Under pressure: Global evidence on conflict and firms*

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ABSTRACT

This paper provides the first global analysis of the impact of conflict exposure on firm performance, combining geolocalized longitudinal firm-level data with information on political violence events across 89 countries between 2006 and 2019. Our results show that higher conflict exposure leads to declines in both sales and total costs, resulting in no significant effect on profits for surviving firms. The reduction in sales is driven by lower output, which reflects conflict-induced shortages of raw materials and production inputs, as well as increased informal competition. Firms respond to declining sales by adjusting labor costs, substituting skilled workers with unskilled ones. The negative effects of conflict on firms are more pronounced in countries with high trade openness, low economic complexity, weak bureaucratic quality, involvement in illegal drug production, or an initial state of peace. In these contexts, greater conflict exposure leads to reductions in both sales and profits.

Keywords: firm, conflict, political violence, intermediate inputs, labor market, market competition, informality

JEL codes: H56, D22, E26, O17

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1 Introduction

Conflict and political violence have become the *new normal* in many developing and developed economies, with over 1.7 billion people worldwide living in fragile or conflict-affected countries. In 2024 alone, 167 countries reported incidents of political violence, with 50 facing extreme, high, or turbulent levels of conflict —resulting in more than 233,000 fatalities (ACLED, 2024).[1]

Despite the severe suffering and disruption caused by conflict and political violence, economic activity persists. Economists and policymakers are increasingly seeking to understand how and why this occurs across different contexts. Yet, research on firm behavior in conflict-affected settings remains limited. While existing studies provide rigorous estimates of the effect of conflict on firm performance and explore underlying mechanisms in depth, their findings are largely country- and conflict-specific (see, e.g., Couttenier et al., 2022; Korovkin and Makarin, 2023; Bernal et al., 2024).

This paper is the first to provide a global perspective on the impact of conflict on a range of firm-level outcomes, along with the mechanisms underlying these effects. Moreover, by exploiting differences in cross-country characteristics, it shows how economic, institutional, and political factors shape the influence of conflict exposure.

Our analysis relies on the combination of two main data sources. First, we use a confidential version of the global World Bank Enterprise Survey (WBES), which, beyond the rich firm-level data in the public version, includes geolocation information for each firm. We focus on its panel component, covering 89 countries from 2006 to 2019, and exclude the COVID-19 pandemic period. Second, we draw on the Integrated Crisis Early Warning System (ICEWS) dataset to obtain geolocated data on conflict and political violence events in each country. This allows us to construct a firm-specific, time-varying measure of conflict exposure, which we use in a firm-level fixed-effects model to estimate the impact of conflict on several outcomes.

[1]Political violence refers to the intentional use of power or force to achieve political objectives. It includes war and related violent conflicts, state violence, and similar acts carried out by organized groups WHO (2002). Throughout the paper, we use the terms exposure to political violence events and exposure to conflict-related events interchangeably.

This study yields a number of empirical findings. We show that higher conflict exposure —as measured by the number of violent events occurring in the neighborhood around a firm— leads to declines in both sales and total costs, with no significant effect on profits among surviving firms. However, because conflict reduces both revenue and spending on production inputs, it implies a contraction in output. Thus, even if profits remain stable for surviving firms, the overall level of economic activity appears to be severely hampered by conflict-related violence.

We address several potential identification threats to the interpretation of our empirical results. First, we tackle the issue of reverse causality by showing that the local intensity of conflict is not driven by the level or structure of local economic activity, nor is the occurrence or intensity of conflict events systematically related to firm characteristics. We also show that our results remain robust across several alternative specifications. These include controlling for placebo firm exposure to account for potential non-random assignment of conflict events, restricting the estimation sample to clean controls, and incorporating the conflict exposure of suppliers and buyers to address potential violations of the SUTVA. Moreover, we address the possibility that our findings are driven by selection on unobservables. Finally, we show that our conclusions are robust to a range of sample restrictions, alternative definitions of conflict exposure —including variations in buffer size, time window, event type, and data source— as well as to different choices for clustering standard errors.

Next, we explore the mechanisms through which conflict exposure reduces sales and total costs, ultimately leading to a decline in output. We first show that conflict leads to a reduction in firms' purchases of intermediate inputs and raw materials —both domestic and imported— as well as electricity. Given that prices are unlikely to decrease during periods of conflict, this offers indirect evidence that firms are purchasing fewer production inputs. Furthermore, conflict disrupts access to these inputs by increasing import clearance times and intensifying both the frequency and duration of power outages, further contributing to the decline in output. In addition, conflict exacerbates the informal competition faced by firms. Since informal competition reduces output, employment,

and investment among formal firms, this channel contributes to explaining the observed decline in sales.

