The Impact of Conflicts on Foreign Investments: Evidence from Project-level Data

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Abstract

This paper examines how armed conflict affects foreign direct investment using monthly conflict fatality data matched with project-level greenfield foreign direct investment (FDI) and cross-border mergers and acquisitions (M&As) during 2003–2023. The unexpected emergence of conflict events of more than 1,000 fatalities is associated with a decline in the number of FDI mergers and acquisitions, as well as in greenfield projects, associated jobs, capital expenditure, and the number of origin countries and sectors of FDI. The estimates are consistent with FDI responding to realized, high-intensity violence rather than to increases in conflict risk alone. When we compare increases in conflict risk that materialize into large fatalities to those that turn out to be false alarms, we find that only large fatalities lead to drops in FDI. Reduced FDI may be one channel through which severe conflict contributes to persistent economic losses and development traps.

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Not long ago, we lived in one of the most peaceful eras in modern history. 2005 saw the fewest recorded deaths in armed conflict since the second world war, according to the Uppsala Conflict Data Program. How times change. Last year, there were 61 "state-based conflicts", the most since 1946, reports the Peace Research Institute Oslo.

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1 Introduction

Beyond death and destruction, violent conflict often depresses economic activity and livelihoods for decades after hostilities subside, keeping countries trapped in poverty (Collier, 2007; Blattman and Miguel, 2010). Early cross-country studies such as Collier (1999) have shown that conflict weakens growth, slows capital accumulation, and erodes institutional capacity. Federle et al. (2024) find GDP declines of 30% in affected areas within the first 5 years of conflict, while de Groot et al. (2022) estimate that global GDP in 2014 would have been 12% higher absent post-1970 violent episodes. Micro-level work links conflict to reduced investments by households, firms and government (Di Maio and Sunde, 2025), poorer outcomes in health (Bundervoet et al., 2009) and education (Salah and Saxena, 2025; Brück et al., 2019), and in firm performance (Brancati et al., 2024) and — though with surprisingly muted long-run effects in some contexts — local consumption and living standards. Conflicts also reverberate beyond national borders. They affect stock markets around the world (Guidolin and Ferrara, 2010; Boungou and Yatié, 2022), disrupt international trade (Martin et al., 2008; Glick and Taylor, 2010), and shape multinational firms' decisions about where and how to allocate capital (Blair et al., 2022; Li et al., 2017).

In this paper, we revisit the relationship between armed conflict and foreign direct investment (FDI) using comprehensive project-level data spanning 2003–2023 and new difference-in-difference methods. We measure conflict intensity using event-level data

on conflict-related deaths from the Uppsala Conflict Data Program (Sundberg and Melander, 2013) and relate it to two complementary measures of multinational activity: (1) greenfield projects from *fDiMarkets*, which allow us to look into jobs created, capital expenditure (capex), and the diversity of sectors and source countries, and (2) cross-border mergers and acquisitions from *Refinitiv Eikon*. This combination provides complementary project- and transaction-level measures of multinational investment activity across countries as well as of their sectoral diversity. Although a recent review by Blair et al. (2022) identified 75 peer-reviewed studies examining the FDI-conflict nexus, revisiting this relationship with up-to-date data and recent methods is warranted for several reasons.

First, armed conflict has intensified globally, with major wars in Ukraine, the Syrian Arab Republic, and elsewhere generating unprecedented violence. Second, many of the studies Blair et al. (2022) identify rely on broad political-risk indices rather than measures of conflict itself, most rest on cross-country correlations, and the evidence is mixed. It is not yet clear whether FDI is mostly affected by political risk or the intensity of violence. Third, new methods for identifying causal effects with difference-in-difference estimators in panel settings with staggered treatment have emerged (Baker et al., 2026), as well as new conflict forecast tools (Mueller et al., 2024). These allow us to allow us to strengthen causal identification by examining investment dynamics around abrupt, large violence shocks and testing for pre-existing trends, as well as dig into the risk-vs-violence mechanisms. Finally, we think this is also an important issue given FDI's critical role in driving productivity over long horizons, notably via technology and management transfer, and job creation (UNCTAD, 2024), and thus quantifying its responsiveness to conflict helps identify one of the mechanisms through which conflict has persistent economic consequences.

In our main specification, we estimate the effect of unexpected violence bursts, i.e. 1,000 conflict fatalities over a 3-month period preceded by 12 peaceful months, on FDI inflows, by comparing FDI levels in affected countries to those in peaceful

countries over a two-year period around the violent events. Our difference-in-differences estimates, based on the LP-DiD estimator of Dube et al. (2025), indicate that large and unexpected bursts of violence reduce foreign investment substantially, but only when they are large. In the twelve months following a burst of at least 1,000 fatalities that exceeds the preceding year's total tenfold, greenfield projects fall by roughly 40%, and so does the number of distinct sectors receiving greenfield investment and the number of distinct origin countries, while announced jobs and capital expenditure by around 75%. We assess the robustness of this effect across different fatality thresholds and find that at lower fatality counts, from 25 to 500 deaths over three months, the estimated effects are small and statistically indistinguishable from zero, whatever the degree of surprise involved.

To assess the robustness of this results, we re-estimate the entire specification grid using a Poisson pseudo-maximum likelihood estimator adapted to the LP-DiD setting and find similar negative effects on greenfield projects, while we also detect negative responses at lower fatality thresholds. This may be due to this estimator placing greater effective weight on countries with larger expected project counts.

We also re-estimate the entire specification grid by defining unexpected violence bursts as those that were surprises against what forecasts predicted rather than against a country's own recent history, and find similar effects.

As an illustration of the magnitudes involved, a synthetic control analysis of Syria suggests that the civil war cost the country between two and five thousand announced FDI-related jobs per year over 2011–2017.

In a second empirical exercise, we show that this deterrent effect operates through violence rather than through the anticipation of it. Using monthly conflict forecasts from Mueller et al. (2024), we isolate episodes in which the assessed probability of armed conflict in a previously calm country rose by at least twenty-five percentage points, and split them by what followed. Where the alarm proved false, we do not

detect statistically significant changes in the measured outcomes. Where the risk materialized and the country witnessed 1,000 fatalities over a 3-month period in the following year, all outcomes fall, with the clearest and most precisely estimated decline for greenfield projects; estimates for the other margins are generally negative although less precise. This result speaks to the risk-vs-violence mechanism, and suggests that publicly assessed deterioration in a country's conflict outlook, absent actual violence, may leave foreign investment unaffected, and it is thus the actual violence that deters FDI.

Our paper contributes to the literature on the determinants of FDI across countries (Blonigen and Piger, 2014; Florez-Orrego et al., 2024), with a focus on the effect of conflict on FDI. As noted by Blair et al. (2022), the existing literature on armed conflict and FDI has found mixed results, predominantly using annual aggregate flows and broad measures of political risk to capture the role of conflict. Previous studies of conflict and FDI include Li et al. (2017), who find that FDI flows to developing countries fall during and after civil wars, Busse and Hefeker (2007) who suggest that the absence of internal conflict is among the political-risk components most closely associated with FDI inflows to developing countries, Barry (2018) who use data on the foreign operations of 77 major multinationals in developing countries and suggests that conflict deters foreign investment mainly at the entry stage, and Blair et al. (2022) who use project-level data from the mining sector and show that firms operating at conflict sites cut investment sharply, while firms in the same country but away from the fighting increase it, plausibly because conflict weakens government oversight.

We contribute by combining monthly project-level greenfield announcements with completed cross-border acquisitions spanning the last two decades (2003–2023) across the world, exploiting abrupt violence bursts in an event-study design, and distinguishing forecast conflict risk from realized high-intensity violence. Focusing on large and unexpected rises in conflict-driven fatalities in a difference-in-differences setting allows for a causal interpretation under parallel trends and no anticipation. This point is

particularly important in light of studies that suggest that FDI may itself cause conflict. Examples here include Mihalache-O'Keef (2018) and Pinto and Zhu (2022), who suggest FDI can increase the probability of civil conflict across countries, and Sonno (2025) and Brazys et al. (2025) who use firm-level data to show that FDI can increase conflict at the local level across Africa. Our estimates can contribute to the broad literature quantifying the economic costs of violent conflict at both the micro (Di Maio and Sunde, 2025) and the macro (Federle et al., 2024) levels.

To put the magnitude of the FDI response within the wider literature on the economic costs of conflict, Di Maio and Sunde (2025) place estimates of the macro-level effect at between 1 and 4 percent of GDP per year of conflict. More recent work by Federle et al. (2024) documents a 10% drop in GDP in war-affected countries roughly four years after onset, alongside a capital stock falling by more than 4% within eight years and consumer prices rising by approximately 20%. Chupilkin and Kóczán (2022) report GDP per capita losses above 7 percentage points for wars fought on national territory, and Abadie and Gardeazabal (2003) estimate a permanent fall of around 10 percentage points in Basque per capita GDP after the onset of terrorism. Firm-level evidence points from Del Prete et al. (2023) find that conflict events near a firm in Libya reduce revenues by 1.4%, while Brancati et al. (2024) find that across a large set of countries, a 50% increase in conflict events lowers firms' sales by roughly 8%. Our estimate is larger in proportional terms than any of these. Greenfield project announcements fall by roughly 40% within a year of an unexpected burst of violence. These numbers are not directly comparable, however. FDI projects are few and volatile, and a 40% drop can correspond to losing only one or two projects. The other studies also how much firms and output scale back under violence, whereas we estimate whether foreign firms enter at all. Foregone FDI plausibly contributes to the persistence of lower output that Federle et al. (2024) document, with recovery still incomplete more than a decade after onset.

The remainder of the paper is organized as follows. Section 2 details our data

and empirical methodology, and Section 3 presents the results, with robustness checks included in the Appendix. The last section provides concluding remarks.

2 Data and Methodology

2.1 Data on conflict

We use data on conflict from the Uppsala Conflict Data Program (Sundberg and Melander, 2013). We focus on battle-related deaths to capture conflict intensity. These refer to deaths caused by warring parties that can be directly related to combat, and include battlefield fighting, guerrilla activities (e.g., hit-and-run attacks and ambushes), and all kinds of bombardments of military bases, cities, and villages. The target for the attacks can military forces or representatives for the parties, as well as mass killings of civilians being killed in crossfire and indiscriminate bombings. All fatalities—military as well as civilian—are included in our measure, and whether they are part of state-based conflicts, non-state conflicts, and one-sided violence. State-based conflicts include interstate wars (such as the Russian Federation's invasion of Ukraine) and civil wars (such as in the Syrian Arab Republic), non-state conflicts encompass violence between organized armed groups such as drug cartels in Latin America, while one-sided violence includes terrorist attacks and genocides.

Fatalities are not the only channel through which conflict may affect FDI, but they make them a good measure for our purposes. First, they are an observable indicator of the intensity of conflict-related disruption. Second, they are recorded at a high frequency and on a consistent basis across countries. Third, they are widely reported internationally by news outlets, so that firms and investors observe them in close to real time, and can hence plausibly drive investor decisions.

We also use data on conflict risk using armed conflict forecast data from (Mueller et al. (2024)), who develop a machine learning model that analyses global news text to

generate monthly, country-level predictions of conflict onset within the subsequent 12 months. The average risk of armed conflict among peaceful countries is approximately 6%, ranging from 1% to 12%. In recent years, conflict risk in previously peaceful countries has increased from a range of 2%-5% to higher levels.

Table 1 presents key characteristics of the 40 most conflict-affected countries during 2003–2023, ranked by total conflict deaths. The Syrian Arab Republic experienced the highest fatalities with over 400,000 deaths, followed by Ethiopia (328,303) and Afghanistan (244,139). The table reveals that for a substantial group of countries, conflict is not an episode but a condition. Among these conflict-affected countries, many saw very few years of peace during 2003–2023. Afghanistan, Ethiopia, Nigeria, DR Congo, Sudan, Pakistan, Colombia, Myanmar, Iraq and Somalia exceeded 25 deaths per year every year. Conflict risk is above 50% on average in most of these countries. Most countries here experience conflict in as a constant feature rather than isolated events of violence during 2003-2023. As these countries often suffer from a range of other development problems at the same time as conflict, it is not easy to associate their FDI patterns to their conflict level. Our estimation strategy will instead use variation from countries where violence erupted sharply and unexpectedly after a prolonged period of peace. As conflict is often persistent in the most affected countries, there is no counterfactual peacetime baseline to compare conflcit years to. The use of event studies thus limits us to isolated cases where we need peaceful windows before fatalities burst.

2.2 Data on FDI

Our empirical analysis draws on two complementary sources of project-level FDI data covering 2003–2023. For greenfield investments, we use fDiMarkets, a Financial Times database that tracks announcements of new foreign direct investment projects, including expansions of existing facilities. This database provides information on investment

Table 1: Conflict exposure of the most affected countries, 2003–2023

Country	Deaths	Risk	Years (%)		Country	Deaths	Risk	Years (%)	
			>25	>1,000				>25	>1,000
Syria	409,695	0.90	62	62	Mali	14,071	0.90	76	29
Ethiopia	328,303	0.76	100	24	Libya	14,001	0.80	48	29
Afghanistan	244,139	0.99	100	86	Burkina Faso	12,972	0.49	38	24
Ukraine	176,952	0.65	48	19	Philippines	11,700	0.75	100	14
Mexico	129,646	0.93	95	81	Azerbaijan	8,723	0.58	43	5
Iraq	95,194	0.99	100	86	Cameroon	8,017	0.69	48	14
Yemen	65,279	0.97	76	52	Turkey	7,971	0.72	95	10
Nigeria	58,236	0.95	100	67	Russia	6,633	0.33	95	5
DR Congo	53,243	0.98	100	71	Nepal	6,194	0.10	24	14
Sudan	49,980	0.98	100	76	Chad	5,997	0.64	76	5
Pakistan	43,690	0.79	100	52	Mozambique	5,473	0.53	43	10
Somalia	40,159	0.99	100	86	Uganda	5,377	0.23	52	10
Israel	34,671	0.80	81	10	Egypt	5,221	0.59	67	0
India	25,057	0.13	100	38	Niger	5,049	0.69	57	5
Sri Lanka	24,118	0.16	38	19	Algeria	4,631	0.59	81	0
Brazil	17,874	0.49	81	33	Kenya	4,475	0.77	90	0
Colombia	17,100	0.78	100	14	Burundi	4,175	0.70	76	5
Myanmar	16,768	0.91	100	19	Thailand	4,045	0.33	100	0
CAR	15,013	0.97	81	19	Liberia	3,284	0.08	5	5
South Sudan	14,549	0.97	62	33	Indonesia	2,801	0.11	43	10

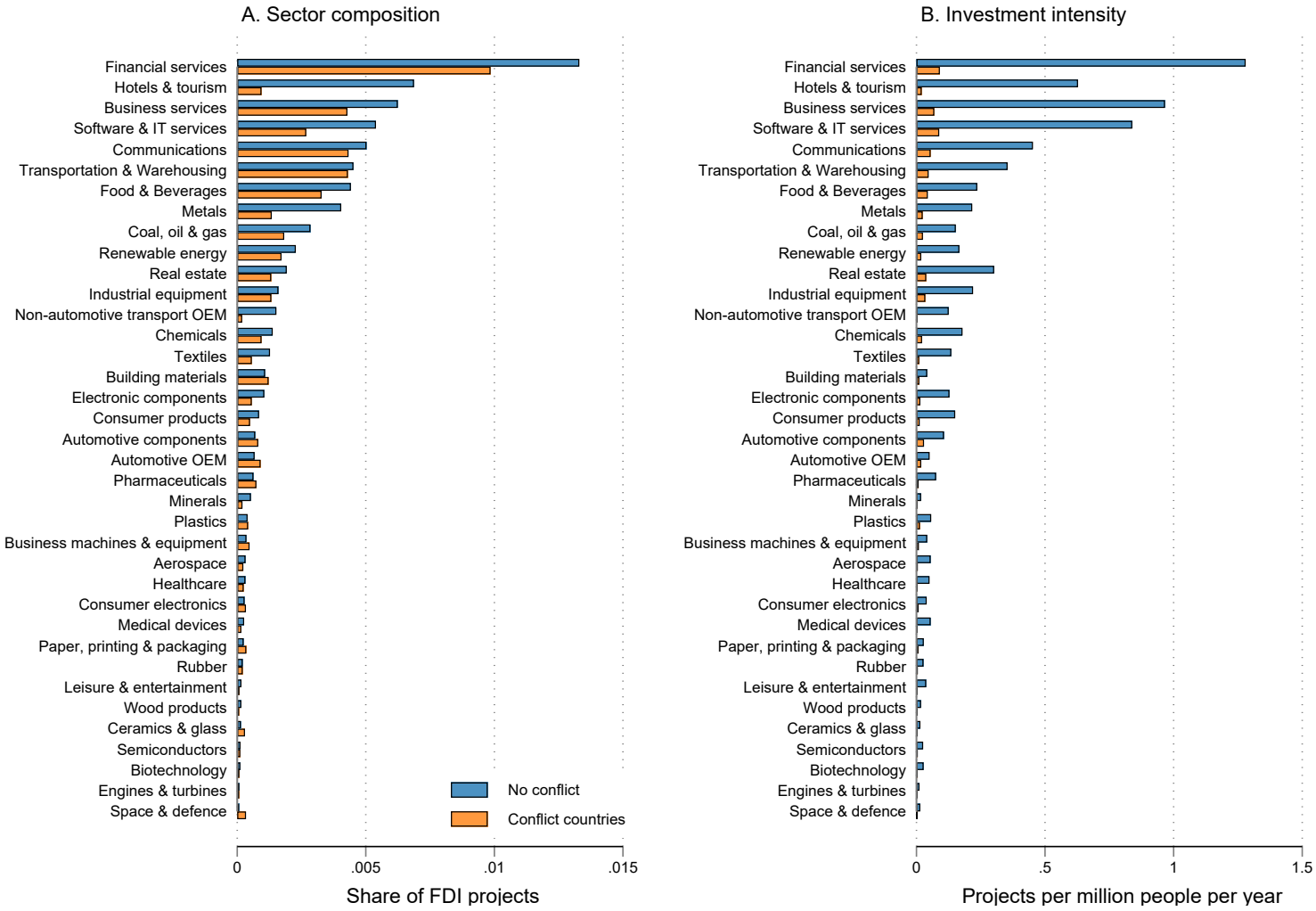
Notes: Countries are ranked by total recorded conflict fatalities over 2003–2023, combining state-based, non-state and one-sided violence. Risk is the mean of the twelve-month armed-conflict onset forecast over the period. The last two columns of each block give the share of the period in which the country recorded more than 25 and more than 1,000 fatalities respectively. Conflict risk = Average yearly armed conflict probability (Mueller et al. (2024)). Years = Temporal coverage (share of years 2003–2023 with conflict). Source: UCDP Georeferenced Event Dataset version 25.1.

value, employment creation, source and destination countries, and sectoral classification for over 200,000 projects globally. The extensive margin of FDI, i.e. the number of projects, provides more precise information than job and capital expenditure numbers which are often estimates, yet those numbers are nonetheless informative. A feature of the fDiMarkets data is that they record announced rather than completed greenfield projects. To the extent that conflict causes announced projects to be abandoned or indefinitely postponed before materializing, our estimates based on announcements may understate the true effect of conflict on realized investment. At the same time, announcement data capture investor intentions and forward-looking sentiment, which are themselves economically meaningful signals of how conflict shapes the global allocation of capital. For cross-border mergers and acquisitions, we use Refinitiv Eikon (formerly Thomson Reuters), which records completed M&A transactions. Using those two sources allow us to understand how conflict shapes both modes of FDI entry.[1]

Figure 1 (Panel A) compares the sectoral distribution of FDI projects between conflict-affected and peaceful countries during 2003–2023 (A country-year is classified as conflict-affected if the country recorded at least 1,000 conflict fatalities in that year or in either of the two preceding years). The composition of FDI is broadly similar across the two groups, with financial services, business services attracting large shares of projects in both contexts. The most striking differences is in hotels & tourism, a sector which receives a substantially smaller share of FDI in conflict countries compared to peaceful ones, in line with a high sensitivity of tourists to security concerns. Panel B shows that investment intensity per capita in conflict-affected countries is roughly a tenth of that elsewhere. Countries free of major conflict record on average 7.3 greenfield FDI projects per million people per year, against 0.8 in conflict-affected country-years. The gap is present in every sector. In business services, the number of projects falls from 0.97 to 0.07 per million, in hotels and tourism from 0.63 to 0.02, and in financial services from 1.28 to 0.09.

[1]One limitation of our data is that we cannot address whether conflict diverts capital toward portfolio flows or other instruments rather than reducing external finance in aggregate.

