1. The number of attacks initiated by any group.
2. The number of attacks initiated by government forces.
3. The number of attacks initiated by rebel forces.
4. The number of attacks initiated by political militias[4] (ACLED only).

We study these dependent variables in a series of fixed-effect (FE) models. We present three models for each type of FDI and each dependent variable. First, we remove all between-country and between-year variation by fitting FEs at these levels. Next, we remove all between-cell variation and study over-time changes within a cell as a function of receiving an FDI. Finally, we fit a cell and year FE model, to also remove between-year confounders. All estimates report standard errors clustered at the cell-level, as this is the geographical unit where the "treatment" (an FDI project) occurs.

[3] ACLED data are not suited to the study of number of civilian fatalities in violent attacks because, unlike UCDP data, they do not distinguish between fatalities belonging to different groups. Thus, we cannot use them in the research design we presented in the main text.

[4] These are generally considered to be violent actors that are supported and armed by state forces.

Table G.1: The effect of FDI on civilian casualties, panel of PRIO-GRID cells

	Extractive FDI			Industry FDI			Services FDI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDI project	0.060**	0.058*	0.043+	0.066**	-0.006	-0.022	0.095***	0.015	-0.001
	(0.020)	(0.024)	(0.024)	(0.023)	(0.025)	(0.025)	(0.020)	(0.022)	(0.022)
Country FE	Yes			Yes			Yes		
Cell FE		Yes	Yes		Yes	Yes		Yes	Yes
Year FE	Yes		Yes	Yes		Yes	Yes		Yes
Mean of outcome	0.035	0.035	0.035	0.035	0.035	0.035	0.035	0.035	0.035
Num.Obs.	181407	181407	181407	181407	181407	181407	181407	181407	181407
R2	0.028	0.257	0.258	0.029	0.257	0.258	0.030	0.257	0.258
R2 Adj.	0.028	0.210	0.212	0.029	0.210	0.212	0.029	0.210	0.212

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001
Notes: Models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Number of civilian casualties data are drawn from UCDP GED. Unit of analysis is a 50km x 50km PRIO-GRID cell observed in a given year. Standard errors are clustered at the cell level.

In Table G.1 we find that extractive FDI has a positive and significant effect on the number of civilian casualties (measured from UCDP data). In Table G.2 we replicate the analysis studying the number of deliberate attacks against civilians using UCDP data, where we find similar estimates. Next, we study the number of attacks against civilians recorded by UCDP and initiated by government forces (Table G.3) and rebels (Table G.4). We find that FDI of any type increases significantly the number of attacks staged by government forces but not by rebels.

Table G.2: The effect of FDI on number of attacks against civilians (UCDP DATA), panel of PRIO-GRID cells

	Extractive FDI			Industry FDI			Services FDI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDI project	0.034**	0.036*	0.026+	0.040**	0.006	-0.004	0.061***	0.018	0.008
	(0.012)	(0.015)	(0.015)	(0.014)	(0.014)	(0.013)	(0.013)	(0.012)	(0.012)
Country FE	Yes			Yes			Yes		
Cell FE		Yes	Yes		Yes	Yes		Yes	Yes
Year FE	Yes		Yes	Yes		Yes	Yes		Yes
Mean of outcome	0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017
Num.Obs.	181407	181407	181407	181407	181407	181407	181407	181407	181407
R2	0.035	0.290	0.292	0.036	0.290	0.292	0.038	0.290	0.292
R2 Adj.	0.035	0.246	0.247	0.036	0.245	0.247	0.037	0.245	0.247

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001
Notes: Models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Dependent variable data are drawn from UCDP. Unit of analysis is a 50km x 50km PRIO-GRID cell observed in a given year. Standard errors are clustered at the cell level.

Table G.3: The effect of FDI on number of attacks against civilians staged by government forces (UCDP data), panel of PRIO-GRID cells

	Extractive FDI			Industry FDI			Services FDI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDI project	0.027**	0.021+	0.018	0.031***	0.012*	0.009+	0.050***	0.022**	0.020**
	(0.009)	(0.012)	(0.012)	(0.009)	(0.006)	(0.006)	(0.009)	(0.007)	(0.007)
Country FE	Yes			Yes			Yes		
Cell FE		Yes	Yes		Yes	Yes		Yes	Yes
Year FE	Yes		Yes	Yes		Yes	Yes		Yes
Mean of outcome	0.008	0.008	0.008	0.008	0.008	0.008	0.008	0.008	0.008
Num.Obs.	181407	181407	181407	181407	181407	181407	181407	181407	181407
R2	0.030	0.240	0.241	0.032	0.240	0.241	0.035	0.241	0.241
R2 Adj.	0.030	0.193	0.194	0.031	0.193	0.194	0.034	0.193	0.194

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Dependent variable data are drawn from UCDP. Unit of analysis is a 50km x 50km PRIO-GRID cell observed in a given year. Standard errors are clustered at the cell level.

Table G.4: The effect of FDI on number of attacks against civilians staged by rebel forces (UCDP data), panel of PRIO-GRID cells

	Extractive FDI			Industry FDI			Services FDI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDI project	0.006	0.008	0.002	0.013	-0.010	-0.017	0.018*	-0.004	-0.012
	(0.008)	(0.013)	(0.013)	(0.010)	(0.014)	(0.014)	(0.008)	(0.012)	(0.012)
Country FE	Yes			Yes			Yes		
Cell FE		Yes	Yes		Yes	Yes		Yes	Yes
Year FE	Yes		Yes	Yes		Yes	Yes		Yes
Mean of outcome	0.011	0.011	0.011	0.011	0.011	0.011	0.011	0.011	0.011
Num.Obs.	181407	181407	181407	181407	181407	181407	181407	181407	181407
R2	0.024	0.260	0.261	0.024	0.260	0.261	0.025	0.260	0.261
R2 Adj.	0.024	0.213	0.215	0.024	0.213	0.215	0.024	0.213	0.215

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Dependent variable data are drawn from UCDP. Unit of analysis is a 50km x 50km PRIO-GRID cell observed in a given year. Standard errors are clustered at the cell level.