Faced with these pressures, firms respond by adjusting their cost structures to maintain profitability. While limited access to inputs mechanically lowers expenditures, firms also reduce labor costs by substituting skilled workers with unskilled ones. We also find that conflict reduces sales by decreasing both domestic and foreign demand. In contrast, other potential channels —such as increased credit constraints, corruption, or crime— do not appear to explain our findings. Taken together, these findings indicate that conflict reduces sales through lower output, intensified informal competition, and diminished demand, while profits remain stable due to firms' adaptive cost-cutting responses.

A key advantage of using a harmonized global dataset is the possibility to compare the effects of conflict on firms across countries with different structural and institutional characteristics. Our results show no meaningful heterogeneity in the effects of conflict exposure among firms operating in countries with differing levels of per capita income, fragility, or reliance on agriculture and natural resources. However, we do find substantial variation along other dimensions. Conflict exposure has more negative effects —reducing both sales and profits— for firms operating in countries with lower economic complexity, greater openness to international trade, weaker bureaucratic quality, higher corruption levels, or involvement in illegal drug production. Importantly, the impact of conflict also differs depending on a country's initial conflict status. In countries that are initially at peace, increased conflict exposure leads to a sizable decline in both sales and profits, suggesting that conflict carries higher costs for firms in more stable settings. Moreover, by comparing firms in these countries to those located in countries already affected by conflict at the beginning of the period, we also show that the relative importance of the mechanisms through which conflict affects firm performance varies with this country characteristic.

Our paper contributes to the growing literature on the microeconomic consequences of conflict. This strand of research has largely focused on outcomes related to education and health (for reviews, see Verwimp et al., 2019; Rohner, 2023). In contrast, studies

examining the economic effects of conflict remain relatively limited —partly due to the difficulty of accessing reliable economic data in conflict-affected settings.[2] While early work explored the macroeconomic effects of conflict (e.g., Abadie and Gardeazabal, 2003; Martin et al., 2008; Glick and Taylor, 2010), more recent empirical studies have shifted attention to its impact on firms.[3] These studies examine various forms of conflict and political violence in specific country contexts —including Afghanistan (Blumenstock et al., 2024), Angola (Guidolin and La Ferrara, 2007), Colombia (Camacho and Rodriguez, 2013; De Roux and Martinez, 2023; Bernal et al., 2024), India (Couttenier et al., 2022), Kenya (Ksoll et al., 2023), Libya (Del Prete et al., 2023), Mozambique (Custodio et al., 2025), Ukraine (Korovkin et al., 2024; Korovkin and Makarin, 2023), Sierra Leone (Collier and Duponchel, 2013), and the West Bank (Amodio and Di Maio, 2018). However, these are all single-country analyses that focus on different outcomes, rely on distinct data sources, and employ different methodological approaches, making their results not directly comparable.

This paper contributes to the literature along three main dimensions. First, it is the first global study to examine the effects of conflict on firms. By leveraging harmonized firm-level data from both developed and developing countries, we document several empirical regularities that hold across different settings and contexts. Second, we assess how the effects of conflict exposure vary across a wide range of firm- and country-level characteristics, showing that country-specific factors play a central role in shaping the impact of conflict on firm performance. Third, we identify several mechanisms —reduced access to inputs, changes in workforce composition, increased informal competition, and declining demand— that help explain the adverse effects of conflict. While some of these channels have been examined in previous studies, we show that their relative importance depends on a country's characteristics.

Overall, our findings underscore that the effects of conflict on firms are highly heterogeneous and operate through context-specific channels. From a policy perspective, this suggests that interventions in conflict and post-conflict settings should be tailored to the

[2] A related literature examines the economic determinants of conflict, including price shocks (Dube and Vargas, 2013), natural resources (Berman et al., 2017), and import restrictions (Amodio et al., 2021).

[3] A parallel line of research documents the effects of crime-related violence on economic activity (Pinotti 2015; Rozo 2018; Utar 2024; Piemontese 2023).

specific conditions of each country to effectively support firms.

The paper proceeds as follows. Section 2 describes the data. Section 3 outlines the empirical strategy. Section 4 presents the results. Section 5 concludes.

2 Data

2.1 Firm data

Our main data source is the World Bank Enterprise Survey (WBES), a global dataset providing firm-level information for the manufacturing, retail, and other service sectors across 148 countries.[4] Data are collected through face-to-face interviews using a standardized questionnaire and are designed to be representative at the country level. The survey targets privately owned firms with at least five employees operating in the formal (non-agricultural) sector.[5] Our data cover the period 2006-2019.

We use a confidential version of the WBES global dataset, which also provides firms' geo-localization. This information allows us to match each firm with conflict and political violence events occurring in its geographical neighborhood and is therefore essential for constructing the firm-specific measure of conflict exposure used in our analysis.[6] Another important feature of the WBES is that —although designed as a repeated cross-section— it includes a sizable panel component for a subset of countries, with some firms interviewed in multiple waves. As discussed in Section 3, our estimation strategy relies on firms for which geo-localized data are available and that belong to this panel component. Restricting further to firms with non-missing sales data, our main estimating sample consists of 36,087 firm-year observations across 89 countries.[7] Table A1 provides a detailed

[4]The WBES dataset is the most comprehensive resource for conducting firm-level studies worldwide, as demonstrated in Fisman et al. (2024) and Armangué-Jubert et al. (2024).