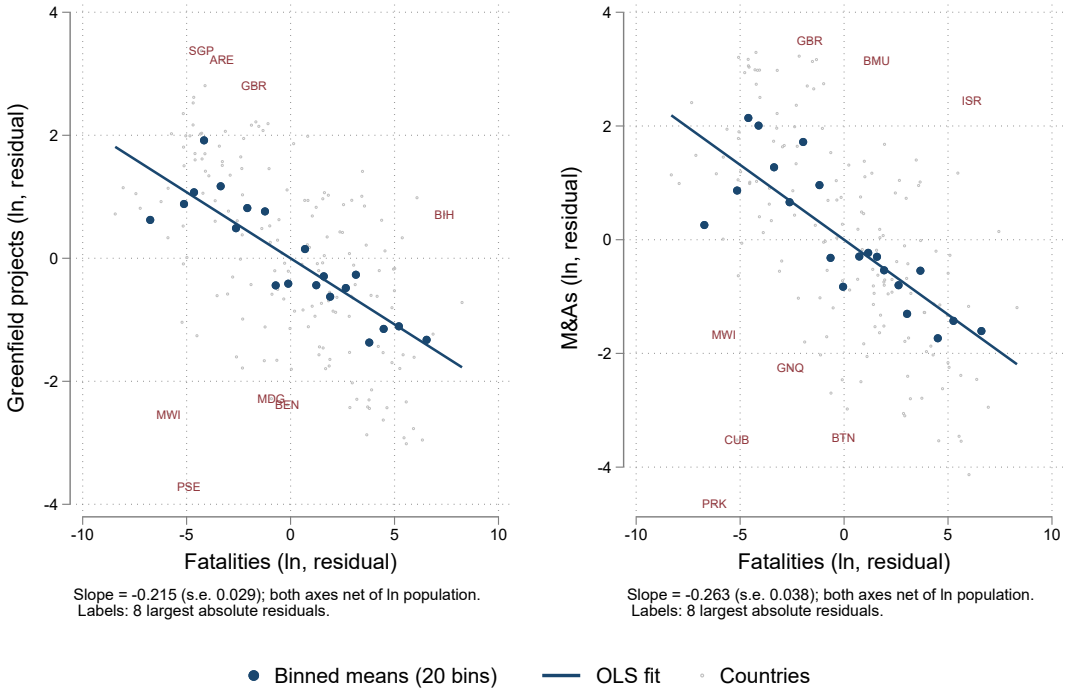
Figure 1: Conflict and FDI, 2003–2023



Notes: Panel A shows the average share of a country's greenfield FDI projects accounted for by each sector; Panel B shows greenfield projects per million people per year. Both panels are computed on the same balanced country × sector × year panel, in which country-sector-years with no recorded projects are set to zero rather than treated as missing. Averages are taken within country first and then across countries, so each country carries equal weight regardless of how many years it contributes. A country-year is classified as conflict-affected if the country recorded at least 1,000 conflict fatalities in that year or in either of the two preceding years. Sectors are ordered by their share in non-conflict country-years, and the same ordering is applied to both panels. Source: authors' calculations from fDiMarkets and Sundberg and Melander (2013); population data is from the World Development Indicators.

Figure 2 shows the relationship between the number of conflict fatalities and measures of FDI across countries on average during 2003–2023. The scatter plots display conditional correlations controlling for population. Countries most affected by violence—such as the Syrian Arab Republic, Afghanistan, and Somalia—have received fewer FDI projects from both greenfield investments and M&As. The regression coefficients suggest that a 10% increase in conflict-related violent deaths is associated with a 2.15% reduction in greenfield projects and a 2.63% reduction in M&As. In a previous working paper version of this study (Grover and Vézina, 2026), we also showed that conflict fatalities that are geographically dispersed within a country are more strongly negatively correlated with FDI, and that FDI is most sensitive to one-sided violence such as civilian massacres, whilst state-based violence matters slightly more than non-state violence. These patterns are merely descriptive in showing the association between conflict intensity and foreign investment across countries. The question we aim to answer remains, what happens when a country experiences an unexpected and large increase in conflict lethal violence?

Figure 2: FDI and conflict deaths across countries



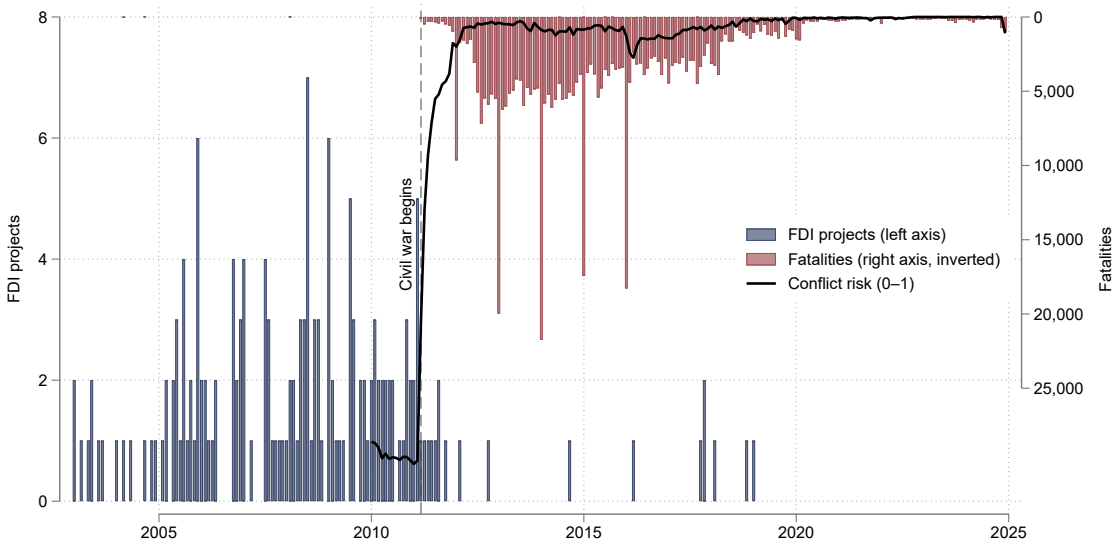
Notes: The scatters show the relationship (in logs) between conflict deaths and greenfield FDI projects or M&As in a cross section of countries, controlling for population. Data on from fDiMarkets, Refinitiv Eikon, and UCDP.

2.3 Methodology

To estimate the effect of conflict on FDI, we focus on unexpected bursts of violence in a difference-in-differences setting. The scatter plots above establish that conflict-affected countries receive less investment, but they cannot say whether conflict causes this. Weak institutions, poor infrastructure and political instability may deter investors while also make violence more likely. In a difference-in-differences design, rather than asking whether violent countries receive less FDI than peaceful ones, we ask whether FDI to a country where violence erupts unexpectedly falls relative to its own pre-violence path, as well as relative to comparable countries that stayed peaceful.

Figure 3 shows the Syrian case, which motivates the empirical design. Navy bars give the monthly count of announced greenfield FDI projects; maroon bars, plotted downward against the inverted right axis, give monthly conflict fatalities; and the black line traces the twelve-month armed-conflict risk forecast. Through the 2000s Syria received a steady if modest flow of FDI projects, with fatalities close to zero and forecast risk low. Fatalities rise from essentially nothing to thousands per month within a year from March 2011 after the outbreak of civil war, and project announcements cease almost entirely. The figure illustrates the main feature our empirical design relies on, namely that violence arrives abruptly in a country that had been calm, and in this case how the collapse in investment coincides with the arrival of fatalities rather than preceding it.

Figure 3: FDI projects and conflict fatalities in Syria



Notes: Bars show monthly greenfield FDI projects (left axis) and conflict fatalities (right axis, inverted). The dashed vertical line marks the outbreak of the civil war (March 2011). The black line shows the 12-month conflict risk forecast (0–1, no axis shown). Source: fDiMarkets and (Sundberg and Melander, 2013).

To estimate the response of FDI to a violence burst we use the local projections difference-in-differences (LP-DiD) estimator of Dube et al. (2025). For each horizon $h = -12, \ldots, 12$ we estimate

$$(1) \quad y_{i,t+h} - \bar{y}_{i,t-1} \;=\; \beta_h \, \Delta B_{it} \;+\; \delta_{th} \;+\; \varepsilon_{i,t+h},$$

where y_{it} is the FDI outcome, B_{it} is an absorbing indicator equal to one from the month of country i's burst onwards, $\Delta B_{it} = B_{it} - B_{i,t-1}$ marks the month of transition into treatment, and δ_{th} is a set of horizon-specific time effects. The dependent variable is expressed relative to $\bar{y}_{i,t-1} = \frac{1}{12}\sum_{j=1}^{12} y_{i,t-j}$, the average of the outcome over the twelve months preceding the burst, rather than to the single month $t-1$; this pre-mean differencing reduces the influence of month-to-month volatility in project announcements, which is considerable in small economies. Each regression is estimated on the subsample of newly treated units and clean controls countries, i.e. never-treated or not yet-treated countries and hence the estimator is free of the negative weighting problems that affect two-way fixed effects designs with staggered adoption (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). Our estimation sample is a balanced panel of country-months running from January 2003 to December 2023. Country-months with no recorded project are set to zero rather than treated as missing. We drop from the control group a country recording a month at or above the fatality floor within the $[-12, +12]$ window, so that the comparison group consists of countries that were and remained peaceful over the relevant horizon. Not-yet-treated country-months of eventually-treated countries remain in the control group until their own event window opens.

The coefficients β_h for $h < 0$ serve as a test of the identifying assumption whether FDI in treated countries was already diverging from that in comparable peaceful countries before violence arrived. Standard errors are clustered by country. Country fixed effects absorb differences in institutions, geography and market size that are fixed over the 24-month period around violence bursts, while year-month fixed effects absorb global shocks to investment such as the financial crisis or the pandemic.

Our method described above works only where violence has a well-defined onset. As Table 1 shows, conflict is chronic in most affected countries, leaving no peaceful pre-period against which a post-conflict path could be compared. We therefore restrict attention to episodes in which large-scale violence arrives suddenly in a country that had been calm, which gives us a clean pre-period as part of the control group. Our estimates thus speak to the effect of an outbreak of violence rather than to the cumulative burden borne by countries at war for two decades, and may be a lower bound on the total cost of conflict.

We track six outcomes at the country-month level, each capturing a different margin along which foreign investment can respond to violence. *Greenfield projects* is the count of new investment announcements recorded by fDiMarkets, our primary measure of the extensive margin of entry. *M&As* is the count of completed cross-border acquisitions from Refinitiv Eikon, capturing the alternative mode of foreign entry. *Origin countries* counts the distinct source economies from which a country received greenfield investment in a given month, and *subsectors* the distinct industries in which projects were announced; both measure the breadth of investment rather than its volume, and a decline in either indicates that a country's investor base or productive base has narrowed even where the project count has not. *Jobs created* and *capital investment* are the employment and capital expenditure figures attached to announced greenfield projects, capturing the size of the commitments being made rather than just their number. Job and capital figures are estimated by fDiMarkets where firms do not disclose them and are therefore measured with more error than the counts, which is one reason we treat the project count as our headline outcome. We take the inverse hyperbolic sine of the outcomes in our baseline estimates ($\text{asinh}(y) = \ln\left(y + \sqrt{y^2+1}\right)$, which is defined at zero and behaves like the logarithm for values above roughly five), as levels differ widely across countries, and we use a Poisson pseudo maximum likelihood adaption of the LP-DiD estimator in robustness checks (Appendix C).

As any definition of what constitutes a violence burst may be arbitrary, we estimate our model for 36 different definitions. Formally, let D_{it} denote conflict fatalities in country i and month t, summing state-based, non-state and one-sided violence. Country i experiences a *violence burst* at month t if

$$(2) \quad \underbrace{\sum_{s=0}^{2} D_{i,t+s} \geq \bar{F}}_{\text{scale}} \quad \text{and} \quad \underbrace{\sum_{s=0}^{2} D_{i,t+s} \geq \rho \sum_{s=1}^{12} D_{i,t-s}}_{\text{surprise}},$$

where $\bar{F}$ is a floor on the absolute number of fatalities recorded over the three-month event window and ρ is the multiple by which those fatalities must exceed the country's total over the preceding twelve months. The floor condition ensures that the event is a major episode of violence rather than a small absolute increase from a low base; the ratio condition ensures that it represents a break from the recent past rather than the continuation of an ongoing war. The thresholds $(\bar{F}, \rho)$ in equation (2) are arbitrary. A low floor admits many small episodes, while a high ratio restricts attention to the sharpest breaks from a country's own history. Rather than defend one pair, we estimate equation (1) for all thirty-six combinations of the two parameters and report the full set of event-study paths for each outcome, in the spirit of the specification-curve approach of Simonsohn et al. (2020). In our baseline we set $\bar{F} = 1{,}000$ (in line with Correlates of War definition of a war (Stinnett et al., 2002)) and $\rho = 10$, and we report every combination of $\bar{F} \in \{25, 50, 100, 500, 1{,}000, 2{,}000\}$ and $\rho \in \{2, 3, 5, 10, 15, 20\}$.

We summarize the results in our event study graphs as well as in heat plots that give the average coefficient over horizons 0 to 12 for each specification. In Appendix D we show results using an alternative definition of unexpected violence, where we defined unexpected violence bursts as those that were not predicted by the conflict risk measure of Mueller et al. (2024). In Appendix G, we estimate whether such violence bursts also affects FDI flows to neighboring countries.

Importantly, in section 3.2 we check whether the deterrent effect of conflict on FDI operates through the intensity of violence or through an increase in country risk, using monthly conflict forecasts from Mueller et al. (2024). To do so we isolate episodes in which the assessed probability of armed conflict in a previously calm country rose by at least twenty-five percentage points, and split them by what followed, i.e. false alarms vs. materialized risk followed by mass fatalities. We leave the description of our methodology to that sub-section (Section 3.2).

3 Results

3.1 The effect of violence bursts on FDI

Figure 4 plots the LP-DiD event-study paths for each of the six outcomes. Each panel shows the estimated response for every combination of the two parameters defining a violence burst, with the baseline specification—at least 1,000 fatalities over the three-month window, and at least ten times the country's total over the preceding twelve months—drawn in navy with its 95% confidence band. The full estimates are reported in Table 5.

The results show that FDI drops in the months following a violence burst of at least 1,000 fatalities over the three-month window. The response begins in the month of the burst and the decline is persistent over twelve months. Importantly, there is no anticipatory decline. The pre-treatment coefficients are around zero, confirming that FDI into countries about to experience a burst of violence was not already diverging from that into comparable peaceful countries.

In the baseline specification, the average post-treatment coefficient over horizons 0 to 12 suggest that the number of greenfield projects falls by 0.503 in inverse hyperbolic sine units ($p < 0.05$). For FDI projects in the range typical of affected countries (with on average 4.3 FDI projects per month), a coefficient of -0.5 corresponds to a reduction

of roughly 40% (asinh(4.3) = 2.1613, sinh(2.1613 - 0.503) = 2.5504, so a drop from 4.3 to 2.55 is about −40.7% or 1.75 projects).[2] For the other FDI outcomes, we find the number of sub sectors receiving investment to drop by 0.471, the number of distinct origin countries by 0.451, announced jobs by 1.546 and announced capital expenditure by 1.363, all significant at least at the 90% level. M&A deals show declines at each time horizons, though the average treatment effects for our benchmark specification ——−0.415 is statistically insignificant.

The results suggest that investors are reacting to news of the violence itself rather than to a deterioration in the outlook that preceded it; we return to this distinction in Section 3.2.

The estimated effect of violence on FDI depends on how a violence burst is defined. The magnitude of the effect rises systematically with the absolute scale of the violence. This is shown in Figure 4 with the spread of the gray paths around the baseline, as well as Figure 5, which summarizes the average post-treatment coefficient for each definition of violence burst, over horizons 0 to 12. At a floor of 25 fatalities over three months—a threshold that includes between 49 and 66 events—the estimated effect on greenfield projects ranges from −0.053 to −0.118 and is never statistically distinguishable from zero. Raising the floor to 100 leaves the estimates small and imprecise. It is only at a floor of 1,000 fatalities that the effects become both large and precisely estimated. The ratio parameter matters far less. Moving from a two-fold to a twenty-fold surprise multiple at a fixed floor of 1,000 fatalities changes the estimated effect on projects from −0.193 to −0.478, but the estimates at ratios of ten, fifteen and twenty are statistically indistinguishable from one another. In other words, what identifies a large effect is that

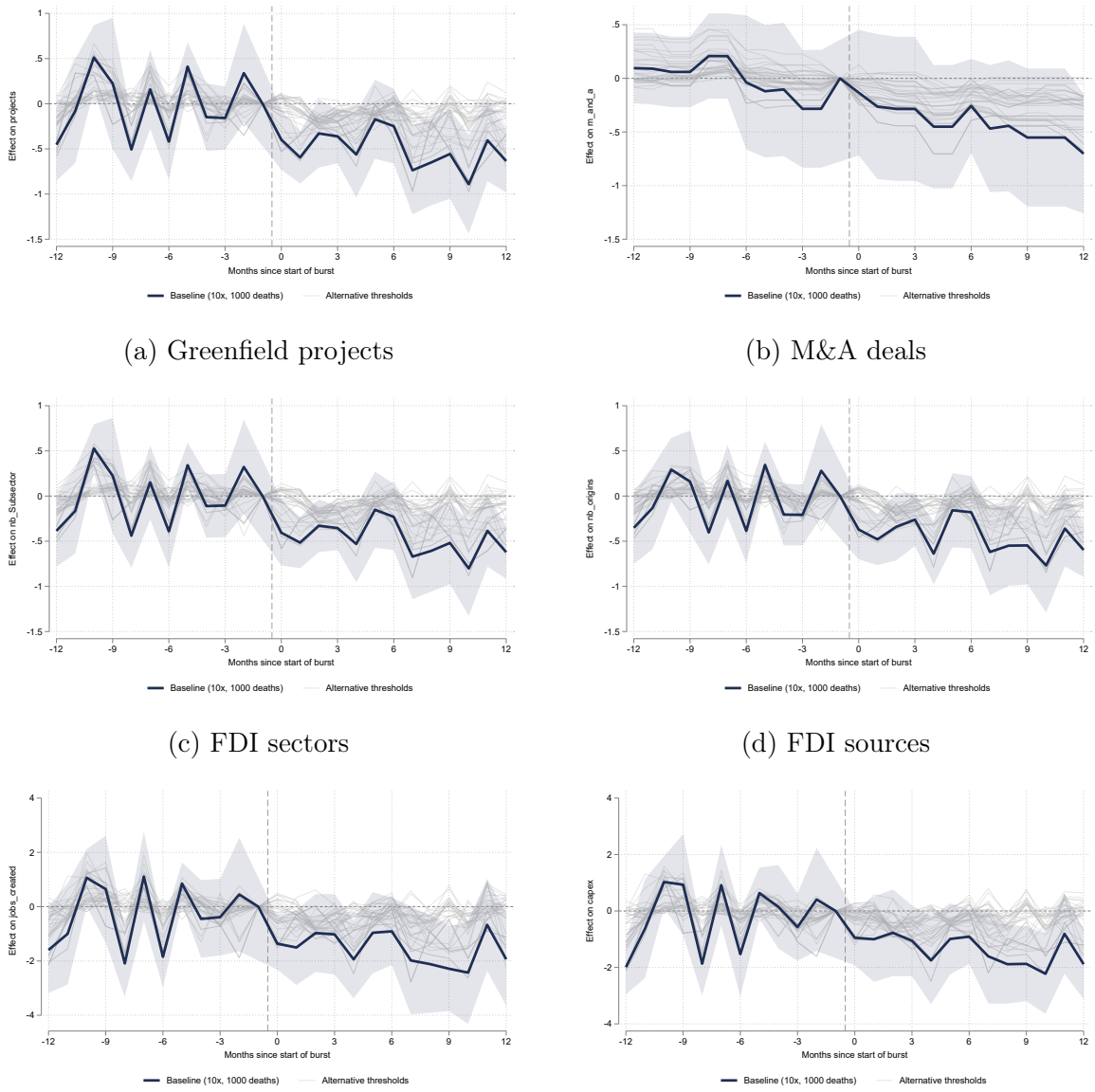
[2]See Table 4 for summary statistics for the benchmark specification's estimation sample in the twelve months preceding treatment effect and the list of treated countries. The inverse hyperbolic sine approximates the logarithm for values above roughly five, so $\exp(\beta) - 1$ gives the proportional change. At smaller values the implied proportional change is somewhat larger, because asinh is still behaving approximately linearly. For FDI values between five and ten, a 0.5-unit decrease corresponds almost exactly to multiplying by $e^{-0.5} \approx 0.61$, a reduction of 39–40%. For values around 2 to 3 the implied reduction is 42–43%, and for a value of 1 it is about 52%, reflecting the more nearly linear behavior of asinh close to zero.

a great many people were killed.

We test for the robustness of our estimated effect and provide additional results in the Appendix. In Appendix B, we show that the benchmark results are not driven by any single conflict episode. Excluding one treated country at a time leaves the sign of every effect unchanged, with a maximum deviation in the mean post-burst response of roughly 13% for all outcomes except M&As. In Appendix C, we show that the negative effect on greenfield investment is robust to estimating a Pseudo Poisson maximum likelihood version of the LP-DiD estimator. In Appendix D, we show that the effect of unexpected violence bursts on FDI are robust to an alternative measure of surprise. We use the conflict forecasts of Mueller et al. (2024), benchmarking realized violence against what was being predicted in the previous year. In Appendix F we look at heterogeneity across FDI sectors and find indications that the effect might be larger in sectors that fall within heavy manufacturing and services and logistics. In Appendix G we focus on peaceful countries and look at the effect of unexpected violence bursts that occur in bordering countries. These reveal no effect.