Table G.5 shows similar results when studying ACLED data. Extractive, industry, and services FDI have positive and sizeable effects on the number of deliberate attacks targeting civilians. In Tables G.6 and G.8 we find that these effects are confirmed (for all types of FDI) when studying, respectively, the number of attacks initiated by government forces and political militias. Instead, we do not find strong evidence that FDI increases number of attacks initiated by rebel forces (Table G.7).

Table G.5: The effect of FDI on number of attacks against civilians (ACLED DATA), panel of PRIO-GRID cells

	Extractive FDI			Industry FDI			Services FDI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDI project	0.209***	0.145***	0.113***	0.252***	0.095***	0.064***	0.309***	0.105***	0.073***
	(0.035)	(0.028)	(0.028)	(0.030)	(0.019)	(0.019)	(0.030)	(0.019)	(0.018)
Country FE	Yes			Yes			Yes		
Cell FE		Yes	Yes		Yes	Yes		Yes	Yes
Year FE	Yes		Yes	Yes		Yes	Yes		Yes
Mean of outcome	0.044	0.044	0.044	0.044	0.044	0.044	0.044	0.044	0.044
Num.Obs.	181407	181407	181407	181407	181407	181407	181407	181407	181407
R2	0.101	0.445	0.451	0.115	0.445	0.451	0.124	0.445	0.451
R2 Adj.	0.100	0.410	0.417	0.114	0.410	0.417	0.124	0.410	0.417

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Notes: Models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Dependent variable data are drawn from ACLED. Unit of analysis is a 50km x 50km PRIO-GRID cell observed in a given year. Standard errors are clustered at the cell level.

Table G.6: The effect of FDI on number of attacks against civilians staged by government forces (ACLED data), panel of PRIO-GRID cells

	Extractive FDI			Industry FDI			Services FDI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDI project	0.109***	0.080***	0.071**	0.122***	0.043**	0.033*	0.154***	0.054***	0.045***
	(0.021)	(0.023)	(0.023)	(0.018)	(0.013)	(0.013)	(0.019)	(0.013)	(0.013)
Country FE	Yes			Yes			Yes		
Cell FE		Yes	Yes		Yes	Yes		Yes	Yes
Year FE	Yes		Yes	Yes		Yes	Yes		Yes
Mean of outcome	0.012	0.012	0.012	0.012	0.012	0.012	0.012	0.012	0.012
Num.Obs.	181407	181407	181407	181407	181407	181407	181407	181407	181407
R2	0.051	0.355	0.358	0.064	0.354	0.357	0.075	0.355	0.358
R2 Adj.	0.051	0.315	0.317	0.063	0.314	0.317	0.074	0.315	0.317

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Notes: Models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Dependent variable data are drawn from ACLED. Unit of analysis is a 50km x 50km PRIO-GRID cell observed in a given year. Standard errors are clustered at the cell level.

Table G.7: The effect of FDI on number of attacks against civilians staged by rebel forces (ACLED data), panel of PRIO-GRID cells

	Extractive FDI			Industry FDI			Services FDI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDI project	0.004	0.000	-0.005	0.017+	-0.013	-0.019+	0.016*	-0.009	-0.015
	(0.005)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.008)	(0.012)	(0.012)
Country FE	Yes			Yes			Yes		
Cell FE		Yes	Yes		Yes	Yes		Yes	Yes
Year FE	Yes		Yes	Yes		Yes	Yes		Yes
Mean of outcome	0.008	0.008	0.008	0.008	0.008	0.008	0.008	0.008	0.008
Num.Obs.	181407	181407	181407	181407	181407	181407	181407	181407	181407
R2	0.028	0.275	0.276	0.029	0.275	0.277	0.029	0.275	0.277
R2 Adj.	0.028	0.230	0.231	0.028	0.230	0.231	0.028	0.230	0.231

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Dependent variable data are drawn from ACLED. Unit of analysis is a 50km x 50km PRIO-GRID cell observed in a given year. Standard errors are clustered at the cell level.

Table G.8: The effect of FDI on number of attacks against civilians staged by political militias (ACLED data), panel of PRIO-GRID cells

	Extractive FDI			Industry FDI			Services FDI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FDI project	0.143***	0.098***	0.082**	0.173***	0.071***	0.055***	0.211***	0.087***	0.071***
	(0.026)	(0.025)	(0.025)	(0.024)	(0.016)	(0.016)	(0.023)	(0.015)	(0.015)
Country FE	Yes			Yes			Yes		
Cell FE		Yes	Yes		Yes	Yes		Yes	Yes
Year FE	Yes		Yes	Yes		Yes	Yes		Yes
Mean of outcome	0.024	0.024	0.024	0.024	0.024	0.024	0.024	0.024	0.024
Num.Obs.	181407	181407	181407	181407	181407	181407	181407	181407	181407
R2	0.089	0.420	0.424	0.102	0.420	0.424	0.110	0.421	0.424
R2 Adj.	0.089	0.384	0.388	0.101	0.384	0.387	0.109	0.385	0.388

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Models are linear regressions estimated using OLS. Fixed effects are fitted by de-meaning the dependent variable. Dependent variable is a logged version of the count + 1. Dependent variable data are drawn from ACLED. Unit of analysis is a 50km x 50km PRIO-GRID cell observed in a given year. Standard errors are clustered at the cell level.