[5]Firms are selected using random sampling techniques with three stratification levels to ensure national representativeness across firm size (5–19 employees; 20–99 employees; and 100+ employees) and sector (manufacturing, retail, and other services). For details on the WBES sampling methodology, see https://www.worldbank.org/content/dam/enterprisesurveys/documents/methodology/Enterprise%20Surveys_Manual%20and%20Guide.pdf.

[6]The WBES collected information on firm geo-localization in 120 countries. This information is missing for 5% of firm-year observations. For firms with geo-localization available for at least one survey wave, we impute the same location across other waves to increase the sample size. Results are virtually identical when excluding firms with imputed geo-localization.

[7]Of the 120 countries for which there is information on firm geo-localization, 89 have at least two survey

list of the countries and survey waves included in our analysis, while Figure 1 shows their geographical distribution.[8]

A typical firm in our sample is domestically owned and operates in the private sector. As shown in Table 1, the median firm has 22 employees and has been operating for 16 years. It mainly serves the domestic market —only 16% of firms are exporters— and generates $334,700 in annual sales and $271,800 in annual profits (in 2000 constant USD).[9] All variables are defined in Table C1.

Selection, attrition, and missing values As discussed in Section 3, our estimating strategy relies on the subset of firms with multiple observations over time, i.e., those included in the panel component of the WBES. This may raise concerns about potential selection issues affecting our estimating sample. In particular, one might worry about the self-selection of firms that are re-interviewed in subsequent survey waves, vis à vis firms that, for various reasons, are interviewed only once. If such selection is correlated with our main regressors of interest and outcome variables, it could bias our results. In Table A2, we test whether firms included in the panel (and thus in our estimation sample) differ systematically from those in the cross-section. Specifically, we consider all firms in countries with at

waves: 42 have two waves, 45 have three, and two have four. Across these country-waves, the sample includes 40,884 firm-year observations, corresponding to 16,608 firms observed twice and 2,556 observed three times. Dropping firms without sales data yields a final sample of 36,087 firm-year observations across 89 countries.

[8]Given the focus of our analysis and the empirical approach adopted, the WBES offers better coverage than any alternative source, including the ORBIS Bureau van Dijk dataset. ORBIS does not cover 47 countries in our sample, including Afghanistan, Azerbaijan, Bhutan, Bolivia, Burkina Faso, Cambodia, Chad, Croatia, El Salvador, Ethiopia, Georgia, Ghana, Guatemala, Honduras, Hungary, Indonesia, Jordan, Kenya, Kyrgyzstan, Lebanon, Lesotho, Malawi, Myanmar, Nepal, Niger, Panama, Paraguay, Peru, Rwanda, Senegal, Sierra Leone, Slovenia, South Africa, Suriname, Tajikistan, Tanzania, Timor-Leste, Togo, Tunisia, Turkey, Uganda, Uruguay, Uzbekistan, Venezuela, Vietnam, Yemen, Zambia, and Zimbabwe. In addition, for 18 countries —Angola, Argentina, Benin, Botswana, Cameroon, Cape Verde, Congo, Côte d'Ivoire, Dominica, Ecuador, Egypt, Lao People's Democratic Republic, Liberia, Mali, Mongolia, Nicaragua, Nigeria, and West Bank and Gaza— the number of panel firms with available sales data in ORBIS is lower than in WBES.

[9]Profits are not directly provided in the WBES. We construct *Profits* as *Sales* minus *Total costs*, defined as the sum of labor costs and raw materials and intermediate inputs costs, including energy expenditures. For the latter group, the WBES questionnaire administered to service firms only collects information on electricity expenses. As a result, for service firms, the *Total costs* variable may underestimate overall production expenditures (e.g., by excluding costs related to equipment, telecommunication services, etc.), potentially affecting the comparability of *Profits* across sectors. To address this, we exclude from the sample all service firms with a *Total costs*-to-*Sales* ratio below the fifth percentile of the distribution. This restriction improves the reliability of economic performance comparisons across sectors. Nonetheless, our results are virtually unchanged when focusing on manufacturing firms (see Section 4.1, Table A14).

least two survey waves and for which geo-localization data are available. We then examine whether the likelihood of being interviewed a second time —i.e., of being included in the panel— is correlated with key variables used in the analysis: *Sales*, *Total costs*, *Profits*, and *Conflict exposure* (defined below in Section 2.2). Results in columns 1-5 show that the probability of inclusion in the estimation sample is not driven by these characteristics, allaying concerns about systematic differences between panel and non-panel firms.