3.1.1 The case of Syria: A synthetic counterfactual case study

Finally, to give the estimates a concrete magnitude in the context of one the major conflict of the 21st century, we apply the synthetic control method of Abadie and Gardeazabal (2003), originally developed to estimate the effect of violence on the Basque Country economy, to Syria, to estimate the impact of the outbreak of civil war in 2011. To estimate the counterfactual trajectory of FDI absent the conflict, we compare the actual announced FDI-related jobs (solid line in Figure 6a) to a synthetic counterfactual (dashed line) constructed as a weighted average of countries with similar pre-treatment FDI trend. This method was also use by Chupilkin and Kóczán (2022), who used data across nearly four hundred wars since 1800 and find GDP per capita losses exceeding seven percentage points in the year after wars fought on national territory. Civil wars scar more persistently than interstate ones.



(a) Greenfield projects

(b) M&A deals

(c) FDI sectors

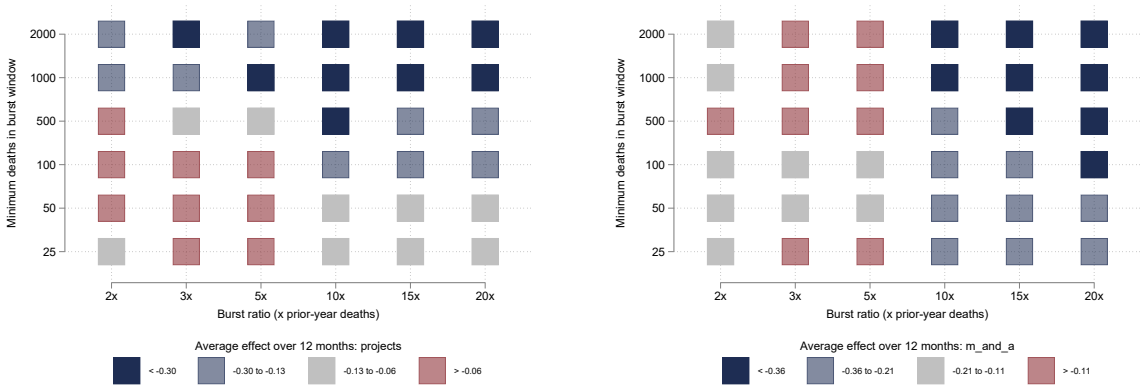
(d) FDI sources

(e) Jobs

(f) Capex

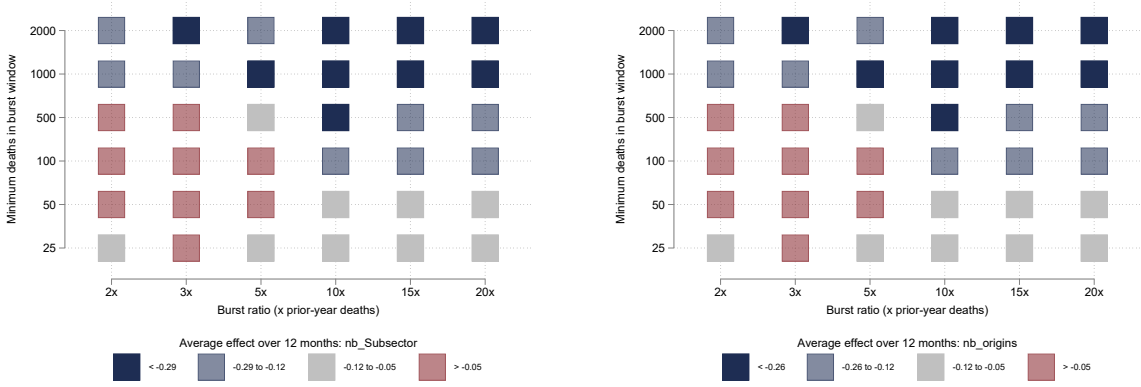
Figure 4: FDI response to violence bursts across event definitions: LP-DiD estimates

Notes: Each panel plots local-projections difference-in-differences event-study paths for one outcome, estimated once for every combination of the two parameters defining a violence burst: the minimum number of fatalities over the three-month burst window and the multiple by which those fatalities exceed the country's total over the preceding twelve months. Grey lines are the alternative specifications; the navy line is the baseline specification, with its 95% confidence band shaded. Outcomes enter in inverse hyperbolic sine form. The event is dated to the first month of the burst window and only the first burst per country is used; treatment is absorbing and control observations are drawn from not-yet-treated and never-treated country-months, excluding those within twelve months of a month meeting the fatality floor. Coefficients are differenced against the pre-treatment mean. The vertical dashed line marks the start of the burst; the horizontal line marks zero. Source: authors' calculations from fDi Markets, Refinitiv and conflict fatality data.



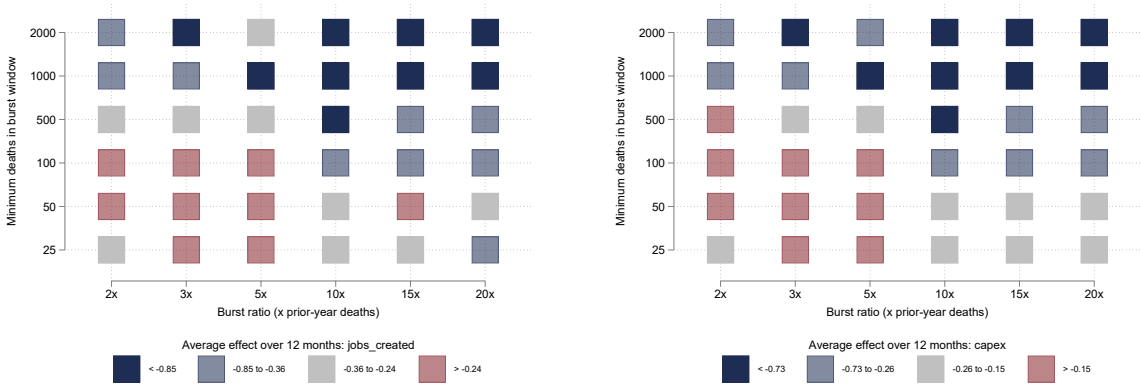
(a) Greenfield projects

(b) M&A deals



(c) FDI sectors

(d) FDI sources



(e) Jobs

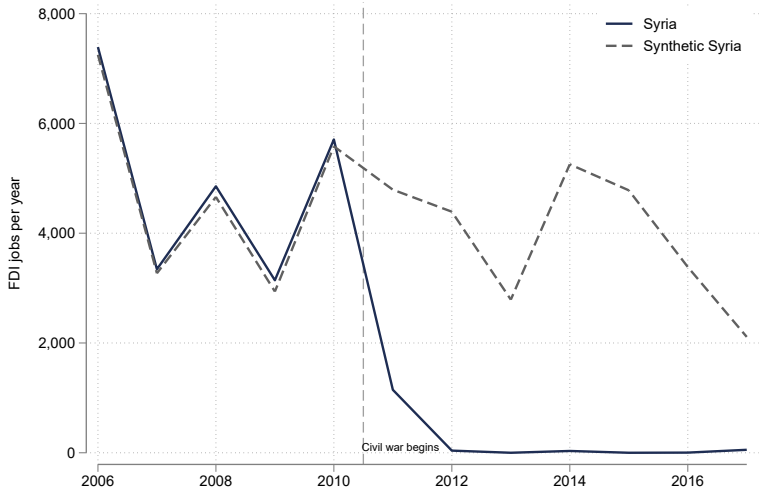
(f) Capex

Figure 5: Average effect of violence bursts across the parameter grid: LP-DiD estimates

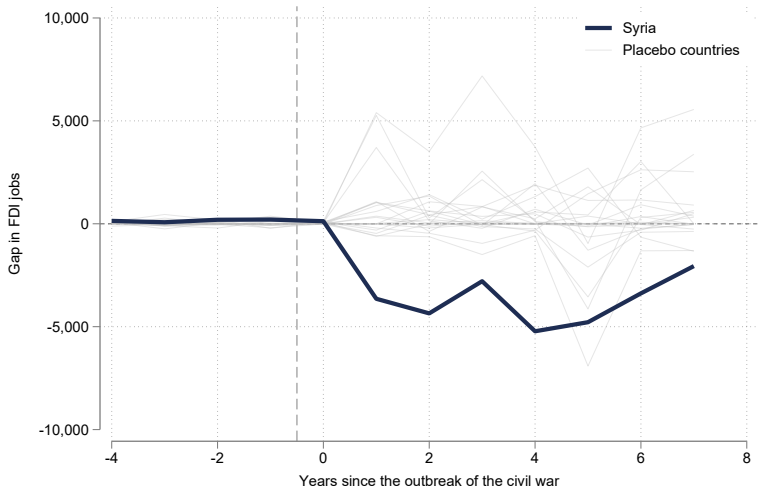
Notes: Each panel summarizes the local-projections difference-in-differences estimates for one outcome across the parameter grid defining a violence burst. Each cell corresponds to one combination of the minimum number of fatalities required over the three-month burst window (vertical axis) and the multiple by which those fatalities must exceed the country's total over the preceding twelve months (horizontal axis). Cells are shaded by quartile of the average post-treatment coefficient, taken over horizons 0 to 12 months; the legend reports the quartile cut-offs for each outcome, which therefore differ across panels. Outcomes enter in inverse hyperbolic sine form. Combinations yielding fewer than three events are not estimated and appear as blank cells. Source: authors' calculations from fDi Markets, Refinitiv and conflict fatality data.

Figure 6 reports the results. Panel (a) compares annual FDI-related job announcements in Syria with those in a synthetic Syria constructed from a donor pool of 73 countries, each recording no more than 100 conflict fatalities over the sample period and with average GDP per capita below $14,000, the World Bank's the high-income threshold. The weights are concentrated on the Maldives (0.363), North Macedonia (0.301) and Jamaica (0.063) together account for roughly 73% of the total, with Venezuela (0.020), Jordan (0.008), Tanzania (0.006) and Botswana (0.006) making up most of the remainder and the other 66 donors receiving 0.005 or less each. The match over the five pre-treatment years is close, with a root mean squared prediction error of 48 jobs against a pre-war average of roughly 4,900 jobs per year. The two series diverge sharply from 2011. Syria's announced FDI jobs fall to essentially zero and remain there, while the synthetic control continues on its pre-war path. The estimated effect ranges from roughly 2,100 to 5,200 lost jobs per year across the seven post-treatment years, with the largest gap in 2014, and averages close to 3,700 annually. Cumulated over 2011–2017 this amounts to some 26,000 FDI-related jobs that would have been announced absent the war.

Panel (b) plots Syria's gap against placebo gaps obtained by applying the same estimator in turn to each donor country as though it had been treated in 2011. Inference follows Abadie and Gardeazabal (2003) in ranking Syria's estimated effect within this distribution. Two sets of p-values are informative. The raw p-values, which compare gaps without adjusting for how well each unit was fitted before treatment, range from 0.151 to 0.329 with a joint value of 0.274. The standardized p-values, which divide each unit's post-treatment gap by its own pre-treatment RMSPE so that poorly fitted units are not credited with large gaps generated by noise, are 0.027, 0.027, 0.055, 0.014, 0.027, 0.027 and 0.055 across the seven post-treatment years, with a joint value of 0.041. With 73 placebo units the smallest attainable p-value is $1/74 \approx 0.014$, which Syria attains in the fourth post-treatment year.



(a) Synthetic control



(b) Placebo gaps

Figure 6: The Syrian civil war and FDI-related job creation

Notes: The outcome is the annual number of jobs announced in greenfield FDI projects. Treatment is dated to 2011, the year the civil war broke out. Panel (a) compares Syria with a synthetic control constructed as a weighted average of donor countries, with weights chosen to match Syria's pre-treatment path; the donor pool comprises countries recording no more than 100 conflict fatalities over the sample period and below the high-income GDP per capita threshold of 14,000 USD. The synthetic control for Syria places most weight on the Maldives (0.363), North Macedonia (0.301) and Jamaica (0.063), together about 73% of the total, followed by Venezuela (0.020), Jordan (0.008), Botswana (0.006) and Tanzania (0.006). The remaining 65 donors each receive a weight of 0.005 or less: Angola, Albania, Argentina, Armenia, Benin, Bangladesh, Bulgaria, Bosnia and Herzegovina, Belarus, Bolivia, Bhutan, China, Republic of Congo, Comoros, Costa Rica, Cuba, Djibouti, Dominica, Dominican Republic, Eritrea, Fiji, Gabon, Georgia, Ghana, Guinea, The Gambia, Guinea-Bissau, Equatorial Guinea, Guatemala, Guyana, Honduras, Kazakhstan, Kyrgyz Republic, Cambodia, Lao PDR, Morocco, Moldova, Madagascar, Montenegro, Mongolia, Mauritania, Mauritius, Malawi, Malaysia, Namibia, Nicaragua, Peru, Papua New Guinea, Paraguay, West Bank and Gaza, Romania, Rwanda, Senegal, Sierra Leone, El Salvador, Serbia, Togo, Tajikistan, Turkmenistan, Timor-Leste, Tunisia, Uzbekistan, Vietnam, South Africa, Zambia and Zimbabwe. Panel (b) plots that gap (navy) between Syria and its synthetic control, so that the distance from zero after 2011 is the estimated effect in jobs per year, against placebo gaps obtained by applying the same estimator in turn to each of the 73 donor countries as if it had been treated in 2011 (gray). Estimated effects range from roughly −2,100 to −5,200 FDI jobs per year. Source: fDiMarkets and (Sundberg and Melander, 2013).

3.2 False alarms vs. materialized conflict forecasts

The results so far establish that FDI falls after large bursts of violence. Two mechanisms are consistent with the effects. FDI may fall because violence destroys capital, disrupts operations and raises the cost of production and distribution. Or it may fall because a deterioration in a country's political outlook leads firms to postpone commitments. In other words FDI may respond to realized damage and/or to perceived risk. We test for these two mechanisms using the conflict forecasts of Mueller et al. (2024) which allow the two to be separated, because they generate episodes in which the assessed probability of armed conflict rose sharply and violence did not in fact follow.

Let π_{it} denote the forecast probability of armed conflict onset in country i over the following twelve months. We define a *risk burst* at month t as

$$\pi_{it} - \min_{j\in\{1,2,3\}} \pi_{i,t-j} \geq 0.25 \qquad \text{with} \qquad \min_{j\in\{1,2,3\}} \pi_{i,t-j} < 0.25, \tag{3}$$

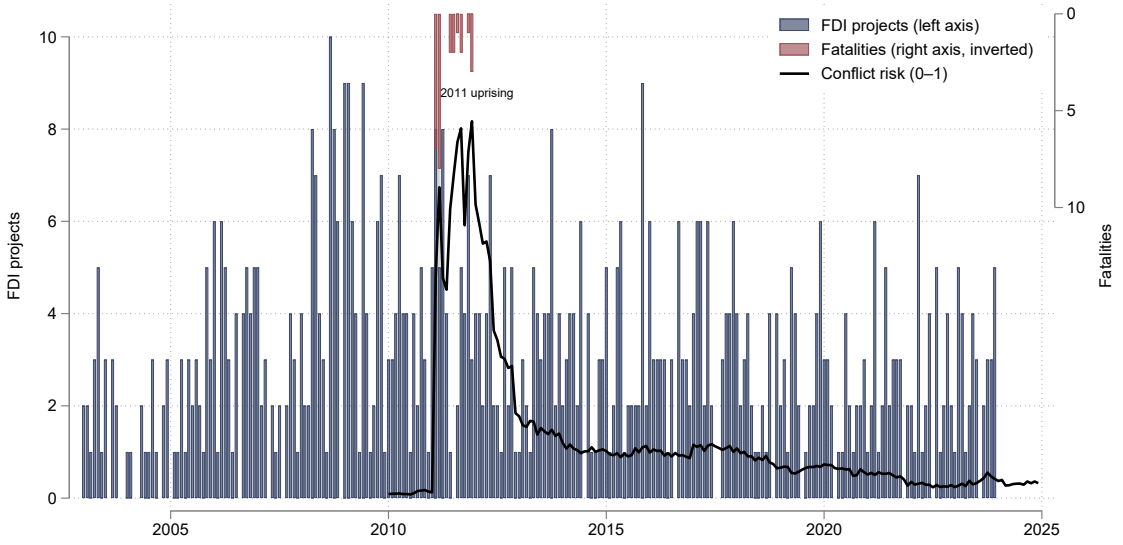
so that the assessed probability of conflict rises by at least 25 percentage points from a low base. We require that no other risk burst occurred in the preceding six months, and that no month of the preceding year recorded 25 or more fatalities.

A burst is a *false alarm* if no month over the subsequent twelve reaches 25 fatalities. It is *materialized* if some three-month window beginning in the following year records at least 1,000 fatalities, the same fatality criterion that defines a violence burst in equation (2). If FDI falls after false alarms as much as after materialized bursts, investors are responding to perceived risk; if it falls only after materialized bursts, the destruction channel dominates and the risk increase alone carries no consequence for investment.

Bahrain in 2011 illustrates what a false alarm looks like. Figure 7 shows the conflict risk forecast rising sharply from a low base as the Arab Spring protests spread to

Manama in February, reflecting a widely shared assessment at the time that the country was on the verge of serious armed conflict. The uprising was suppressed by force, Gulf Cooperation Council troops entered the country, and martial law was declared. But the fatality count never approached the threshold that defines armed conflict in our data. Bahrain is thus a case in which the signal available to investors deteriorated sharply while the violence that would have destroyed capital or disrupted operations did not arrive.

Figure 7: FDI projects and conflict fatalities in Bahrain



Notes: Bars show monthly greenfield FDI projects (left axis) and conflict fatalities (right axis, inverted). The black line shows the 12-month conflict risk forecast (0–1, no axis shown). The 2011 uprising was inspired by the Arab Spring, with the Shia-majority opposition protesting for political freedom and an end to the monarchy. Forces led by Saudi Arabia crushed the uprising, and martial law followed, but no mass fatalities. (Source: fDiMarkets and Sundberg and Melander (2013)).

We estimate equation (1) separately for each event type. In each case the countries experiencing the other event type are dropped, and controls are required to have neither a risk burst nor a month above 25 fatalities within the $[-12, +12]$ window. The two groups are alike in what was assessed at the time of the burst and differ only in what subsequently happened. Tables 9 and 10 describe the two estimation samples in the twelve months preceding a burst and the list of treated countries. Table 11 compares the two treated groups over the twelve months preceding each country's burst and

confirms that they are similar along FDI outcomes and conflict risk measures.

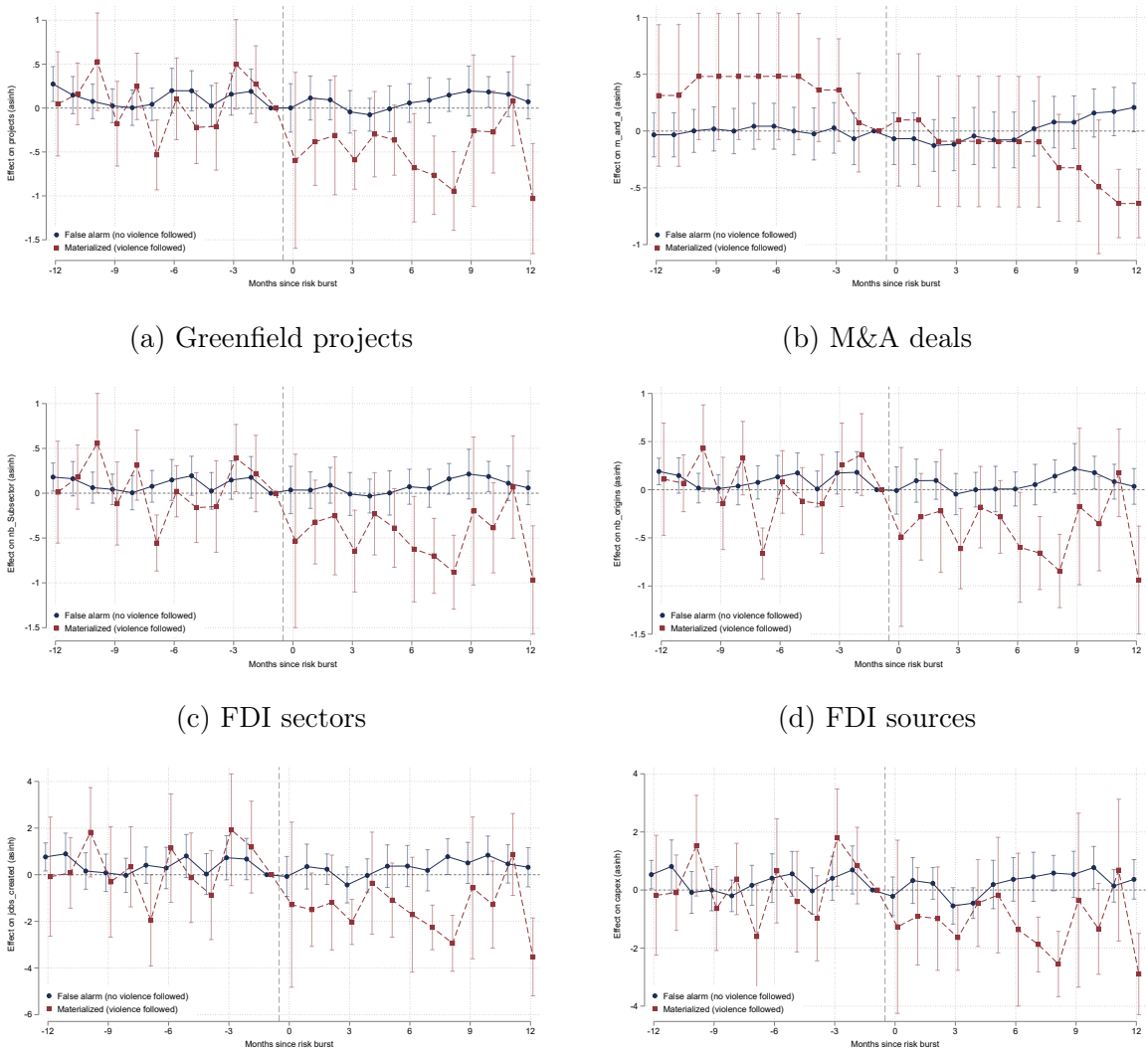
Figure 8 plots the event-study paths for both event types side by side, and Tables 2 and 3 report the coefficients averaged over four horizon windows. Figure 15 shows the PPML estimates.

After a false alarm, nothing moves. The average effect over the 12 months following the risk increase is 0.075 for greenfield projects, 0.010 for M&A deals, 0.075 for subsectors, 0.066 for origin countries, 0.297 for jobs and 0.212 for capital investment. Not one is statistically distinguishable from zero at any horizon, and the point estimates are as often positive as negative. A sharp and publicly assessed deterioration in a country's conflict outlook, in a country that had been calm, leaves foreign investment unaffected. Figure 7 shows greenfield project announcements continuing through the Bahrain risk surge episode, in line with the estimates in Table 2 for the 28 false-alarm countries as a group.

After a materialized burst on the other hand, every outcome falls. Over the full post-burst year, greenfield projects fall by 0.494 in inverse hyperbolic sine units, the number of subsectors by 0.466, the number of origin countries by 0.420, announced jobs by 1.451 and announced capital expenditure by 1.163. For outcomes at the low FDI levels typical of these countries (a mean of 2.6 FDI projects), the coefficient on projects corresponds to a reduction of 43% (asinh(2.6)=1.6836, sinh(1.68360.494)=1.4907, so a drop from 2.6 to 1.49, or 42.7%). The coefficients are largest at horizons seven to twelve for every outcome. For M&A deals, the pooled estimate of -0.213 is not distinguishable from zero, and only the seven-to-twelve-month coefficient of -0.418 is significant, which is due to the imprecision of the M&A estimates rather than with acquisitions being unaffected. The event-study paths in Figure 8 show that in both specification, investment in treated countries was not already diverging from the control group before the risk burst. The absence of differential pre-trends supports the identifying assumption and strengthens a causal interpretation with the caveat that estimates may still incorporate economic and political shocks that arrive with major violence.

Comparing the two designs, the materialized estimate on greenfield projects (-0.494) is very close to the baseline fatality-burst estimate of (-0.503) from our benchmark exercise, and those on jobs and capital (-1.451 and -1.163) are slightly smaller than the corresponding baselines (-1.546 and -1.363). The differences reflect the composition of the treated countries rather than a difference in what counts as violence.

The results thus point to the intensity of violence and fatalities as driving the negative impact of conflict on FDI, rather than increases in conflict risk and the accompanying political instability.



(a) Greenfield projects

(b) M&A deals

(c) FDI sectors

(d) FDI sources

(e) Jobs

(f) Capex

Figure 8: FDI response to conflict risk bursts: false alarms versus materialized risk (LP-DiD)

Notes: Each panel plots local-projections difference-in-differences event-study coefficients with 95% confidence intervals for one outcome, estimated separately for two types of event. A risk burst is an increase of at least 0.25 in the 12-month armed-conflict risk forecast relative to its minimum over the preceding three months, from a base below 0.25, with no other burst in the preceding six months and no month reaching 25 fatalities in the preceding twelve. Bursts are then split by what followed: *false alarms* (navy circles) are those with no month reaching 25 fatalities over the subsequent twelve months, while *materialized* bursts (maroon squares) are those in which some three-month window beginning in the following year records at least 1,000 fatalities; bursts falling between these thresholds enter neither group. Outcomes enter in inverse hyperbolic sine form and treatment is absorbing from a country's first burst of the relevant type. In each specification, control observations are drawn from not-yet-treated and never-treated country-months, excluding any observation with a conflict month or a risk burst within twelve months before or after. Source: fDiMarkets, Refinitiv Eikon, Sundberg and Melander (2013) and Mueller et al. (2024).

Table 2: False alarms: conflict risk rose but violence did not follow

	Pre-burst	Months since burst			
	$[-12,-1]$	0–3	4–6	7–12	0–12
Greenfield projects	0.111	0.042	-0.009	0.140	0.075
	(0.110)	(0.126)	(0.113)	(0.114)	(0.117)
M&As	-0.003	-0.095	-0.067	0.119	0.010
	(0.104)	(0.116)	(0.125)	(0.113)	(0.117)
Subsectors	0.102	0.037	0.014	0.131	0.075
	(0.099)	(0.115)	(0.107)	(0.102)	(0.107)
Origin countries	0.096	0.034	0.005	0.118	0.066
	(0.094)	(0.111)	(0.100)	(0.099)	(0.103)
Jobs created	0.400	0.018	0.235	0.515	0.297
	(0.419)	(0.411)	(0.424)	(0.423)	(0.420)
Capital investment	0.270	-0.052	0.034	0.476	0.212
	(0.371)	(0.333)	(0.357)	(0.356)	(0.349)

Notes: LP-DiD estimates on inverse hyperbolic sine transformed outcomes. Each cell reports the mean coefficient over the horizons in that column. A risk burst is an increase of at least 0.25 in the twelve-month armed-conflict probability, isolated from other bursts and preceded by twelve months with no month above 25 fatalities; a burst enters this table if no month in the following year records 25 or more fatalities. Countries experiencing the other event type are excluded, and remaining controls have neither a burst nor a month above 25 fatalities in the $[-12,+12]$ window. The pre-burst column is a placebo. Approximate standard errors in parentheses (mean of the horizon-specific standard errors; exact only under perfect correlation across horizons). $*$ $p<0.10$, $**$ $p<0.05$, $***$ $p<0.01$.

Table 3: Materialized risk: conflict risk rose and violence followed

	Pre-burst	Months since burst			
	$[-12,-1]$	0–3	4–6	7–12	0–12
Greenfield projects	0.060	-0.470	-0.448*	-0.532*	-0.494*
	(0.234)	(0.318)	(0.254)	(0.284)	(0.287)
M&As	0.359	0.004	-0.093	-0.418*	-0.213
	(0.273)	(0.293)	(0.291)	(0.229)	(0.263)
Subsectors	0.060	-0.438	-0.414*	-0.511*	-0.466
	(0.213)	(0.323)	(0.251)	(0.281)	(0.287)
Origin countries	0.047	-0.401	-0.354	-0.465*	-0.420
	(0.207)	(0.308)	(0.231)	(0.260)	(0.268)
Jobs created	0.261	-1.500	-1.055	-1.617*	-1.451
	(1.038)	(1.027)	(1.054)	(0.885)	(0.968)
Capital investment	0.110	-1.200	-0.663	-1.387	-1.163
	(0.807)	(0.959)	(1.033)	(0.884)	(0.942)

Notes: LP-DiD estimates on inverse hyperbolic sine transformed outcomes. Each cell reports the mean coefficient over the horizons in that column. A risk burst is an increase of at least 0.25 in the twelve-month armed-conflict probability, isolated from other bursts and preceded by twelve months with no month above 25 fatalities; a burst enters this table if some three-month window beginning in the following year records at least 1000 fatalities, the same fatality criterion used in the main design. Countries experiencing the other event type are excluded, and remaining controls have neither a burst nor a month above 25 fatalities in the $[-12,+12]$ window. The pre-burst column is a placebo. Approximate standard errors in parentheses (mean of the horizon-specific standard errors; exact only under perfect correlation across horizons). $*$ $p<0.10$, $**$ $p<0.05$, $***$ $p<0.01$.

4 Conclusion

Armed conflict has emerged as a defining challenge of the twenty-first century, with humanitarian, political and economic consequences. Understanding how violence shapes the allocation of global capital via FDI matters for economic development. This paper examines the relationship between conflict and foreign direct investment between 2003 and 2023, combining monthly conflict fatality data from the Uppsala Conflict Data Program with project-level greenfield FDI from fDiMarkets, cross-border M&A transactions from Refinitiv Eikon, and machine-learning forecasts of conflict onset derived from global news text from Mueller et al. (2024).

Our central results come from a difference-in-differences design that isolates episodes in which large-scale violence arrives suddenly in a country that had previously been calm. We find that violence deters FDI, but only when it is large. In the twelve months following a burst of at least 1,000 fatalities that exceeds the preceding year's total tenfold, greenfield projects fall by roughly 40% relative to comparable peaceful countries. At lower fatality thresholds the estimated effects are small and statistically indistinguishable from zero. Conflict narrows the composition of investment as well as reducing its volume. The number of distinct sectors receiving greenfield investment and the number of distinct origin countries fall by proportions similar to the project count.

Importantly, we find that this deterrent effect operates through violence rather than through the anticipation of it. We find that a sharp and publicly assessed deterioration in a country's conflict outlook, absent actual violence, does not by itself deter foreign investment.

These findings have important implications for economic development and policy as reduced FDI may be one of the channels through which conflict imposes costs long after the fighting has stopped. As armed conflicts proliferate globally—with 61 state-based conflicts in 2024, the most since 1946—understanding and mitigating these

investment effects become increasingly critical for sustaining economic development and prosperity.

Appendix

A Baseline results: Summary statistics and estimates table

Table 4 provides summary statistics for the estimation sample in the twelve months preceding a burst, before any treatment effect has operated in the benchmark specification. Eight countries meet the baseline definition—Azerbaijan, Brazil, Ethiopia, Israel (which includes Palestine violence as per the UCDP country codes), Lebanon, Libya, Syria and Ukraine—contributing 96 pre-burst country-months, against 171 control countries and just over 42,000 country-months. Treated countries are smaller recipients of FDI on average, receiving 4.3 greenfield projects and 41 M&A deals per month against 6.5 and 59 in the control group, with correspondingly fewer jobs and less capital committed. None of these differences in levels is statistically significant, however, and the control distributions are far more dispersed. The standard deviation of monthly projects among controls is nearly three times that among treated countries, reflecting the presence of major FDI destinations in the comparison group that have no counterpart among the treated. Control countries record no greenfield projects at all in 42% of months, against 22% among the treated ($p = 0.009$), so the treated group consists of countries not over characterized by zero flows.

Table 5: Specification grid: own-country conflict bursts

Ratio	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
2×	25	66	-0.063	-0.130	-0.062	-0.059	-0.252	-0.171
			(0.074)	(0.081)	(0.068)	(0.065)	(0.285)	(0.253)
3×	25	61	-0.053	-0.093	-0.047	-0.045	-0.203	-0.128
			(0.077)	(0.091)	(0.073)	(0.071)	(0.303)	(0.268)
5×	25	58	-0.059	-0.107	-0.054	-0.054	-0.195	-0.112
			(0.081)	(0.098)	(0.076)	(0.073)	(0.321)	(0.281)
10×	25	51	-0.110	-0.229**	-0.098	-0.093	-0.339	-0.228
			(0.082)	(0.093)	(0.077)	(0.074)	(0.329)	(0.293)
15×	25	51	-0.111	-0.222**	-0.099	-0.094	-0.337	-0.227
			(0.081)	(0.095)	(0.076)	(0.074)	(0.326)	(0.290)
20×	25	49	-0.118	-0.221**	-0.106	-0.102	-0.381	-0.254
			(0.083)	(0.097)	(0.078)	(0.075)	(0.328)	(0.294)
2×	50	54	-0.035	-0.181*	-0.033	-0.036	-0.086	-0.009
			(0.083)	(0.094)	(0.078)	(0.074)	(0.328)	(0.290)
3×	50	49	-0.019	-0.172	-0.019	-0.020	-0.070	-0.023
			(0.094)	(0.110)	(0.088)	(0.083)	(0.351)	(0.310)
5×	50	45	-0.047	-0.207*	-0.046	-0.047	-0.124	-0.071

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Table 5 continued

Ratio	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
			(0.097)	(0.119)	(0.091)	(0.086)	(0.375)	(0.329)
10×	50	38	-0.097	-0.308***	-0.090	-0.085	-0.260	-0.186
			(0.101)	(0.112)	(0.095)	(0.090)	(0.380)	(0.342)
15×	50	37	-0.085	-0.320***	-0.079	-0.074	-0.220	-0.148
			(0.099)	(0.117)	(0.092)	(0.089)	(0.376)	(0.339)
20×	50	35	-0.093	-0.321***	-0.086	-0.083	-0.275	-0.180
			(0.102)	(0.121)	(0.095)	(0.091)	(0.380)	(0.345)
2×	100	38	-0.048	-0.148	-0.041	-0.038	-0.119	-0.054
			(0.103)	(0.102)	(0.098)	(0.092)	(0.392)	(0.347)
3×	100	33	-0.001	-0.113	-0.004	0.001	-0.082	-0.052
			(0.118)	(0.131)	(0.111)	(0.105)	(0.413)	(0.369)
5×	100	31	-0.050	-0.150	-0.050	-0.048	-0.191	-0.147
			(0.122)	(0.143)	(0.115)	(0.109)	(0.441)	(0.387)
10×	100	25	-0.158	-0.335**	-0.152	-0.141	-0.546	-0.449
			(0.134)	(0.132)	(0.124)	(0.118)	(0.456)	(0.412)
15×	100	24	-0.146	-0.357***	-0.143	-0.130	-0.505	-0.399
			(0.133)	(0.138)	(0.124)	(0.118)	(0.458)	(0.417)
20×	100	23	-0.153	-0.360**	-0.149	-0.138	-0.557	-0.424
			(0.135)	(0.140)	(0.126)	(0.119)	(0.457)	(0.424)
2×	500	23	-0.061	-0.071	-0.042	-0.044	-0.277	-0.122
			(0.150)	(0.157)	(0.144)	(0.137)	(0.501)	(0.457)
3×	500	18	-0.063	-0.082	-0.044	-0.050	-0.292	-0.151
			(0.179)	(0.188)	(0.171)	(0.163)	(0.591)	(0.532)
5×	500	14	-0.084	-0.106	-0.074	-0.065	-0.315	-0.181
			(0.217)	(0.232)	(0.206)	(0.196)	(0.710)	(0.641)
10×	500	10	-0.310	-0.294	-0.294	-0.269	-0.857	-0.746
			(0.243)	(0.238)	(0.227)	(0.218)	(0.807)	(0.726)
15×	500	10	-0.269	-0.375	-0.246	-0.238	-0.793	-0.684
			(0.234)	(0.269)	(0.217)	(0.212)	(0.806)	(0.722)
20×	500	10	-0.269	-0.375	-0.246	-0.238	-0.793	-0.684
			(0.234)	(0.269)	(0.217)	(0.212)	(0.806)	(0.722)
2×	1000	17	-0.193	-0.122	-0.189	-0.169	-0.638	-0.484
			(0.183)	(0.207)	(0.172)	(0.166)	(0.603)	(0.545)
3×	1000	12	-0.283	-0.085	-0.276	-0.250	-0.810	-0.704
			(0.230)	(0.277)	(0.215)	(0.207)	(0.774)	(0.675)
5×	1000	10	-0.317	-0.090	-0.297	-0.271	-0.861	-0.750
			(0.257)	(0.304)	(0.246)	(0.237)	(0.853)	(0.747)
10×	1000	8	-0.503**	-0.415	-0.471**	-0.451**	-1.546**	-1.363**
			(0.213)	(0.310)	(0.196)	(0.197)	(0.787)	(0.688)
15×	1000	7	-0.478**	-0.370	-0.452**	-0.432**	-1.389*	-1.273*
			(0.227)	(0.347)	(0.207)	(0.205)	(0.819)	(0.699)
20×	1000	7	-0.478**	-0.370	-0.452**	-0.432**	-1.389*	-1.273*
			(0.227)	(0.347)	(0.207)	(0.205)	(0.819)	(0.699)

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Table 5 continued

Ratio	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
2×	2000	10	-0.266	-0.117	-0.252	-0.239	-0.836	-0.721
			(0.263)	(0.338)	(0.247)	(0.236)	(0.817)	(0.709)
3×	2000	8	-0.346	-0.095	-0.321	-0.296	-0.911	-0.825
			(0.298)	(0.385)	(0.283)	(0.272)	(0.965)	(0.832)
5×	2000	6	-0.160	-0.049	-0.146	-0.143	-0.272	-0.274
			(0.337)	(0.465)	(0.316)	(0.310)	(1.059)	(0.915)
10×	2000	5	-0.429	-0.416	-0.408*	-0.393	-1.068	-0.958
			(0.263)	(0.381)	(0.234)	(0.240)	(0.897)	(0.771)
15×	2000	5	-0.429	-0.416	-0.408*	-0.393	-1.068	-0.958
			(0.263)	(0.381)	(0.234)	(0.240)	(0.897)	(0.771)
20×	2000	5	-0.429	-0.416	-0.408*	-0.393	-1.068	-0.958
			(0.263)	(0.381)	(0.234)	(0.240)	(0.897)	(0.771)

Notes. Each cell reports the mean LP-DiD coefficient over horizons 0–12 for the burst-definition thresholds in that row; all outcomes are inverse hyperbolic sine transformed. Bursts are defined on own-country conflict deaths. Approximate standard errors in parentheses (mean of the horizon-specific standard errors; exact only under perfect correlation across horizons). Bold indicates the baseline specification. Dashes denote threshold pairs yielding too few events to estimate. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table 4: Treated and control groups: Fatality bursts (pre-burst months only)

	Control		Treated			
	Mean	SD	Mean	SD	Diff.	p
Greenfield projects	6.470	16.756	4.333	5.803	-2.136	0.336
M&As	59.474	156.053	41.198	68.187	-18.276	0.496
Subsectors	4.547	9.455	3.667	4.836	-0.880	0.606
Origin countries	2.975	4.919	2.792	3.043	-0.184	0.858
Jobs created	967.200	3,164	558.302	1,130	-408.898	0.184
Capital investment	385.800	1,408	210.857	326.137	-174.943	0.062
Zero-project months (share)	0.422	0.494	0.219	0.416	-0.203	0.009
country average	0.422	0.353	0.219	0.218	-0.203	0.009
Countries	171		8			
Country-months	42,047		96			

Notes: Baseline burst definition: at least 1000 fatalities over three months and at least 10× the preceding twelve months. Control countries with any qualifying violence in the $[-12, +12]$ window are excluded. Differences are treated minus control; standard errors clustered by country, except the country- average row, which has one observation per country and uses robust standard errors. Treated countries (ISO3): AZE, BRA, ETH, ISR, LBN, LBY, SYR, UKR.