Another potential concern with our data is that missing values may be correlated with conflict exposure, possibly biasing the estimation sample.[10] Results reported in Table A3 suggest that this is not the case. Column 1 shows that, in the full sample, our measure of conflict exposure (see Section 2.2) is not correlated with firms *not reporting* sales. Column 2 confirms that this result also holds in the subsample of firms for which baseline control variables are available. Moreover, in the estimation sample, conflict exposure is not correlated with non-reporting of raw material and intermediate input costs (column 3) or labor costs (column 4). Taken together, these results indicate that, for the firm outcome variables collected in the WBES, non-reporting is not systematically associated with conflict exposure.

Finally, we explore potential issues related to attrition. In Table A4, we test whether conflict exposure is correlated with firms exiting our sample. As expected, the results show that firms are more likely to exit when they are exposed to a higher number of conflict events between two survey waves, and when their pre-exit sales levels are lower. This pattern suggests that, if anything, our estimates of the negative effect of conflict on sales may be downward biased —potentially underestimating the broader impact of conflict on economic activity.

[10] A related concern is that firms may strategically choose not to report their performance to avoid being targeted by fighters. However, it is worth noting that participation in the WBES is voluntary, and firms are under no obligation to respond to the questionnaire. Moreover, the number of missing values is higher for *Total Costs* than for *Sales* (see the number of observations in Table 2), which suggests that non-reporting is unlikely to be a strategic choice of firms trying to hide their economic performance.

2.2 Conflict data

Our main source of data on political violence events is the Integrated Crisis Early Warning System (ICEWS, Shilliday and Lautenschlager, 2012). For each event, the ICEWS dataset provides information on the date, source and target actors, and precise location (geospatial coordinates). It captures all types of interactions —whether cooperative or hostile— among sociopolitical entities, including individuals, groups, and nation-states. These events are categorized according to the Conflict and Mediation Event Observations (CAMEO) classification (Schrodt and Yilmaz, 2012). Each category is then assigned an intensity score ranging from -10 to $+10$, ranking events from the most hostile to the most cooperative.

We construct our panel dataset of events by selecting all hostile entries in the ICEWS dataset —i.e., those with negative intensity values— that are classified as violent according to Amodio et al. (2021).[11] This includes a broad spectrum of political violence and conflict episodes, even those that did not result in fatalities. Our final dataset comprises a total of 1.1 million political violence events occurring between 2006 and 2019. A detailed breakdown of event types and their frequency over the period of analysis is provided in Table A5, while Figure 2 displays their geographical distribution.

We exploit this data to construct our firm-specific measure of exposure to political violence, labeled *Conflict Exposure*. This variable is defined as the logarithm (log of 1+) of the number of political violence events that occurred within a 20 km radius of the firm's location during the 12 months preceding the end of the last fiscal year in each country. In absolute terms, the mean is 13, the standard deviation is 7.3, and the median and third quartile are 13 and 79, respectively.

As robustness checks, we construct alternative measures of conflict exposure: i) by using different radii for the buffer around each firm; ii) by exploring different types of conflict events; and iii) by employing alternative data from the PRIO/Uppsala Armed Conflict and Location Event Dataset (ACLED) (Raleigh et al., 2010). Like the ICEWS dataset, ACLED provides the date and geolocation of several types of conflict-related

[11] See Table A5 in the Online Appendix for details on this classification.

events.However, ACLED's country coverage during our period of analysis is significantly smaller.[12] The bottom panel of Table A5 reports the number of conflict events by type over our period of analysis.

Our measures of conflict exposure may be subject to measurement error, as the ICEWS dataset shares common limitations of large-scale datasets compiled from multiple sources. One such issue is the presence of duplicated events, which we address by excluding entries with identical characteristics (e.g., date, location, actors). Another potential source of measurement error is related to possible biases in event reporting, which may favor certain locations. To mitigate this concern, we account for potential (time-invariant) geographical differences in reporting by including firm and area fixed effects in our analysis. Additionally, we cross-validate ICEWS conflict event data with information from the ACLED dataset. The correlation between the number of conflict events reported by the two datasets is very high (0.77) within the same sample. Robustness checks further confirm that our main results hold when using ACLED as dataset for conflict events.

2.3 Additional data

In our heterogeneity analysis, we draw on a wide range of additional data sources to classify countries across several economic, institutional, and political dimensions.

Income Countries are grouped by their level of economic development, following the World Bank classification.[13]

Trade, agriculture, and natural resources shares in GDP Countries are classification based on three indicators from the World Bank: i) merchandise trade as a share of GDP, capturing countries' engagement in international trade; ii) the share of agriculture in GDP; and iii) the share of natural resources in GDP.[14]

[12] Until 2016, ACLED collected data only for African countries and a limited number of Asian countries. For details on the ACLED coverage see: https://acleddata.com/acleddatanew/wp-content/uploads/dlm_uploads/2019/07/Country-and-Time-Period-coverage_updOct2019.pdf.

[13] The World Bank classifies countries into four income categories: low, lower-middle, upper-middle, and high-income, based on per capita income. The full list of countries and their income categories is available at: https://datahelpdesk.worldbank.org/knowledgebase/articles/906519.