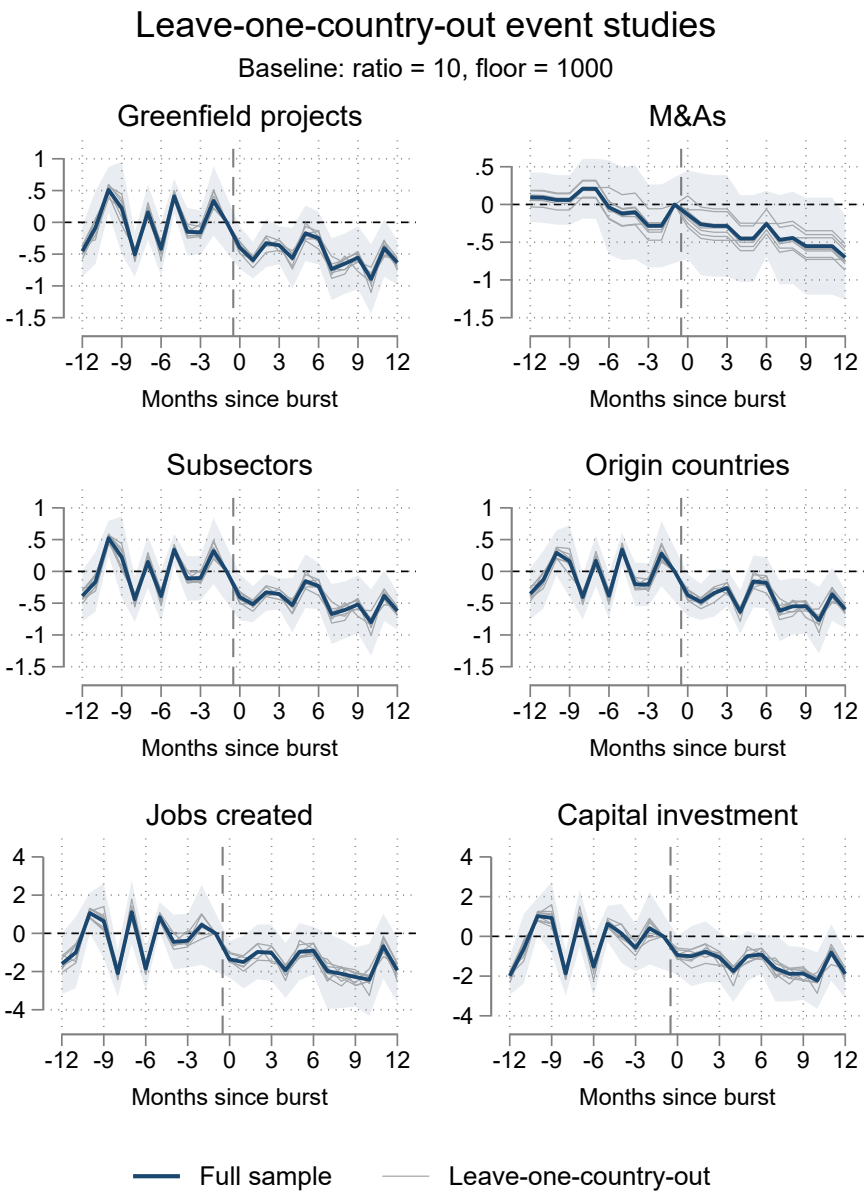
B Leave-one-out analysis of main effects

With only a handful of treated countries, a natural concern is that the estimated response is driven by one or two episodes rather than by a general pattern. We therefore re-estimate the baseline specification eight times, each time excluding one treated country, and compare the resulting paths to the full-sample estimate. Figure 9 plots the resulting event-study paths and Figure 10 summarizes each country's influence on the mean post-burst effect.

The results are stable. Across the six outcomes, no exclusion reverses the sign of the estimated effect, and for five of the six the largest single-country deviation in the mean post-burst response lies between 8.9 and 13.2 percent of the full-sample coefficient. For greenfield projects the full-sample estimate of -0.503 ranges from -0.463 to -0.568 across the eight subsamples; for the number of origin countries, -0.451 ranges from -0.411 to -0.503; for subsectors, -0.471 ranges from -0.428 to -0.531. Capital investment and jobs created behave similarly, with maximum deviations of 13.2 and 11.9 percent respectively.

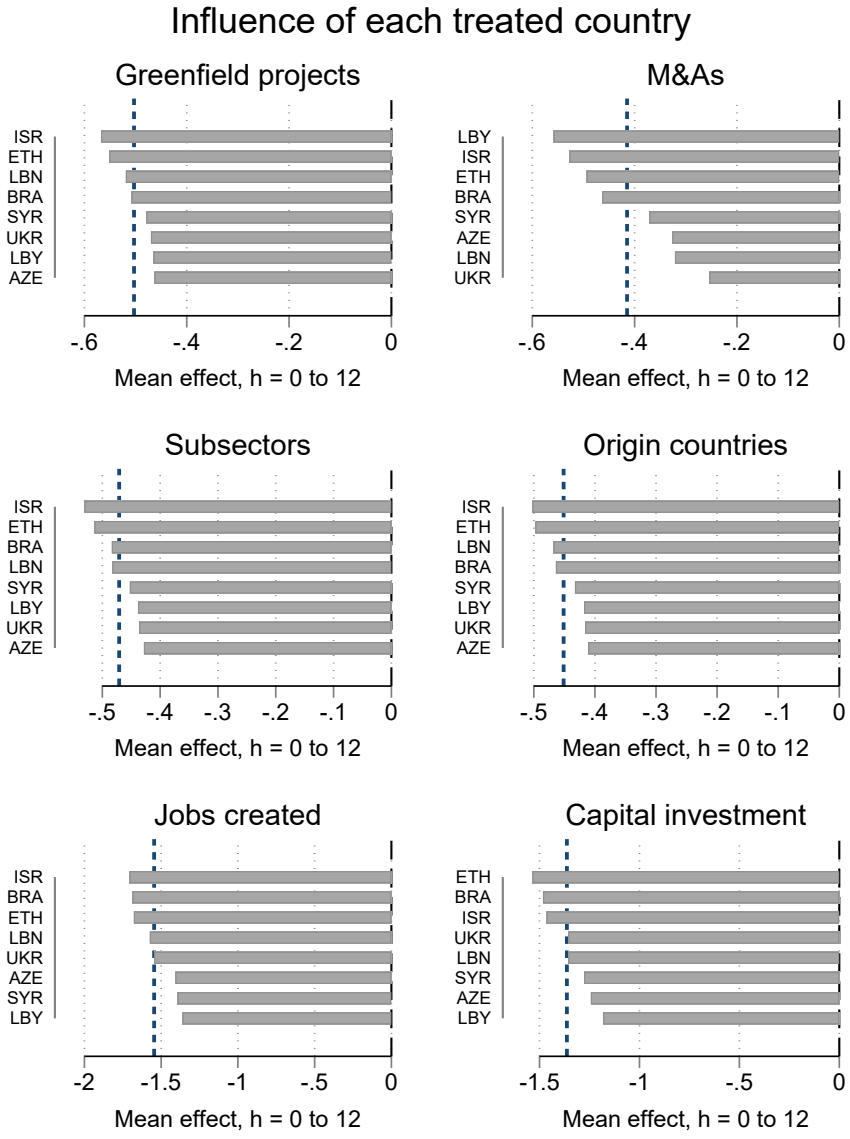
Excluding Israel or Ethiopia consistently strengthens the estimated contraction, while excluding Azerbaijan or Libya attenuates it. This is consistent with heterogeneity in the depth of the FDI response across episodes rather than with a single outlier. The full-sample estimate sits near the middle of the leave-one-out distribution in every case. Mergers and acquisitions are the exception. The full-sample coefficient of -0.415 moves to -0.254 when Ukraine is dropped and to -0.559 when Libya is dropped, a range of roughly ± 35 percent. The sign is unchanged in every subsample, but the magnitude is not well pinned down. This is unsurprising given that cross-border M&A is both rarer and lumpier than greenfield entry, so a small number of transactions in any one episode can shift the average.

Figure 9: Leave-one-country-out event studies



Notes: The figure reports LP-DiD estimates of the effect of a conflict burst on each outcome, measured in months relative to the burst. A burst is defined as at least 1,000 conflict deaths over a three-month window, representing at least a tenfold increase relative to the preceding twelve months; only the first burst per country is retained. Outcomes are transformed using the inverse hyperbolic sine. The navy line is the full-sample estimate with its 95% confidence interval; each grey line re-estimates the specification dropping one treated country at a time. The eight treated countries are Azerbaijan, Brazil, Ethiopia, Israel (which includes Palestine violence as per the UCDP country codes), Lebanon, Libya, Syria and Ukraine. Standard errors are clustered at the country level.

Figure 10: Influence of individual treated countries



Notes: Each bar reports the mean LP-DiD estimate over the twelve months following a conflict burst when the indicated country is excluded from the sample. A burst is defined as at least 1,000 conflict deaths over a three-month window, representing at least a tenfold increase relative to the preceding twelve months; only the first burst per country is retained. Outcomes are transformed using the inverse hyperbolic sine. The solid navy line marks the full-sample estimate; bars close to the navy line indicate that no single country drives the result. Standard errors are clustered at the country level.

C Robustness of main effect to PPML

Because the inverse hyperbolic sine transformation is sensitive to the units in which the outcome is measured, and because a substantial share of country-months record no projects at all, we re-estimate the entire grid using a Poisson pseudo-maximum likelihood version of the LP-DiD estimator on raw counts (Silva and Tenreyro, 2006). We build a stacked dataset in which each event cohort e contributes its treated country and all countries that are clean over the window $[e-12, e+12]$, and estimate

$$\mathbb{E}\left[y_{it}^{(e)} \mid \cdot\right] = \exp\left(\sum_{\substack{k=-12 \\ k\neq -1}}^{12} \beta_k \, \mathbf{1}\{t-e=k\} \cdot B_i^{(e)} + \mu_i^{(e)} + \lambda_t^{(e)}\right), \tag{4}$$

where $B_i^{(e)}$ indicates the treated country in cohort e, $\mu_i^{(e)}$ and $\lambda_t^{(e)}$ are stack-specific country and month fixed effects, and the month before the burst is the omitted reference period. Standard errors are clustered by country. Coefficients are in log points, so $\exp(\beta_k)-1$ gives the proportional effect on the expected outcome. This specification delivers coefficients that are interpretable as semi-elasticities. The drawback is that it's not clear whether it satisfies the assumptions of the LP-DiD model.

Figures 11 and 12 report the event-study paths and grid summaries, and Table 6 the average post-treatment coefficients. Coefficients are in log points, so $\exp(\beta)-1$ gives the proportional effect on the expected outcome. The Poisson estimates confirm the negative effect on greenfield investment but locate its precision differently across the grid. Where the LP-DiD estimates were insignificant at low fatality floors and became large and significant only at a floor of one thousand, the Poisson estimates are significant across most of the low and middle range and less precise at the top. At a floor of 50 fatalities the effect on projects ranges from -0.170 to -0.209 and is significant at conventional levels for every surprise ratio, implying reductions of 16 to 19 per cent in the expected number of projects. Subsectors move similarly, and announced jobs fall by around 40 per cent ($\beta \approx -0.50$, $p < 0.01$ at a three-fold ratio). At the baseline of 1,000 fatalities and a ten-fold ratio, projects fall by 0.375 log points—roughly 30 per cent—and subsectors by 0.342, both significant at the 90% level, with origin countries negative but imprecisely estimated. The baseline effects are slightly smaller under Poisson than under LP-DiD. The LP-DiD coefficient of -0.503 on projects implies a reduction of around 40 per cent; the Poisson coefficient of -0.375 implies 30. Where the effect is concentrated among smaller recipients, as it appears to be here, the Poisson estimate will be the more conservative of the two. All in all, the Poisson estimates support the central finding that bursts of violence reduce the number of greenfield projects and narrow their sectoral composition, while suggesting that the magnitudes in Section **??** may be at the upper end of a plausible range.

Table 6: Specification grid, PPML: own-country conflict bursts

Ratio	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
2×	25	66	-0.145*	-0.029	-0.111	-0.068	-0.242	-0.109
			(0.085)	(0.036)	(0.081)	(0.083)	(0.195)	(0.268)
3×	25	61	-0.164*	-0.036	-0.127	-0.088	-0.273	-0.173
			(0.087)	(0.039)	(0.084)	(0.085)	(0.206)	(0.269)
5×	25	58	-0.152*	-0.046	-0.119	-0.055	-0.213	-0.016

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Table 6 continued

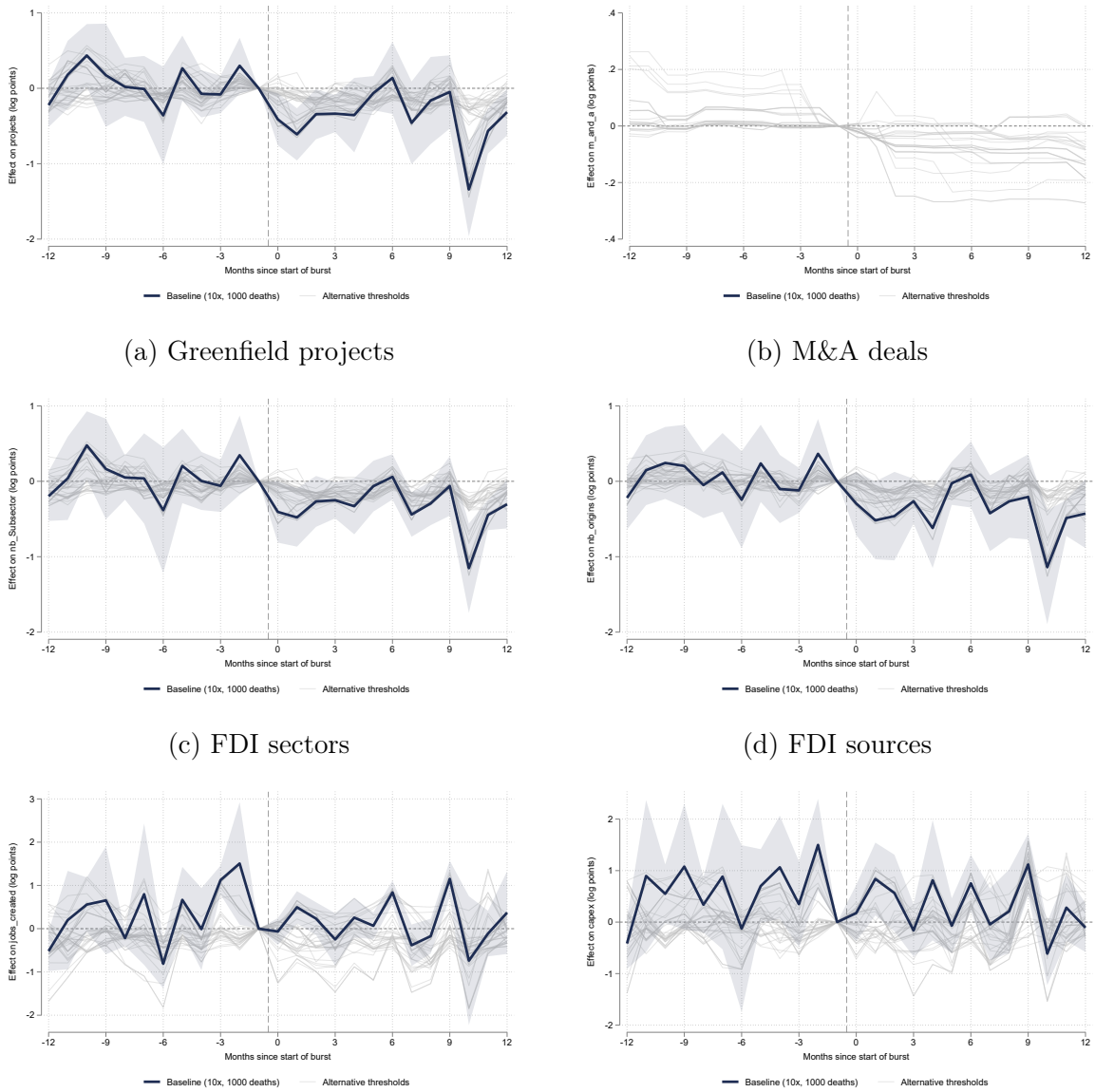
Ratio	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
			(0.088)	(0.037)	(0.084)	(0.087)	(0.200)	(0.270)
10×	25	51	-0.143	-0.072*	-0.141	-0.088	-0.214	-0.014
			(0.094)	(0.043)	(0.092)	(0.093)	(0.228)	(0.304)
15×	25	51	-0.138	-0.072*	-0.138	-0.073	-0.205	-0.021
			(0.094)	(0.043)	(0.092)	(0.090)	(0.227)	(0.303)
20×	25	49	-0.141	-0.073*	-0.138	-0.096	-0.208	-0.007
			(0.095)	(0.044)	(0.092)	(0.087)	(0.226)	(0.308)
2×	50	54	-0.174**	-0.033	-0.162*	-0.082	-0.455**	-0.316
			(0.088)	(0.038)	(0.083)	(0.087)	(0.187)	(0.279)
3×	50	49	-0.209**	-0.036	-0.188**	-0.121	-0.501***	-0.351
			(0.083)	(0.039)	(0.081)	(0.083)	(0.191)	(0.268)
5×	50	45	-0.199**	-0.042	-0.183**	-0.117	-0.510**	-0.321
			(0.086)	(0.039)	(0.083)	(0.086)	(0.203)	(0.298)
10×	50	38	-0.180**	-0.083**	-0.204**	-0.127	-0.420*	-0.260
			(0.089)	(0.042)	(0.087)	(0.096)	(0.230)	(0.337)
15×	50	37	-0.170*	-0.083*	-0.205**	-0.112	-0.446**	-0.286
			(0.089)	(0.042)	(0.087)	(0.095)	(0.222)	(0.341)
20×	50	35	-0.174*	-0.084**	-0.207**	-0.137	-0.452**	-0.275
			(0.089)	(0.043)	(0.087)	(0.090)	(0.222)	(0.346)
2×	100	38	-0.122	0.012	-0.100	-0.085	-0.113	0.013
			(0.095)	(0.046)	(0.081)	(0.105)	(0.291)	(0.304)
3×	100	33	-0.135	0.011	-0.116	-0.111	-0.162	-0.036
			(0.097)	(0.046)	(0.083)	(0.102)	(0.299)	(0.307)
5×	100	31	-0.135	0.005	-0.112	-0.131	-0.169	-0.005
			(0.100)	(0.044)	(0.085)	(0.110)	(0.317)	(0.352)
10×	100	25	-0.113	-0.105*	-0.142	-0.152	-0.317	-0.169
			(0.135)	(0.055)	(0.127)	(0.157)	(0.489)	(0.450)
15×	100	24	-0.055	-0.107*	-0.110	-0.076	-0.343	-0.196
			(0.130)	(0.057)	(0.125)	(0.153)	(0.493)	(0.448)
20×	100	23	-0.074	-0.110*	-0.125	-0.152	-0.371	-0.147
			(0.131)	(0.058)	(0.125)	(0.144)	(0.492)	(0.474)
2×	500	23	-0.007	-0.040	-0.005	-0.029	-0.338	0.222
			(0.171)	(0.134)	(0.173)	(0.187)	(0.453)	(0.520)
3×	500	18	0.025	-0.006	0.003	0.020	-0.311	0.055
			(0.177)	(0.144)	(0.181)	(0.209)	(0.561)	(0.578)
5×	500	14	0.023	-0.159**	0.010	-0.023	-0.499	-0.136
			(0.220)	(0.070)	(0.232)	(0.246)	(0.744)	(0.649)
10×	500	10	-0.213	-0.121	-0.266	-0.298	-0.898	-0.503
			(0.216)	(0.082)	(0.213)	(0.248)	(0.870)	(0.783)
15×	500	10	-0.283	-0.226**	-0.240	-0.283	-0.914	-0.575
			(0.231)	(0.092)	(0.219)	(0.255)	(0.854)	(0.785)
20×	500	10	-0.283	-0.226**	-0.240	-0.283	-0.914	-0.575
			(0.231)	(0.092)	(0.219)	(0.255)	(0.854)	(0.785)

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Table 6 continued

Ratio	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
2×	1000	17	-0.237*	–	-0.182	-0.215	0.323	0.597*
			(0.121)		(0.135)	(0.162)	(0.241)	(0.349)
3×	1000	12	-0.214	–	-0.256	-0.291	0.397	0.409
			(0.194)		(0.191)	(0.204)	(0.295)	(0.323)
5×	1000	10	-0.190	–	-0.204	-0.267	0.448	0.452
			(0.188)		(0.198)	(0.208)	(0.299)	(0.321)
10×	1000	8	-0.375*	–	-0.342*	-0.388	0.130	0.289
			(0.208)		(0.176)	(0.238)	(0.274)	(0.338)
15×	1000	7	-0.388*	–	-0.352**	-0.402	0.119	0.246
			(0.213)		(0.178)	(0.246)	(0.280)	(0.338)
20×	1000	7	-0.388*	–	-0.352**	-0.402	0.119	0.246
			(0.213)		(0.178)	(0.246)	(0.280)	(0.338)
2×	2000	10	-0.219**	–	-0.130	-0.128	0.318	0.504
			(0.108)		(0.116)	(0.147)	(0.213)	(0.337)
3×	2000	8	-0.171	–	-0.176	-0.140	0.428	0.401
			(0.153)		(0.178)	(0.177)	(0.284)	(0.284)
5×	2000	6	-0.247	–	-0.187	-0.177	0.337	0.302
			(0.171)		(0.178)	(0.166)	(0.317)	(0.310)
10×	2000	5	-0.323*	–	-0.311**	-0.241	0.117	0.273
			(0.178)		(0.143)	(0.191)	(0.261)	(0.338)
15×	2000	5	-0.323*	–	-0.311**	-0.241	0.117	0.273
			(0.178)		(0.143)	(0.191)	(0.261)	(0.338)
20×	2000	5	-0.323*	–	-0.311**	-0.241	0.117	0.273
			(0.178)		(0.143)	(0.191)	(0.261)	(0.338)

Notes. Poisson pseudo-maximum likelihood estimates. Each cell reports the mean coefficient over horizons 0–12 for the burst-definition thresholds in that row; outcomes enter in levels and coefficients are semi-elasticities. Bursts are defined on own-country conflict deaths. Approximate standard errors in parentheses (mean of the horizon-specific standard errors; exact only under perfect correlation across horizons). The dagger marks the baseline specification. Dashes denote threshold pairs yielding too few events to estimate. $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.



(a) Greenfield projects

(b) M&A deals

(c) FDI sectors

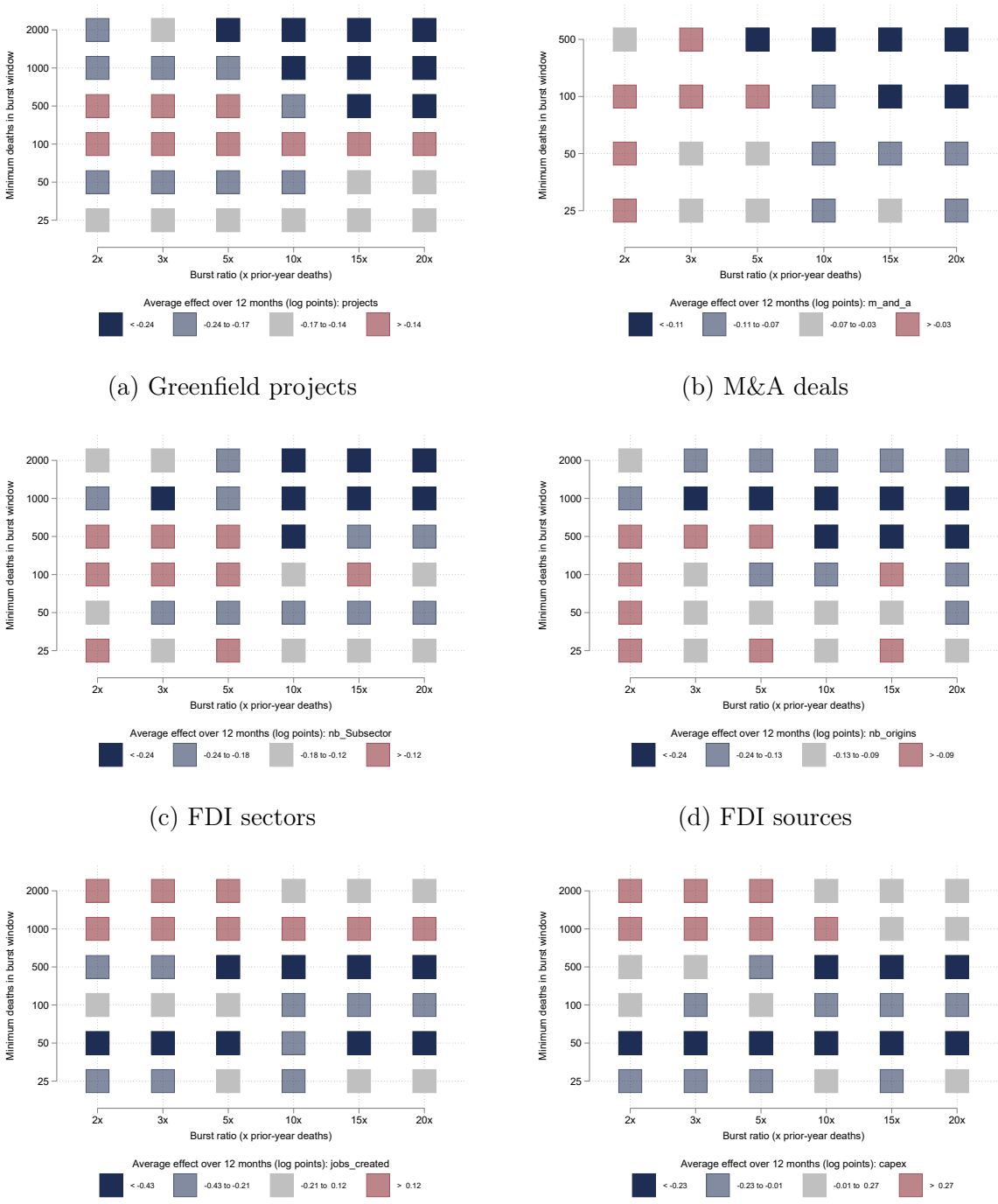
(d) FDI sources

(e) Jobs

(f) Capex

Figure 11: FDI response to violence bursts across event definitions: Poisson estimates

Notes: As Figure 4, but estimated by Poisson pseudo-maximum-likelihood on raw counts using stacked event cohorts, with stack-specific country and month fixed effects and standard errors clustered by country. Coefficients are in log points, so $\exp(\beta) - 1$ gives the proportional effect on the expected outcome, and the month before the burst is the omitted reference period. Specifications for which the estimator fails to converge, or which yield fewer than three events, are not plotted. Source: fDiMarkets, Refinitiv Eikon, and Sundberg and Melander (2013).



(a) Greenfield projects

(b) M&A deals

(c) FDI sectors

(d) FDI sources

(e) Jobs

(f) Capex

Figure 12: Average effect of violence bursts across the parameter grid: Poisson estimates

Notes: As Figure 5, but estimated by Poisson pseudo-maximum-likelihood on raw counts using stacked event cohorts, with stack-specific country and month fixed effects and standard errors clustered by country. Each cell corresponds to one combination of the minimum number of fatalities required over the three-month burst window (vertical axis) and the multiple by which those fatalities must exceed the country's total over the preceding twelve months (horizontal axis). Cells are shaded by quartile of the average post-treatment coefficient, taken over horizons 0 to 12 months; coefficients are in log points, so $\exp(\beta) - 1$ gives the proportional effect on the expected outcome, and the month before the burst is the omitted reference period. Quartile cut-offs are computed within each panel and therefore differ across outcomes. Combinations yielding fewer than three events, or for which the estimator fails to converge, are not reported and appear as blank cells. Source: fDiMarkets, Refinitiv Eikon, and Sundberg and Melander (2013).

D Robustness of main effect to alternative definition of unexpected death bursts

The burst definition used so far measures surprise against a country's own recent history. Violence counts as unexpected if it exceeds the preceding twelve months by some multiple. This benchmark identifies as surprising any escalation from a low base, whether or not the escalation was in fact anticipated. We therefore construct an alternative measure of surprise using the conflict forecasts of Mueller et al. (2024), benchmarking realized violence against what was being predicted at the time rather than against what had recently occurred. As set out in equation (5), a country experiences an unexpected death surge when fatalities over a three-month window reach a floor $\bar{F}$ and exceed by a multiple m the most pessimistic twelve-month-ahead forecast issued at any point in the preceding year, with the additional requirement that no month of that year exceeded 25 fatalities. Taking the maximum over the preceding twelve forecasts implies that the violence must exceed not merely the central prediction but the worst case that any forecast during the previous year had entertained.

Let $\hat{D}_{i,t-j}$ denote the twelve-month-ahead fatality forecast issued for country i in month $t-j$. Country i experiences an *unexpected death surge* at t if

$$(5) \qquad \sum_{s=0}^{2} D_{i,t+s} \;\geq\; \bar{F} \qquad \text{and} \qquad \sum_{s=0}^{2} D_{i,t+s} \;\geq\; m \cdot \max_{j \in \{1,\ldots,12\}} \hat{D}_{i,t-j},$$

with the additional requirement that no month of the preceding year exceeded 25 fatalities. Taking the maximum over the preceding twelve forecasts sets a deliberately demanding bar. The realized violence must exceed even the most pessimistic forecast issued during the previous year by a factor of m. We again estimate equation (1) across a grid of $(\bar{F}, m)$ combinations. Finding similar responses under (2) and (5) indicate that the results are not an artifact of how an unexpected violence surge surprise is defined.

Table 7 describes the estimation sample in the twelve months before a surge. Nine countries meet the baseline definition—Azerbaijan, France, Haiti, Israel, Lebanon, Libya, Sri Lanka, Syria and Ukraine, against 158 control countries. The composition differs instructively from the fatality-burst sample. Six countries appear in both, but the forecast-based definition drops Brazil and Ethiopia, whose violence was substantial in absolute terms but broadly in line with what forecasters expected, and adds France, Haiti and Sri Lanka, where the fatalities that occurred were small in absolute number but far exceeded any prediction. The inclusion of France is driven by the November 2015 Paris attacks. On observables the treated group is close to the controls. Greenfield projects, M&A deals, subsectors and origin countries are all statistically indistinguishable between the two groups before the surge, and the share of months with no projects differs insignificantly.

Figure 13 plots the event-study paths across the specification grid, Figure 14 summarizes them, and Table 8 reports the average post-treatment coefficients. The results reproduce the pattern of Section **??** closely. Effects are uniformly negative but small and imprecise at low fatality floors. At a floor of 25 fatalities, the effect on greenfield projects ranges from -0.044 to -0.115 and is never significant, regardless of how demanding the surprise multiple. Magnitudes grow as the floor rises, and significance appears only where the surge is both large in absolute terms and far outside what was forecast. At a floor of 1,000 and a ten-fold multiple, projects fall by 0.514 ($p < 0.05$), subsectors by 0.477, origin countries by 0.463, announced jobs by 1.561 and capital by 1.346.

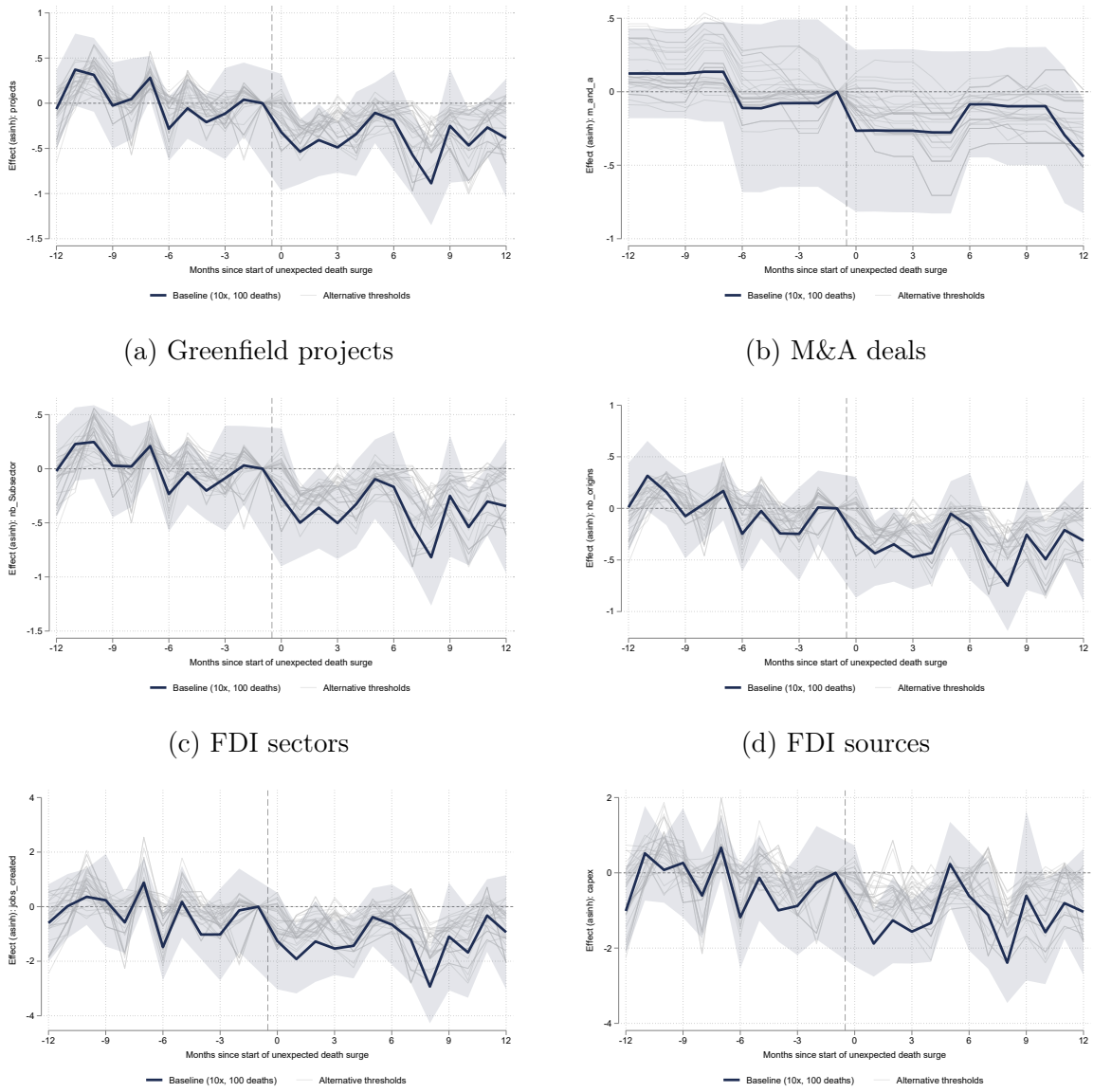
The forecast-based grid also delivers significant effects at combinations where the history-based grid did not. At a floor of 500 with a twenty-fold multiple, projects fall by 0.482 ($p < 0.05$) on seven

events, against an insignificant −0.269 at the corresponding history-based cell. This is what one would expect if the forecast benchmark is the sharper instrument that can separate unanticipated violence from escalation that observers had already priced in, and so identifies a cleaner set of events at any given fatality threshold. M&A behaves as it did under the main definition, negative throughout but never individually significant, and turning marginally positive at floors of five hundred and a thousand with low multiples. We read this as reflecting the small monthly counts rather than a substantive difference in how acquisitions respond.

Table 7: Treated and control groups: unexpected death surges (pre-surge months only)

	Control		Treated			
	Mean	SD	Mean	SD	Diff.	p
Greenfield projects	6.271	16.712	6.519	12.883	0.247	0.953
M&As	60.301	161.683	68.190	125.107	7.889	0.860
Subsectors	4.424	9.350	4.806	8.616	0.382	0.891
Origin countries	2.923	4.887	3.269	4.751	0.345	0.824
Jobs created	833.647	2,517	401.750	647.419	-431.897	0.051
Capital investment	365.456	1,389	165.238	272.321	-200.218	0.024
Zero-project months (share)	0.421	0.494	0.287	0.454	-0.134	0.239
averaged across countries	0.422	0.354	0.287	0.346	-0.135	0.235
Countries	158		9			
Country-months	35,334		108			

Notes: A country is treated at the first month in which fatalities over the following three months reach at least 100 and at least 10× the most pessimistic 12-month-ahead forecast issued in the preceding year, with no month in that year exceeding 25 fatalities. Control countries with any month above that threshold in the $[-12, +12]$ window are excluded. Differences are treated minus control; standard errors clustered by country, except the country-average row, which has one observation per country and uses robust standard errors. Treated countries (ISO3): AZE, FRA, HTI, ISR, LBN, LBY, LKA, SYR, UKR.



(a) Greenfield projects

(b) M&A deals

(c) FDI sectors

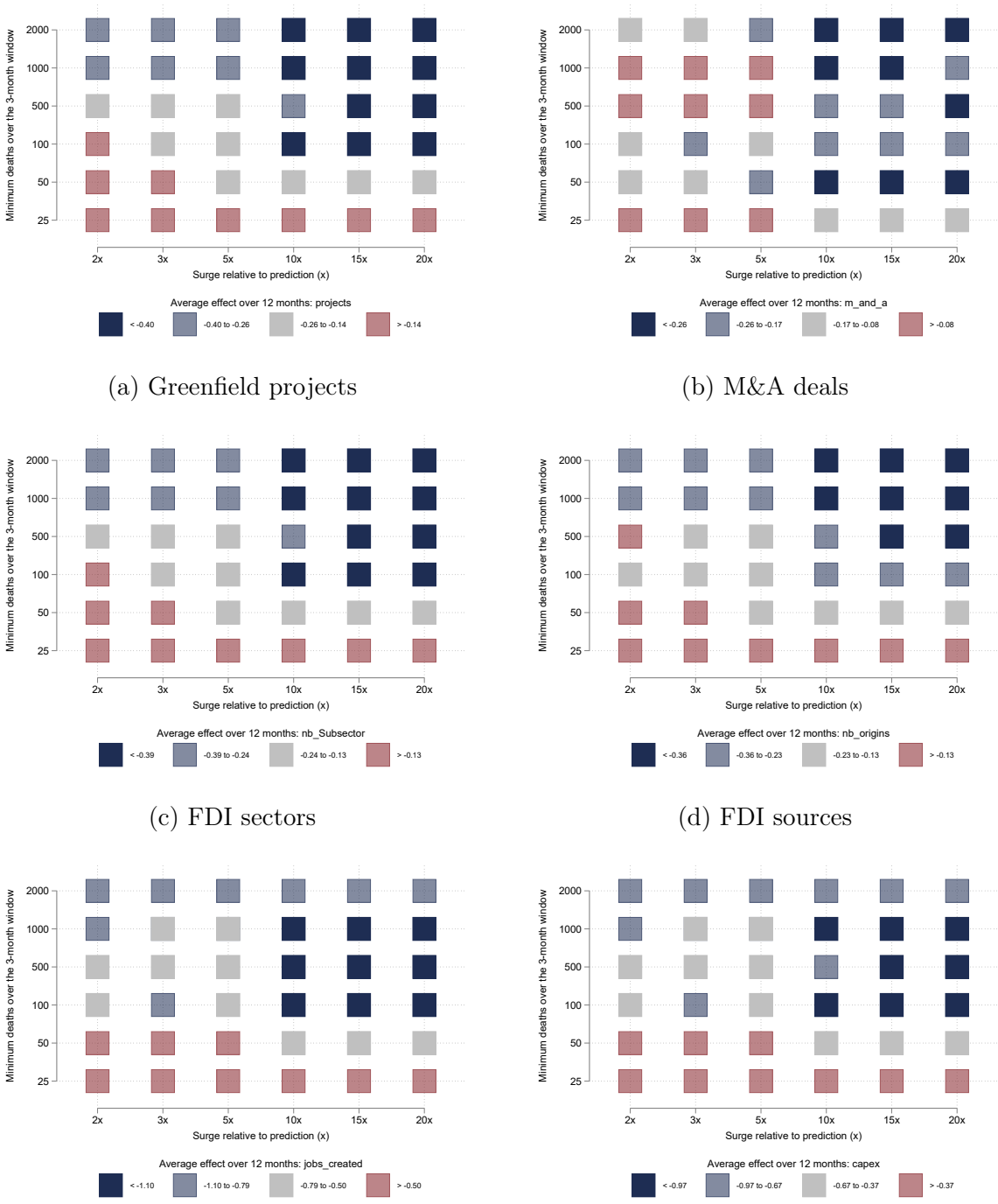
(d) FDI sources

(e) Jobs

(f) Capex

Figure 13: FDI response to unexpected death surges across event definitions: LP-DiD estimates

Notes: Each panel plots local-projections difference-in-differences event-study paths for one outcome, estimated once for every combination of the two parameters defining an unexpected death surge: the minimum number of fatalities over the three-month event window and the multiple by which those fatalities exceed the highest twelve-month fatality total predicted by any forecast issued over the preceding twelve months. Grey lines are the alternative specifications; the navy line is the baseline specification, with its 95% confidence band shaded. Outcomes enter in inverse hyperbolic sine form. Events additionally require that no month of the preceding twelve reached the implied monthly fatality threshold. The event is dated to the first month of the window and only the first surge per country is used; treatment is absorbing and control observations are drawn from not-yet-treated and never-treated country-months, excluding those within twelve months of a month meeting the fatality threshold. Coefficients are differenced against the pre-treatment mean. The vertical dashed line marks the start of the surge; the horizontal line marks zero. Source: fDiMarkets, Refinitiv Eikon, Sundberg and Melander (2013) and Mueller et al. (2024).



(a) Greenfield projects

(b) M&A deals

(c) FDI sectors

(d) FDI sources

(e) Jobs

(f) Capex

Figure 14: Average effect of unexpected death surges across the parameter grid: LP-DiD estimates

Notes: Each panel summarises the local-projections difference-in-differences estimates for one outcome across the parameter grid defining an unexpected death surge. Each cell corresponds to one combination of the minimum number of fatalities required over the three-month event window (vertical axis) and the multiple by which those fatalities must exceed the highest twelve-month fatality total predicted by any forecast issued over the preceding twelve months (horizontal axis). Cells are shaded by quartile of the average post-treatment coefficient, taken over horizons 0 to 12 months; quartile cut-offs are computed within each panel and therefore differ across outcomes. Outcomes enter in inverse hyperbolic sine form. Events additionally require that no month of the preceding twelve reached the implied monthly fatality threshold, and only the first surge per country is used. Combinations yielding fewer than three events are not estimated and appear as blank cells. Source: fDiMarkets, Refinitiv Eikon, Sundberg and Melander (2013) and Mueller et al. (2024).