[14] Data for these variables are available at: https://data.worldbank.org/indicator/TG.VAL.TOTL.GD.ZS, https://data.worldbank.org/indicator/NV.AGR.TOTL.ZS, and https://data.worldbank.org/indicator/NY.GDP.TOTL.RT.ZS, respectively.

Fragility We use the World Bank classification to identify fragile countries —those experiencing deteriorating governance, prolonged political crises, post-conflict transitions, or gradual but still unstable reform processes.[15] In our sample, 16% of countries fall into this category.

Economic Complexity Countries are categorized according to the complexity of their production structure using the Economic Complexity Index (ECI) (Hidalgo and Hausmann, 2009).[16] The ECI has been shown to explain income differences across countries and to predict future growth (see, for instance, Hausmann et al., 2014).

Drug producing Major illicit drug-producing and/or drug-transit countries are identified based on the *2006 International Narcotics Control Strategy Report*, published by the U.S. Bureau for International Narcotics and Law Enforcement Affairs.[17]

Bureaucracy quality The quality of a country's bureaucracy is measured using data from the International Country Risk Guide (ICRG, Political Risk Services Group). This index captures the institutional strength and quality of the civil service, including its ability to function without political interference, ensure continuity in government policy, and effectively implement reforms.[18]

Corruption Country-level corruption is measured using an index from the ICRG that captures perceptions of corruption within the political system. This includes the misuse of public office for private gain through practices such as bribery, patronage, and nepotism.

Peace/conflict status Countries are grouped based on their peace or conflict status at the beginning and throughout the entire period of analysis. A country is classified as *initially at peace* if, at the time of the first survey in that country, there are no major episodes of political violence (such as civil conflicts, ethnic violence, riots, popular protests, or

[15]The list of fragile countries is available at: https://thedocs.worldbank.org/en/doc/9b8fbdb62f7183cef819729cc9073671-0090082022/original/FCSList-FY06toFY22.pdf.

[16]The index is computed by the Harvard Growth Lab and is available for 133 countries for the period 1995–2021. Data are available at: https://doi.org/10.7910/dvn/xtaqmc.

[17]The report is available at: https://2009-2017.state.gov/j/inl/rls/nrcrpt/2006/vol1/index.htm. Among the countries in our sample, the list includes: Afghanistan, Bolivia, Colombia, Dominican Republic, Ecuador, Guatemala, Haiti, Lao PDR, Mexico, Nigeria, Pakistan, Panama, Paraguay, Peru, and Venezuela.

[18]For details on the ICRG rating see https://www.prsgroup.com/wp-content/uploads/2014/08/icrgmethodology.pdf.

repression of dissidents).[19] Countries not meeting this criterion are classified as *initially in conflict*.[20] We also categorize countries based on their conflict status throughout the entire analysis period. A country is classified as *always in conflict* if major episodes of political violence begin before the first survey wave and persist throughout all subsequent waves. All other countries are classified as *not always in conflict*.[21]

3 Empirical Strategy

We estimate the impact of conflict exposure on firms' economic performance by combining longitudinal firm-level data with geolocalized information on conflict events. Our main specification reads as follows:

$$Y_{it} = \alpha + \beta \ \textit{Conflict exposure}_{it} + \gamma' X_{it} + \mu_i + \theta_t + \varepsilon_{it} \tag{1}$$

where Y_{it} represents the outcome of interest for firm i in fiscal year t. The analysis considers several firm-level outcomes as dependent variables, including *Sales*, *Total Costs*, and *Profits*. $\textit{Conflict exposure}_{it}$ denotes the firm-specific measure of conflict exposure. In our baseline specification, it is computed as the logarithm of the number of conflict events that occurred within a 20 km radius of the firm's location during the 12 months preceding the end of the last fiscal year.[22] The vector X_{it} represents a set of time-varying firm characteristics, including size, age, and export status. μ_i denotes firm fixed effects, accounting

[19] The list of countries with major episodes of political violence from 1945 to 2019 is sourced from the Systemic Peace War dataset available at: https://www.systemicpeace.org/warlist/warlist.htm. Major episodes involve at least 500 "directly related" fatalities and reach a level of intensity where the violence is systematic and sustained (with a base rate of 100 "directly related" deaths per year). Only countries directly affected by the violence are listed, i.e. those in which the events have occurred. We expand this list to include countries involved in Operation Juniper Shield, a large counter-terrorism operation initiated in 2007 against armed groups in the Saharan and Sahel regions of Africa. Countries involved in this operation include Algeria, Burkina Faso, Cameroon, Chad, Mali, Mauritania, Morocco, Niger, Nigeria, Senegal, and Tunisia (OECD/SWAC, 2020).

[20] The list of countries *initially in conflict* includes: Afghanistan, Cameroon, Chad, Colombia, Democratic Republic of the Congo, Egypt, Ethiopia, Kenya, Morocco, Myanmar, Niger, Nigeria, Pakistan, Philippines, Russia, Tunisia, Turkey, Uganda, West Bank and Gaza, and Yemen.

[21] The list of countries *always in conflict* includes all countries *initially in conflict* except for Kenya and Uganda.

[22] We also employ alternative measures of conflict exposure constructed using: i) different buffer distances from the firm's location; ii) a different time period; iii) different types of conflict events; and iv) the ACLED dataset as an alternative source for conflict events (see Section 2.2).

for all time-invariant observed and unobserved characteristics of the firm that could potentially influence the outcome of interest. θ_t represents year fixed effects, controlling for overall trends in economic activity and common shocks. As a robustness check, we also include the full set of area-year interacted fixed effects to account for all time-varying location-specific determinants of firm performance, such as infrastructure availability and local labor market characteristics. Finally, ε_{it} is the error term. In all specifications, we report robust standard errors clustered at the firm level, which is the unit of observation used to measure the intensity of conflict exposure. All variables are defined in Table C1.

Our identification strategy relies on the assumption that, conditional on our full set of fixed effects, conflict events occurring near the firm's location are uncorrelated with any latent determinant of its economic performance. Under this assumption, β captures the reduced-form effect of the firm-specific conflict exposure on economic performance.[23] In what follows, we present our core results and examine in detail potential threats to the validity of our identification strategy.

4 Results

4.1 Conflict exposure, sales, total costs, and profits

Table 2 presents our main results. Column 1 shows that conflict exposure leads to a large and statistically significant decline in firm sales. This effect remains robust in column 2 after the inclusion of firm-level controls. Based on our estimates, a one-standard-deviation increase in the number of conflict events occurring within a 20 km radius of the firm during the last fiscal year (eight events) leads to a 2.8% reduction in median sales, equivalent to 10,050 USD. Higher conflict exposure also leads to a significant decline in total production costs, measured by expenditures on raw materials, intermediate inputs, and labor (column 3). Results do not change including controls (column 4).

These reductions in both sales and costs could reflect changes in either prices or quantities. Although the WBES dataset does not allow us to directly test for price changes, it

[23] In our sample, the onset of conflict does not necessarily occur at t_0. As a result, we cannot provide dynamic difference-in-differences estimates of the effect of conflict between t_{-1} and t_0.

is unlikely that conflict lowers consumer or input prices.[24] This suggests that the observed declines in sales and total production costs are mainly driven by a contraction in output and a reduction in the quantity of inputs and materials used in production.

Finally, our results show that conflict exposure does not affect a firm's profits (columns 5 and 6).[25] This null effect is consistent with the fact that higher conflict exposure affects both sales and total costs in the same direction, suggesting that firms respond to reduced sales by cutting production costs.[26] As these adjustments result in reduced output, it follows that, while surviving firms' profitability remains unchanged, overall economic activity is negatively impacted by conflict-related violent events.

Threats to identification The firm-level fixed effects in our baseline specification account for all time-invariant firm, sector, and geographical characteristics. However, our estimates may still capture some factors that vary over time at the local level and are simultaneously correlated with the number of conflict events as well as the firm's economic performance. To address this concern, we augment model 1 with a full set of area-time interacted fixed effects to also control for all time-varying, location-specific determinants of firm performance, such as infrastructure availability and local labor market characteristics.[27] Table A6, columns 1 and 2, shows that the coefficient for *Sales* remains negative, sizable, and significant (though somewhat smaller than in the baseline), while the coefficient for *Profits* stays null.

Another potential threat to our identification is reverse causality. To address this, we start including the lead number of conflict events in our baseline regression for both *Sales* and *Profits* (see Table A6, columns 3-4, respectively). Reassuringly, for both outcomes,

[24] Previous studies find that conflict typically raises consumer and input prices (Amodio and Di Maio, 2018; Utar, 2024; Del Prete et al., 2023; Couttenier et al., 2022).

[25] Due to the coverage of the WBES, the null effect on profits applies only to surviving firms that remain formal —that is, those observable in subsequent survey waves. However, not all firms exiting the sample necessarily go bankrupt. As discussed in Section 4.2.3, conflict may also induce firms to shift to the informal sector, causing them to disappear from our sample (which includes only formal firms) while continuing to operate in the economy.

[26] While the average effect of conflict exposure on profits is null, in Section 4.3, we discuss the conditions under which profits decrease with conflict exposure.

[27] *Area* is the most granular geographical unit of observation in the WBES and represents the sampling region. There are 314 areas, and they are typically larger than the 20 km radius buffer used to construct our conflict exposure measure.

the results are consistent with the baseline, and the coefficients for the lead variables are centered around zero. This pattern indicates that future changes in conflict are not driven by current firm performance, which helps rule out reverse causality and reassures about parallel trends in firm outcomes. A related concern is that the negative relationship between sales and conflict might be due to low-performing firms being targeted because they cannot afford protection. However, this does not seem to be the case: as shown in Table A21 (columns 5-7), conflict exposure does not increase firms' losses from theft or their security expenditures.