Table 8: Specification grid, surge definition: own-country conflict

Mutli.	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
2×	25	35	-0.061	-0.075	-0.057	-0.062	-0.207	-0.189
			(0.114)	(0.110)	(0.105)	(0.099)	(0.394)	(0.345)
3×	25	32	-0.044	-0.066	-0.038	-0.048	-0.141	-0.129
			(0.123)	(0.119)	(0.112)	(0.106)	(0.422)	(0.368)
5×	25	29	-0.096	-0.024	-0.080	-0.089	-0.289	-0.229
			(0.121)	(0.125)	(0.113)	(0.108)	(0.427)	(0.379)
10×	25	21	-0.115	-0.118	-0.103	-0.107	-0.363	-0.312
			(0.146)	(0.159)	(0.137)	(0.129)	(0.485)	(0.428)
15×	25	21	-0.111	-0.123	-0.099	-0.103	-0.346	-0.299
			(0.146)	(0.159)	(0.137)	(0.128)	(0.486)	(0.425)
20×	25	21	-0.111	-0.128	-0.099	-0.103	-0.345	-0.298
			(0.146)	(0.159)	(0.136)	(0.128)	(0.486)	(0.427)
2×	50	29	-0.103	-0.129	-0.093	-0.098	-0.364	-0.265
			(0.122)	(0.125)	(0.114)	(0.108)	(0.441)	(0.399)
3×	50	26	-0.134	-0.170	-0.120	-0.126	-0.416	-0.308
			(0.128)	(0.144)	(0.120)	(0.113)	(0.471)	(0.421)
5×	50	24	-0.158	-0.172	-0.142	-0.149	-0.468	-0.364
			(0.131)	(0.149)	(0.125)	(0.117)	(0.475)	(0.427)
10×	50	17	-0.195	-0.273	-0.174	-0.181	-0.530	-0.416
			(0.156)	(0.184)	(0.150)	(0.139)	(0.561)	(0.512)
15×	50	16	-0.198	-0.269	-0.177	-0.184	-0.549	-0.424
			(0.164)	(0.189)	(0.157)	(0.146)	(0.579)	(0.533)
20×	50	16	-0.198	-0.276	-0.176	-0.184	-0.548	-0.423
			(0.164)	(0.189)	(0.157)	(0.146)	(0.576)	(0.531)
2×	100	16	-0.135	-0.125	-0.132	-0.138	-0.606	-0.565
			(0.180)	(0.146)	(0.170)	(0.158)	(0.592)	(0.520)
3×	100	16	-0.209	-0.198	-0.204	-0.212	-0.831	-0.750
			(0.178)	(0.162)	(0.167)	(0.153)	(0.590)	(0.514)
5×	100	15	-0.184	-0.145	-0.181	-0.189	-0.691	-0.620
			(0.188)	(0.162)	(0.176)	(0.161)	(0.615)	(0.536)
10×	100	9	-0.401*	-0.217	-0.385*	-0.364*	-1.280*	-1.141*
			(0.234)	(0.239)	(0.221)	(0.207)	(0.754)	(0.661)
15×	100	9	-0.401*	-0.217	-0.385*	-0.364*	-1.280*	-1.141*
			(0.234)	(0.239)	(0.221)	(0.207)	(0.754)	(0.661)
20×	100	9	-0.401*	-0.217	-0.385*	-0.364*	-1.280*	-1.141*
			(0.234)	(0.239)	(0.221)	(0.207)	(0.754)	(0.661)
2×	500	13	-0.150	0.082	-0.134	-0.126	-0.536	-0.368
			(0.206)	(0.227)	(0.197)	(0.189)	(0.664)	(0.598)
3×	500	11	-0.166	-0.005	-0.150	-0.139	-0.607	-0.409
			(0.238)	(0.250)	(0.228)	(0.217)	(0.758)	(0.693)
5×	500	10	-0.237	-0.004	-0.216	-0.193	-0.655	-0.511
			(0.249)	(0.275)	(0.239)	(0.230)	(0.831)	(0.750)

continued on next page

Table 8 continued

Mutli.	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
10×	500	9	-0.393*	-0.201	-0.368*	-0.336*	-1.129	-0.909
			(0.218)	(0.217)	(0.208)	(0.201)	(0.759)	(0.711)
15×	500	8	-0.467**	-0.210	-0.440**	-0.405*	-1.325*	-1.122
			(0.225)	(0.233)	(0.215)	(0.208)	(0.761)	(0.694)
20×	500	7	-0.482**	-0.299	-0.444**	-0.432**	-1.469*	-1.269*
			(0.219)	(0.314)	(0.202)	(0.199)	(0.770)	(0.679)
2×	1000	10	-0.312	0.023	-0.290	-0.268	-0.851	-0.722
			(0.259)	(0.302)	(0.249)	(0.238)	(0.845)	(0.738)
3×	1000	9	-0.303	0.020	-0.282	-0.259	-0.794	-0.668
			(0.276)	(0.302)	(0.265)	(0.254)	(0.896)	(0.784)
5×	1000	9	-0.303	0.020	-0.282	-0.259	-0.794	-0.668
			(0.276)	(0.302)	(0.265)	(0.254)	(0.896)	(0.784)
10×	1000	7	-0.514**	-0.321	-0.477**	-0.463**	-1.561*	-1.346*
			(0.221)	(0.319)	(0.202)	(0.204)	(0.797)	(0.696)
15×	1000	7	-0.514**	-0.321	-0.477**	-0.463**	-1.561*	-1.346*
			(0.221)	(0.319)	(0.202)	(0.204)	(0.797)	(0.696)
20×	1000	6	-0.489**	-0.252	-0.459**	-0.444**	-1.391*	-1.246*
			(0.238)	(0.357)	(0.217)	(0.215)	(0.835)	(0.706)
2×	2000	8	-0.367	-0.094	-0.346	-0.319	-1.023	-0.928
			(0.309)	(0.383)	(0.292)	(0.280)	(0.984)	(0.841)
3×	2000	7	-0.360	-0.095	-0.341	-0.311	-0.964	-0.882
			(0.337)	(0.383)	(0.318)	(0.305)	(1.069)	(0.916)
5×	2000	7	-0.284	-0.212	-0.262	-0.257	-0.813	-0.753
			(0.315)	(0.426)	(0.296)	(0.290)	(1.052)	(0.907)
10×	2000	5	-0.433	-0.416	-0.411*	-0.395*	-1.080	-0.969
			(0.264)	(0.381)	(0.234)	(0.240)	(0.898)	(0.772)
15×	2000	5	-0.433	-0.416	-0.411*	-0.395*	-1.080	-0.969
			(0.264)	(0.381)	(0.234)	(0.240)	(0.898)	(0.772)
20×	2000	5	-0.433	-0.416	-0.411*	-0.395*	-1.080	-0.969
			(0.264)	(0.381)	(0.234)	(0.240)	(0.898)	(0.772)

Notes. LP-DiD estimates. Each cell reports the mean coefficient over horizons 0–12 for the burst-definition thresholds in that row; all outcomes are inverse hyperbolic sine transformed. Bursts are defined on own-country conflict deaths. Approximate standard errors in parentheses (mean of the horizon-specific standard errors; exact only under perfect correlation across horizons). The dagger marks the baseline specification. Dashes denote threshold pairs yielding too few events to estimate. $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

E Additional tables for the false alarms vs. materialized risk estimates

Table 9: Estimation sample, false alarms (pre-burst months only)

	Control		Treated			
	Mean	SD	Mean	SD	Diff.	p
Greenfield projects	7.287	18.325	2.402	4.090	-4.886	0.004
M&As	71.662	176.628	13.167	30.648	-58.496	0.001
Subsectors	5.058	10.223	2.089	3.450	-2.969	0.005
Origin countries	3.260	5.266	1.702	2.517	-1.558	0.014
Jobs created	930.006	2,705	322.985	796.650	-607.021	0.007
Capital investment	403.316	1,465	129.652	373.140	-273.664	0.002
Zero-project months (share)	0.406	0.491	0.432	0.496	0.026	0.717
averaged across countries	0.403	0.362	0.432	0.342	0.028	0.693
Countries	134		28			
Country-months	29,635		336			

Notes: Sample as used in the corresponding LP-DiD estimation. A risk burst is an increase of at least 0.25 in the 12-month armed-conflict probability, isolated from other bursts and preceded by twelve months with no month above 25 fatalities. A burst is treated here if no month of the following year records 25 or more fatalities. Countries experiencing the other event type are excluded; remaining controls have no burst and no month above 25 fatalities in the $[-12, +12]$ window. Differences are treated minus control; standard errors clustered by country, robust for the country-average row. Treated countries (ISO3): ARM, BEL, BEN, BFA, BHR, COG, DJI, GIN, GTM, HND, HTI, KGZ, KHM, KWT, LBN, LKA, MAR, MDG, MOZ, MYS, PNG, PRY, RWA, SAU, SLV, TJK, TUN, UGA.

FDI outcomes are of similar levels in false-alarm countries and in those where violence followed, and the difference is not statistically significant. In terms of conflict fatalities in the 12 months before the risk increase, these are also similar and not statistically different in both group of countries, close to zero, as by construction fatalities need to be below 25. The conflict-risk variables differ slightly, but suggest that the risk is slightly higher in false alarm countries, which confirms the revlnace of this treated group. The twelve-month armed-conflict probability averages 0.120 before a false alarm and 0.081 before a materialized burst, a gap of 0.038 that is marginally significant, and the pre-burst minimum differs by a similar amount. Materialized episodes therefore begin from a *lower* forecast level, not a higher one. The size of the risk increase is nearly identical across the two groups (0.010 versus 0.012), which is by construction, since both must clear the same threshold. That the forecast level does not distinguish the two suggests the difference between a false alarm and a materialized escalation is not something the forecast anticipated — which is the premise of using the split at all.

Table 10: Estimation sample, materialized risk (pre-burst months only)

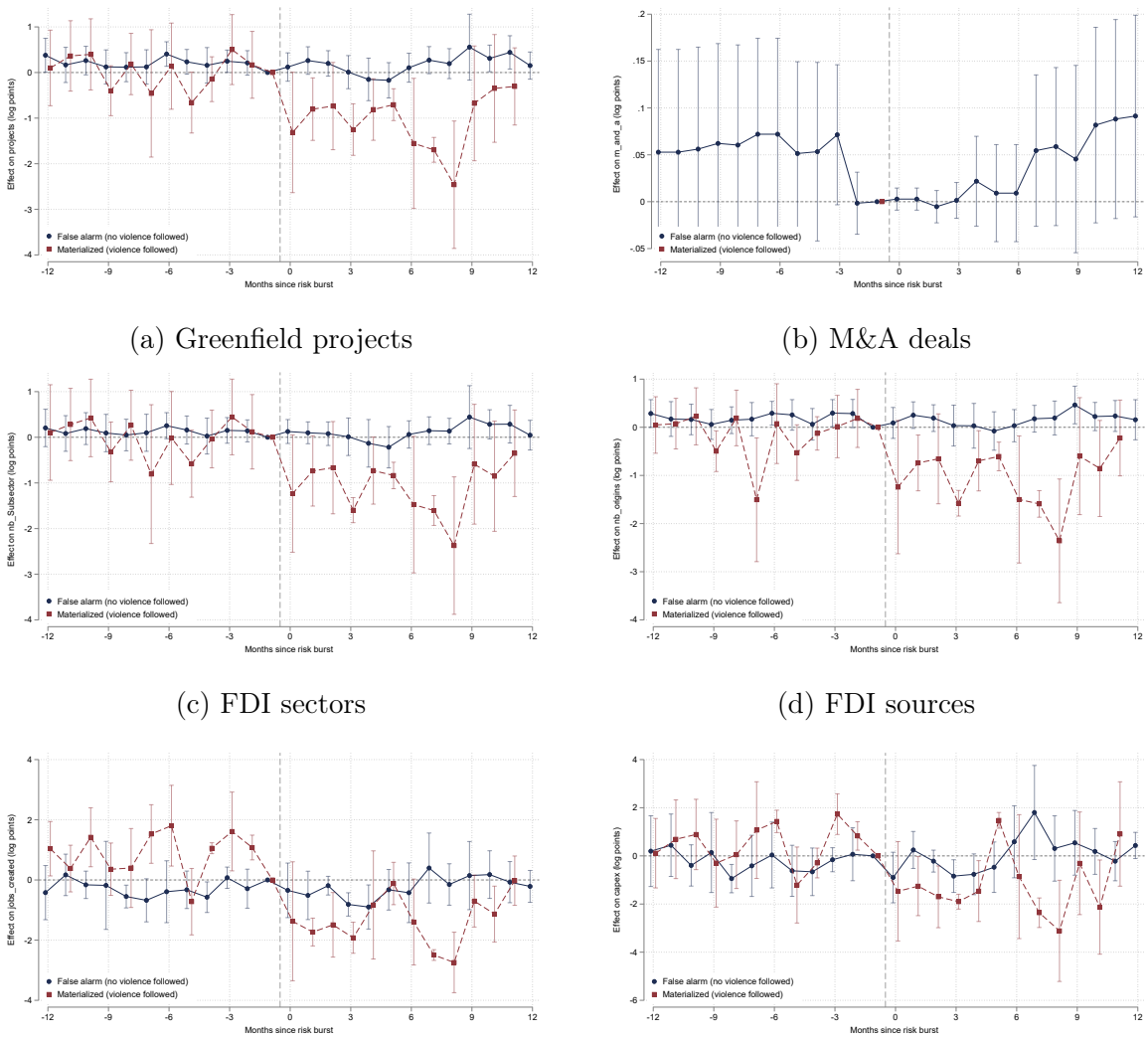
	Control		Treated			
	Mean	SD	Mean	SD	Diff.	p
Greenfield projects	7.287	18.325	2.646	2.740	-4.642	0.016
M&As	71.662	176.628	25.771	39.619	-45.892	0.067
Subsectors	5.058	10.223	2.438	2.517	-2.621	0.057
Origin countries	3.260	5.266	2.167	2.117	-1.093	0.269
Jobs created	930.006	2,705	392.375	619.048	-537.631	0.024
Capital investment	403.316	1,465	141.699	212.853	-261.617	0.006
Zero-project months (share)	0.406	0.491	0.271	0.449	-0.135	0.317
averaged across countries	0.403	0.362	0.271	0.299	-0.133	0.326
Countries	134		4			
Country-months	29,635		48			

Notes: Sample as used in the corresponding LP-DiD estimation. A risk burst is an increase of at least 0.25 in the 12-month armed-conflict probability, isolated from other bursts and preceded by twelve months with no month above 25 fatalities. A burst is treated here if some three-month window beginning in the following year records at least 1000 fatalities. Countries experiencing the other event type are excluded; remaining controls have no burst and no month above 25 fatalities in the $[-12, +12]$ window. Differences are treated minus control; standard errors clustered by country, robust for the country-average row. Treated countries (ISO3): CMR, LBY, SYR, UKR.

Table 11: Pre-burst characteristics: false alarms versus materialized risk

	False alarm		Materialized		
	Mean	SD	Mean	SD	Diff.
Panel A: FDI outcomes					
Greenfield projects	2.402	4.090	2.646	2.740	0.244
M&As	13.167	30.648	25.771	39.619	12.604
Subsectors	2.089	3.450	2.438	2.517	0.348
Origin countries	1.702	2.517	2.167	2.117	0.464
Jobs created	322.985	796.650	392.375	619.048	69.390
Capital investment	129.652	373.140	141.699	212.853	12.047
Zero-project months (share)	0.432	0.496	0.271	0.449	-0.161
Panel B: Conflict risk					
12-month conflict probability	0.120	0.072	0.081	0.049	-0.038*
pre-burst minimum	0.109	0.068	0.069	0.042	-0.040*
size of the increase	0.010	0.041	0.012	0.037	0.0016
Panel C: Fatalities					
Fatalities, monthly	0.199	1.135	0.146	0.875	-0.054
12-month maximum	3.705	19.620	0.083	0.279	-3.622
Countries	28		4		
Country-months	336		48		

Notes: Means over the twelve months preceding each country's own risk burst. A risk burst is an increase of at least 0.25 in the twelve-month armed-conflict probability, isolated from other bursts and preceded by twelve months with no month above 25 fatalities. A burst is a false alarm if no month of the following year records 25 or more fatalities, and materialized if some three-month window beginning in the following year records at least 1000 fatalities. Both columns are treated groups; there is no control group in this table. Differences are materialized minus false alarm, with standard errors clustered by country. Given the small number of countries in each group, the p-values should be read as indicative. False-alarm countries: ARM BEL BEN BFA BHR COG DJI GIN GTM HND HTI KGZ KHM KWT LBN LKA MAR MDG MOZ MYS PNG PRY RWA SAU SLV TJK TUN UGA. Materialized: CMR LBY SYR UKR. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.



(a) Greenfield projects

(b) M&A deals

(c) FDI sectors

(d) FDI sources

(e) Jobs

(f) Capex

Figure 15: FDI response to conflict risk bursts: false alarms versus materialized risk (Poisson)

Notes: As Figure 8, but estimated by Poisson pseudo-maximum-likelihood on raw counts using stacked event cohorts, with stack-specific country and month fixed effects and standard errors clustered by country. Coefficients are in log points, so $\exp(\beta) - 1$ gives the proportional effect on the expected outcome, and the month before the burst is the omitted reference period. A risk burst is an increase of at least 0.25 in the 12-month armed-conflict risk forecast relative to its minimum over the preceding three months, from a base below 0.25, with no other burst in the preceding six months and no month reaching 25 fatalities in the preceding twelve. Bursts are split by what followed: *false alarms* (navy circles) are those with no month reaching 25 fatalities over the subsequent twelve months, while *materialized* bursts (maroon squares) are those in which some three-month window beginning in the following year records at least 1,000 fatalities, the same fatality criterion used in the main design; bursts falling between these thresholds enter neither group. Source: fDiMarkets, Refinitiv Eikon, Sundberg and Melander (2013) and Mueller et al. (2024).

F Heterogeneity by Sector

In this section we explore the heterogeneity in the relationship between conflict and greenfield FDI across different sectors. Because of the sparse nature of the FDI data across country sectors and months, we construct a sector-by-country-month panel of greenfield project counts and group the sectors into five categories *Resources and energy* covers coal, oil and gas, renewable energy, minerals and metals: activity tied to location-specific deposits that cannot be relocated. *Heavy manufacturing* covers automotive, chemicals, building materials, industrial equipment, textiles and related sectors, characterized by large sunk capital commitments and long payback periods. *Technology* covers semiconductors, electronics, software and IT services, communications, pharmaceuticals and medical devices, where production is more footloose and human capital more mobile. *Consumer-facing and local market* covers consumer products, food and beverages, hotels and tourism, leisure, real estate and healthcare: activity that serves the domestic market and depends on the physical safety of customers and premises. *Services and logistics* covers business services, financial services, and transportation and warehousing.

Figure 16 plots the event-study path for each group and Figure 17 summarizes them as average effects over the twelve months following the burst. We find significant and negative effects in Services and logistics as well as heavy manufacturing. Overall, the estimates are suggestive but insufficiently precise to establish cross-sector differences, due to the sparsity of the data at that level of disaggregation.

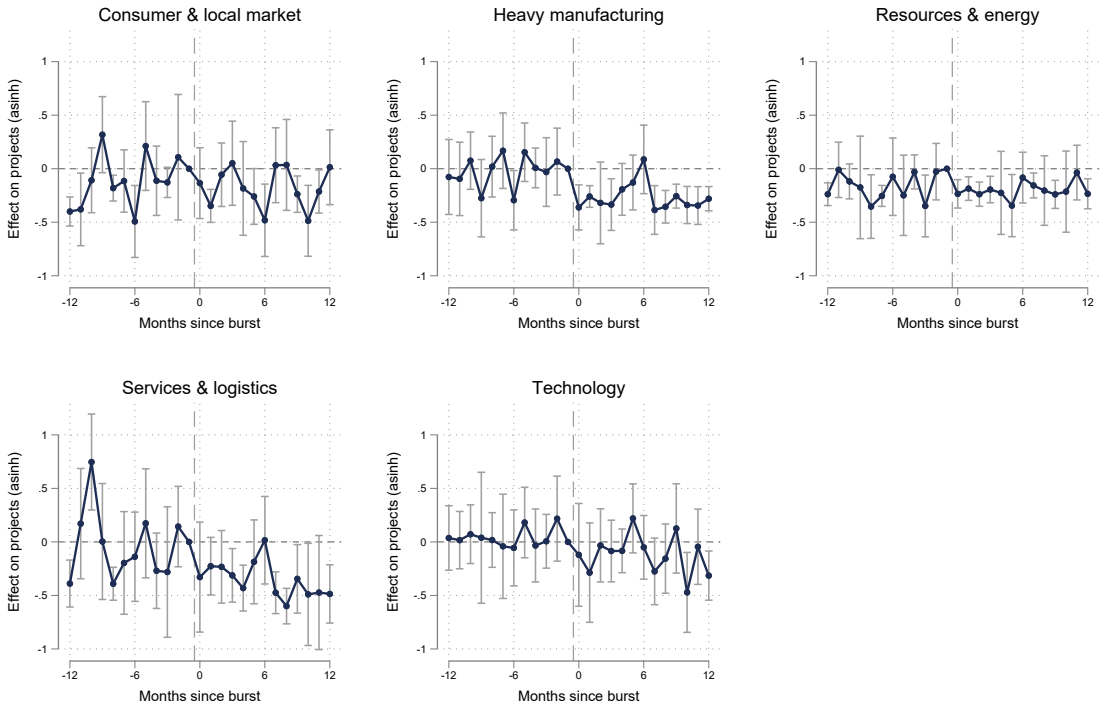


Figure 16: FDI response to violence bursts, by sector group

Notes: Each panel plots event-study coefficients with 95% confidence intervals for the monthly number of greenfield FDI projects in one group of sectors. Sectors are grouped as follows. *Resources and energy*: coal, oil and gas; renewable energy; minerals; metals. *Heavy manufacturing*: automotive OEM; automotive components; building materials; ceramics and glass; chemicals; engines and turbines; industrial equipment; non-automotive transport OEM; paper, printing and packaging; plastics; rubber; wood products; textiles. *Technology*: semiconductors; electronic components; consumer electronics; software and IT services; communications; biotechnology; pharmaceuticals; medical devices; business machines and equipment; space and defence. *Consumer-facing and local market*: consumer products; food and beverages; hotels and tourism; leisure and entertainment; real estate; healthcare. *Services and logistics*: business services; financial services; transportation and warehousing. A violence burst is a three-month window in which recorded fatalities total at least 1,000 and are at least ten times the country's total over the preceding twelve months; the event is dated to the first month of the window and only the first burst per country is used. Treatment is absorbing and control observations are drawn from not-yet-treated and never-treated country-months, excluding those within twelve months of a month meeting the fatality threshold. These are local-projections difference-in-differences estimates with the outcome in inverse hyperbolic sine form, differenced against the pre-treatment mean. Source: fDiMarkets, Refinitiv Eikon, Sundberg and Melander (2013).

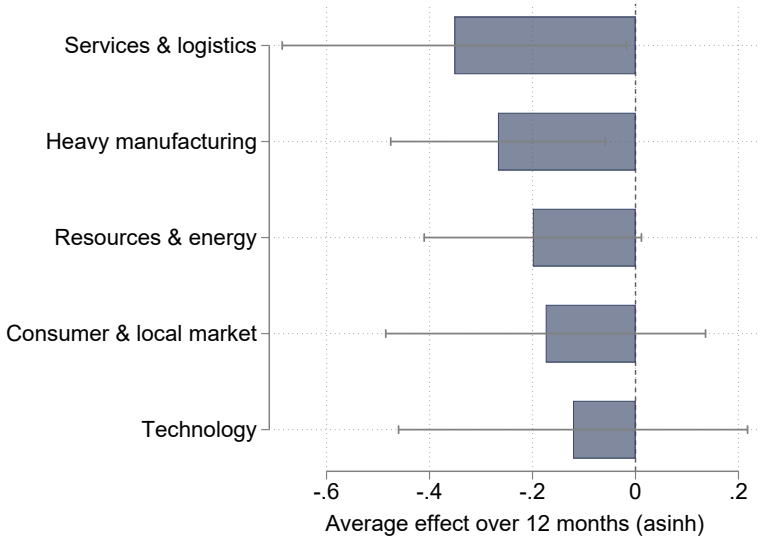


Figure 17: Average effect of violence bursts on greenfield FDI projects, by sector group

Notes: Each bar is the average post-treatment effect of a violence burst on the monthly number of greenfield FDI projects in one group of sectors, taken over horizons 0 to 12 months, with the whiskers giving the corresponding average confidence bounds. Sectors are grouped as follows. *Resources and energy*: coal, oil and gas; renewable energy; minerals; metals. *Heavy manufacturing*: automotive OEM; automotive components; building materials; ceramics and glass; chemicals; engines and turbines; industrial equipment; non-automotive transport OEM; paper, printing and packaging; plastics; rubber; wood products; textiles. *Technology*: semiconductors; electronic components; consumer electronics; software and IT services; communications; biotechnology; pharmaceuticals; medical devices; business machines and equipment; space and defence. *Consumer-facing and local market*: consumer products; food and beverages; hotels and tourism; leisure and entertainment; real estate; healthcare. *Services and logistics*: business services; financial services; transportation and warehousing. A violence burst is a three-month window in which recorded fatalities total at least 1,000 and are at least ten times the country's total over the preceding twelve months; the event is dated to the first month of the window and only the first burst per country is used. These are local-projections difference-in-differences estimates with the outcome in inverse hyperbolic sine form. The corresponding event-study paths are shown in Figure 16. Source: fDiMarkets, Refinitiv Eikon, Sundberg and Melander (2013).

G Geographic spillovers

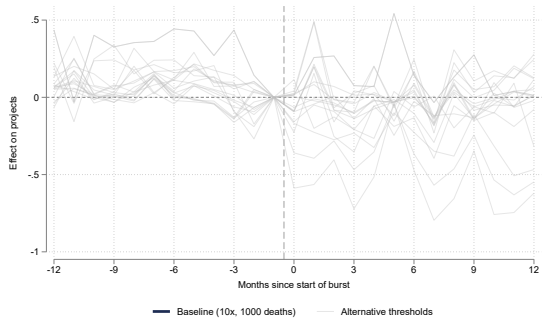
Conflict may not only affect countries directly embroiled in armed violence. The risk of conflict itself might deter FDI, and this may be linked to armed conflict in neighboring countries transmitted through regional instability and the diffusion of armed groups across borders. Take Jordan for example. Although the country is generally peaceful, it is situated in the midst of several countries that have experienced major episodes of armed violence in recent years. Such proximity to conflict may generate investment costs even in the absence of violence within the country.

To assess whether violence deters investment beyond the country in which it occurs, we replace D_{it} in equation (2) with the total fatalities recorded in countries sharing a land border with i. To isolate the spillover from any direct effect, we retain an event only where the country itself records no month with twenty-five or more fatalities from twelve months before through twelve months after the burst, and we require control countries to be free of violence both domestically and in their neighborhood over the same window. The estimating equation and the specification grid are otherwise unchanged.

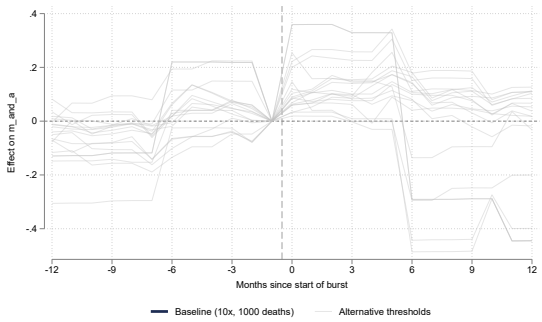
Requiring the country itself to stay peaceful through a large burst of violence next door is demanding, and no country-month in our panel satisfies it at neighbor-fatality floors above one hundred. The grid in Table 12 therefore covers floors of twenty-five, fifty and one hundred only, against the six floors available in the own-country design. We can say nothing about spillovers from the very largest episodes of violence, which are precisely the episodes for which the own-country effects were strongest.

Figure 18 plots the event-study paths and Figure 19 summarizes the grid; Table 12 reports the average post-burst coefficients. We find no evidence that violence in neighboring countries reduces FDI into peaceful ones. Not one of the 108 coefficients in the table is statistically distinguishable from zero at conventional levels. The point estimates are small and inconsistent in sign. At a neighbor-fatality floor of twenty-five, the effect on greenfield projects ranges from -0.084 to $+0.058$ depending on the surprise ratio; at a floor of fifty it ranges from -0.155 to $+0.130$. Estimates at a floor of one hundred are larger in magnitude—reaching -0.556 for projects at a ten-fold ratio—but rest on four to eleven events and carry standard errors of comparable size, so they are uninformative. The sign reverses between the ten-fold and fifteen-fold ratios at every floor, which is a further indication that the variation being picked up is noise rather than a response to violence. M&A estimates are positive in fourteen of the eighteen cells, though never significantly so. We do not think this reflects investment diverting toward peaceful neighbors when war breaks out nearby, though such a diversion effect is theoretically possible and would be an interesting finding if it survived scrutiny. The estimates are too imprecise to support it, and we note it only to be explicit that the null result runs in both directions.

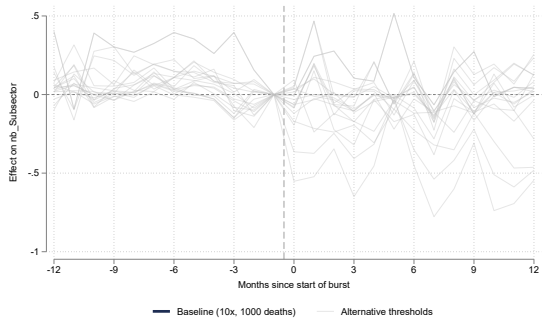
Two readings are consistent with these estimates, and we cannot distinguish between them. The first is that there is no spillover. Investors evaluating a country respond to the security situation within that country, and violence next door does not change their assessment so long as it stays next door. This would be consistent with the finding of Section **??** that firms respond to realized violence rather than to elevated risk. The second is that the design cannot detect a spillover even if one exists. The sample selection is severe by construction, countries that experience a major burst of violence next door and yet remain entirely peaceful themselves are, almost by definition, unusually well insulated from regional instability, whether through geography, state capacity, or a border that is closed in practice. The countries that would be most affected by a neighbor's war are precisely those most likely to be drawn into it, and they are excluded. The estimates are therefore best read as the effect on the countries most able to absorb a regional shock, which is plausibly a lower bound on the average spillover.



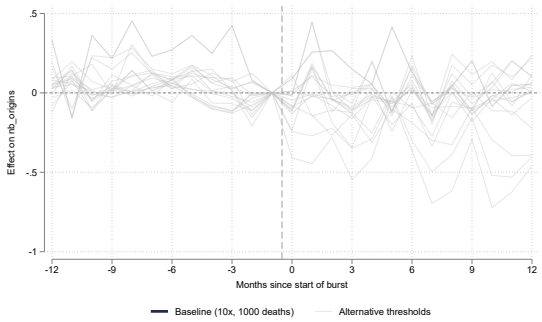
(a) Greenfield projects



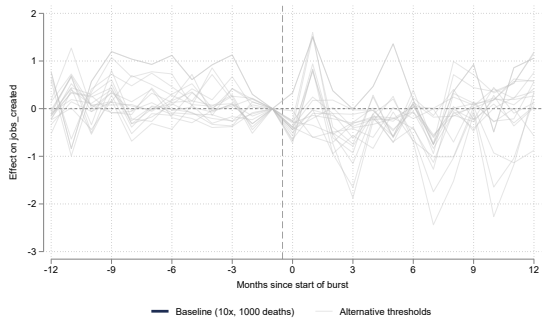
(b) M&A deals



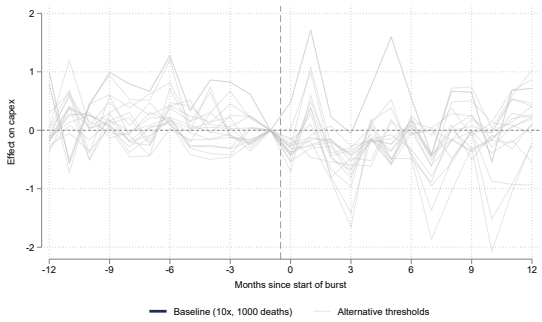
(c) FDI sectors



(d) FDI sources



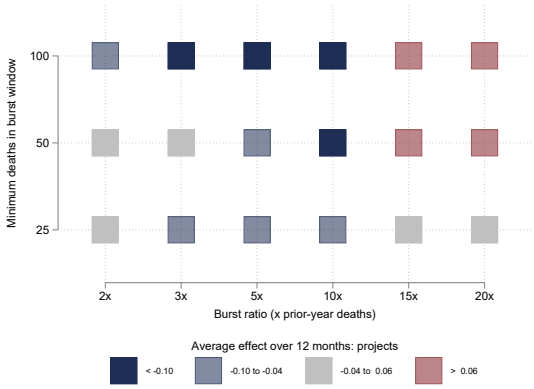
(e) Jobs



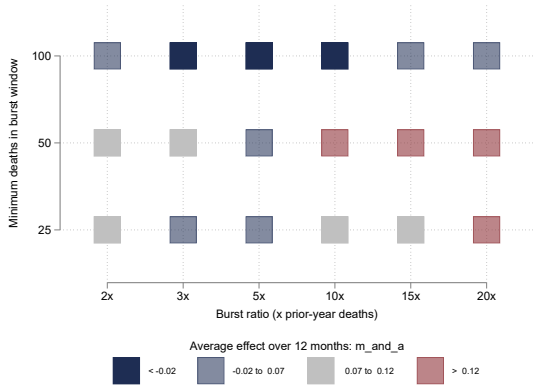
(f) Capex

Figure 18: FDI response to violence bursts in neighboring countries across event definitions: LP-DiD estimates

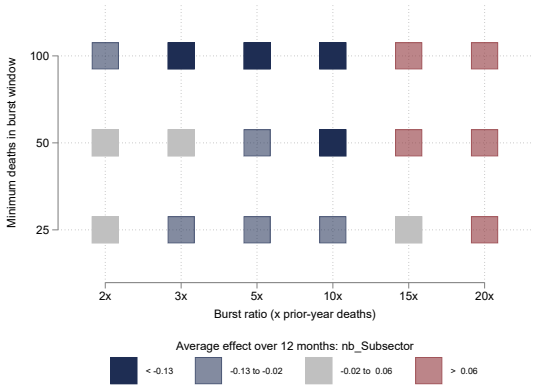
Notes: Each panel plots local-projections difference-in-differences event-study paths for for different FDI outcomes, estimated once for every combination of the two parameters defining a burst of violence in neighboring countries: the minimum number of fatalities recorded in neighboring countries over the three-month burst window and the multiple by which those fatalities exceed the corresponding total over the preceding twelve months. Outcomes enter in inverse hyperbolic sine form. To isolate the spillover from any direct effect of domestic conflict, events are retained only where the country itself records no month with 25 or more fatalities from twelve months before through twelve months after the burst. The event is dated to the first month of the burst window and only the first burst per country is used; treatment is absorbing and control observations are drawn from not-yet-treated and never-treated country-months, excluding those within twelve months of a month meeting the fatality threshold either domestically or in a neighboring country. Coefficients are differenced against the pre-treatment mean. The vertical dashed line marks the start of the burst; the horizontal line marks zero. Source: fDiMarkets, Refinitiv Eikon, and Sundberg and Melander (2013).



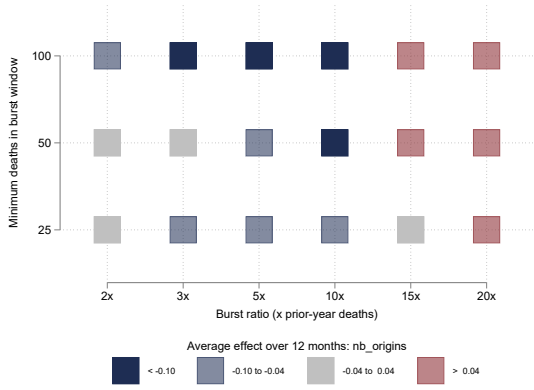
(a) Greenfield projects



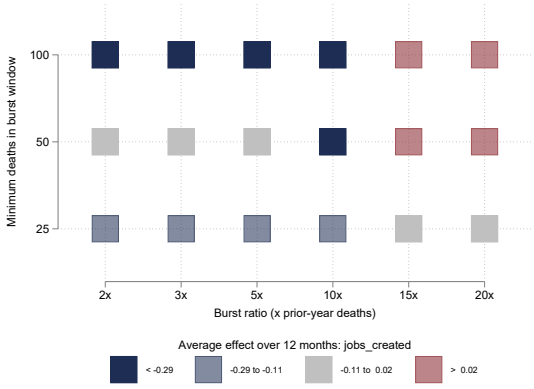
(b) M&A deals



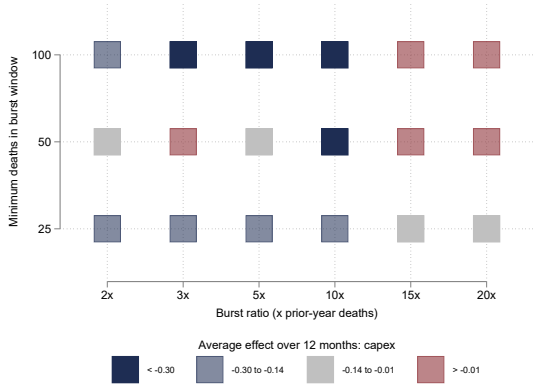
(c) FDI sectors



(d) FDI sources



(e) Jobs



(f) Capex

Figure 19: Average effect of violence bursts in neighboring countries across the parameter grid: LP-DiD estimates

Notes: Each panel summarizes the local-projections difference-in-differences estimates for different FDI outcomes across the parameter grid defining a burst of violence in neighboring countries. Each cell corresponds to one combination of the minimum number of fatalities required in neighboring countries over the three-month burst window (vertical axis) and the multiple by which those fatalities must exceed the corresponding total over the preceding twelve months (horizontal axis). Cells are shaded by quartile of the average post-treatment coefficient, taken over horizons 0 to 12 months; quartile cut-offs are computed within each panel and therefore differ across outcomes. Outcomes enter in inverse hyperbolic sine form. To isolate the spillover from any direct effect of domestic conflict, events are retained only where the country itself records no month with 25 or more fatalities from twelve months before through twelve months after the burst. Combinations yielding fewer than three events are not estimated. Source: fDiMarkets, Refinitiv Eikon, and Sundberg and Melander (2013).

Table 12: Specification grid: neighbouring-country conflict bursts

Ratio	Floor	Events	Greenfield projects	M&As	Subsectors	Origin countries	Jobs created	Capital investment
2×	25	46	-0.035	0.094	-0.021	-0.036	-0.185	-0.199
			(0.114)	(0.095)	(0.107)	(0.099)	(0.359)	(0.318)
3×	25	46	-0.040	0.041	-0.027	-0.040	-0.190	-0.194
			(0.113)	(0.106)	(0.106)	(0.098)	(0.354)	(0.313)
5×	25	41	-0.036	0.066	-0.029	-0.041	-0.140	-0.177
			(0.126)	(0.104)	(0.116)	(0.107)	(0.379)	(0.334)
10×	25	35	-0.084	0.096	-0.066	-0.075	-0.249	-0.264
			(0.139)	(0.113)	(0.129)	(0.119)	(0.414)	(0.367)
15×	25	33	0.029	0.116	0.037	0.018	-0.027	-0.091
			(0.108)	(0.110)	(0.100)	(0.094)	(0.382)	(0.338)
20×	25	31	0.058	0.128	0.058	0.041	0.022	-0.043
			(0.111)	(0.117)	(0.102)	(0.096)	(0.393)	(0.344)
2×	50	32	-0.010	0.079	-0.002	-0.003	-0.070	-0.076
			(0.150)	(0.118)	(0.139)	(0.128)	(0.438)	(0.387)
3×	50	29	0.002	0.096	0.011	0.006	0.005	-0.009
			(0.160)	(0.127)	(0.148)	(0.135)	(0.456)	(0.404)
5×	50	24	-0.051	0.045	-0.038	-0.037	-0.042	-0.107
			(0.190)	(0.166)	(0.176)	(0.160)	(0.514)	(0.457)
10×	50	19	-0.155	0.125	-0.131	-0.122	-0.347	-0.312
			(0.221)	(0.184)	(0.205)	(0.185)	(0.586)	(0.535)
15×	50	18	0.089	0.176	0.094	0.079	0.144	0.115
			(0.146)	(0.165)	(0.131)	(0.123)	(0.496)	(0.460)
20×	50	17	0.130	0.204	0.125	0.108	0.234	0.183
			(0.146)	(0.173)	(0.132)	(0.123)	(0.495)	(0.452)
2×	100	11	-0.099	0.034	-0.130	-0.098	-0.289	-0.295
			(0.349)	(0.229)	(0.312)	(0.282)	(0.784)	(0.693)
3×	100	10	-0.283	-0.116	-0.266	-0.233	-0.656	-0.481
			(0.342)	(0.218)	(0.316)	(0.282)	(0.791)	(0.725)
5×	100	7	-0.405	-0.219	-0.373	-0.322	-0.624	-0.475
			(0.489)	(0.284)	(0.454)	(0.404)	(1.077)	(1.002)
10×	100	5	-0.556	-0.230	-0.512	-0.453	-0.881	-0.718
			(0.651)	(0.341)	(0.604)	(0.536)	(1.420)	(1.321)
15×	100	4	0.139	-0.021	0.135	0.121	0.475	0.542
			(0.211)	(0.284)	(0.185)	(0.159)	(0.732)	(0.718)
20×	100	4	0.139	-0.021	0.135	0.121	0.475	0.542
			(0.211)	(0.284)	(0.185)	(0.159)	(0.732)	(0.718)

Notes. Each cell reports the mean LP-DiD coefficient over horizons 0–12 for the burst-definition thresholds in that row; all outcomes are inverse hyperbolic sine transformed. Bursts are defined on neighbours' conflict deaths; country-months with own-country violence are excluded from both treated and control units. Approximate standard errors in parentheses (mean of the horizon-specific standard errors; exact only under perfect correlation across horizons). Bold indicates the baseline specification. Dashes denote threshold pairs yielding too few events to estimate. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.

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