We also consider the possibility that poor local economic conditions may themselves trigger violent events. To address this concern, we conduct two additional tests. Table A7 shows that local conflict intensity —defined at the second-level administrative unit (i.e., GID2)— is not predicted by the level of economic activity. Specifically, we find no correlation between conflict intensity in a given area and various proxies for economic activity measured in the preceding period —namely, total sales, total employment, total profits, sales growth, and employment growth (columns 1–5, respectively). This result holds whether conflict intensity is measured in the following year, over the next two years, or over the next three years (reported in panels A, B, and C, respectively). At the same time, results in Table A8 rule out the possibility that conflict intensity is driven by the local composition of economic activity. There is no significant correlation between the economic size of any sector and subsequent conflict intensity in the same area —whether measured one, two, or three years later (columns 1-3).

Another concern is that the location and intensity of conflict events may instead be driven by the presence of firms with better economic performance, making them valuable targets for conflict fighters. As an additional indirect test for reverse causality, we re-estimate our model by excluding large firms, those with high sales and profits, and those located in capital cities. The results, reported in Table A9, are virtually identical to those obtained using the full sample. This suggests that our findings are not driven by the location and intensity of conflict events being influenced by the presence of high-value firms or by the activity of firms located in political centers.

Finally, we address the concern that firms may be nonrandomly exposed to conflict events by implementing a test in the spirit of Borusyak and Hull (2023). We construct two different placebo controls designed to account for the possibility that firms are exposed to conflict in a systematic, predictable way. In the first exercise, we hold the firm's location fixed but randomize the timing of conflict events by assigning each firm its actual conflict exposure from previous years ($t-2$ to $t-5$), drawn from a uniform distribution. This placebo measure is then included as an additional control in the baseline sales regression to capture any predictable component of conflict exposure driven by past patterns. In the second exercise, we construct a placebo measure based on the number of conflict events affecting other firms operating in the same year and geographical area. In both cases, we repeat the randomization process 5,000 times and estimate the marginal effect of actual conflict exposure, controlling for the placebo measure. Figure A1 shows that the distribution of our estimates remains centered around our main coefficient, suggesting that the results are not driven by nonrandom assignment of conflict exposure.

Conflict exposure is heterogeneous across firms and varies over time. In a two-way fixed-effects model, this type of treatment may lead to the negative weighting problem (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). To check whether this is an issue in our case, we perform a clean control analysis through sample restriction in the spirit of De Chaisemartin et al. (2022). Table A10 presents the results. As a preliminary step for this analysis, in column 1, we replicate Table 2 using a dummy variable as an alternative measure of conflict. The indicator $\mathbb{I}(\text{Conflict exposure} \geq \text{p}(25))$ equals one if the firm is exposed to a number of conflict events above the 25th percentile of the country-year distribution, and zero otherwise. The results indicate that the estimated effect is negative and highly significant. Once confirmed that our results continue to hold using a dummy treatment variable, in columns 2 and 3, we refine the analysis using a "clean" sample of movers and stayers. In the spirit of the methodology proposed by De Chaisemartin et al. (2022), we classify movers as treated units and quasi-stayers as controls. In this framework, we define movers as firms with substantial treatment intensity —those at or above the 25th percentile of the treatment distribution— and quasi-stayers

as firms with negligible exposure, falling below that threshold. In each survey wave, we exclude firms that had already been significantly exposed to conflict. This allows us to compare outcomes between firms experiencing conflict for the first time and those that remain untreated or have not yet been treated. To further refine the sample, we exclude firms with significant conflict exposure in the two years prior to the survey wave. Results in Table A10, columns 2 and 3, show that our findings remain robust across all specifications in this clean controls analysis, using the same dummy treatment variable as in column 1: conflict exposure reduces sales and has no effect on profits. Taken together, this analysis suggests that the observed effects are not driven by heterogeneous treatment responses within our empirical framework.

To support our interpretation of the main results, we also conduct a formal check for selection on unobservables using the test proposed by Altonji et al. (2005) and further refined by Oster (2019). Table A11 presents the estimated impact of conflict exposure under varying assumptions about the relative importance of unobservables, captured by the parameter $0 < \delta \leq 1$.[28] Column 1 reports the baseline effects of conflict exposure on sales and total costs (corresponding to columns 4 and 6 in Table 2, respectively). Columns 2–11 show that the estimated coefficient on conflict exposure remains negative across all values of $\delta \in (0, 1)$. Column 12 reports the hypothetical value of $\hat{\delta}$ required to drive the estimated effect to zero. In both cases, $\hat{\delta}$ is negative and large in absolute value, implying that selection on unobservables would need to be substantial and of the opposite sign to the selection on the observables for the effect to be nullified by the bias. These results suggest that our main findings are unlikely to be driven by selection bias from unobserved factors.

One final potential concern with our results is a possible violation of the stable unit treatment value assumption (SUTVA), which requires that a firm's response depends solely on its own level of conflict exposure, and not on the exposure of other firms. In our setting, the estimated effect of conflict exposure on firm i could be biased if firms in other areas —linked to firm i through supplier or buyer relationships— are also affected

[28] When $\delta = 1$, the selection on unobservables is assumed to be as important as selection on observables.

by conflict events. In such cases, conflict would exert both a direct effect —through firm i's own exposure— and an indirect effect —through network linkages. To address this concern, we check whether our results change when accounting for production and consumption network spillovers in the regression. Ideally, this analysis would require firm-to-firm transaction data for each country. However, such data are not available for the vast majority of countries in our sample. As an alternative, we rely on OECD input-output tables by country and use the WBES identification of the main product sold (ISIC 4D) to construct a product–country–specific matrix of input-output relationships.[29]

For each firm in our sample, we construct two weighting matrices: one reflecting dependence on input purchases and another on output sales. By interacting these matrices with the level of conflict exposure experienced by firms in the supplier and buyer networks, we test whether a firm —beyond being directly affected by conflict— is also indirectly impacted through its production linkages. Table A12 replicates our main results while accounting for potential spatial spillovers of conflict transmitted through the production network within each country. Across all outcomes, the indirect effect of conflict is not statistically significant, while the direct effect remains consistent with our baseline findings: negative and significant for sales and total costs, and insignificant for profits.[30] While these results should be interpreted with caution due to limitations in the construction of the network measures, they suggest that our main estimates are unlikely to be substantially biased by spillover effects through supplier and buyer linkages.

[29]The OECD does not provide input-output matrices for several countries, including Afghanistan, Angola, Albania, Armenia, Azerbaijan, Benin, Bosnia and Herzegovina, Bolivia, Bhutan, Botswana, Democratic Republic of the Congo, Dominican Republic, Ecuador, Ethiopia, Georgia, Ghana, Guatemala, Honduras, Kenya, Kyrgyzstan, Lebanon, Liberia, Lesotho, Moldova, North Macedonia, Mali, Montenegro, Mongolia, Malawi, Niger, Nicaragua, Nepal, Panama, Paraguay, Palestine, Rwanda, Sierra Leone, El Salvador, Serbia, Suriname, Chad, Togo, Tajikistan, Timor-Leste, Tanzania, Uganda, Uruguay, Uzbekistan, Venezuela, Kosovo, Yemen, Zambia, and Zimbabwe. Therefore, data are unavailable for 53 countries in our sample of 89. For these countries, we substitute missing matrices with an average of input-output matrices from available OECD countries within the same region (Africa; East Asia and Pacific; Europe and Central Asia; Latin America and the Caribbean; Middle East and North Africa; South Asia). Moreover, to ensure consistency with available classifications, input-output tables and the WBES product classification have been aggregated into 15 sectors. To avoid possible endogeneity related to sector reliance potentially driven by conflict exposure, we set the input-output table to reflect data before the beginning of our sample period (year 2000).

[30]The negative (though not significant) effect of suppliers' and buyers' conflict exposure on sales is in line with previous studies on conflict and production networks (Couttenier et al., 2022; Korovkin and Makarin, 2023; Korovkin et al., 2024).

Robustness Our results are robust to a number of checks. First, we examine the estimation sample. As discussed in Section 2.1, the number of observations varies across outcomes due to missing values. All results presented thus far are based on the maximum number of available observations for each outcome in each regression. Table A13 confirms that the results for our main dependent variables remain consistent with the baseline when we restrict the sample to firms with data available for all outcomes. As an additional check, we re-run the main analysis focusing only on manufacturing firms. This ensures that our results are not driven by differences in the construction of *Total costs* across sectors (see Section 2.1). Table A14 shows that the results remain unchanged. Next, we assess the robustness of our findings to the countries included in the analysis. We re-run our baseline regression 89 times, sequentially excluding one country at a time from the sample. Figure A2 shows that the negative effects on sales and total costs, and the null effect on profits, do not depend on the inclusion or exclusion of any specific country.

Our results are also robust to different definitions of the conflict exposure measure. Table A15 shows that the effects of conflict on our main outcomes remain qualitatively unchanged when conflict exposure is measured considering events that occurred: i) within a smaller buffer around the firm (5 and 10 km radius in columns 1-2); ii) within a larger buffer (40 km radius, column 3); and iii) within the 20 km radius over the past two years (column 4). Additionally, we examine how our findings depend on the types of events included in the conflict exposure measure, as defined by Amodio et al. (2021) (see Table A5). Table A16, column 1, shows the results when we exclude events that may be less likely to have a direct economic impact on the firm.[31] Column 2 reports results excluding events related to protests and riots.[32] The next two columns show the results when we apply a *quantitative* rule for event selection, considering only very violent events, i.e. with intensity scores between -8 and -10 (column 3), or only events with the maximum violence level, i.e. score -10 (column 4), regardless of whether they appear in the list from

[31] The excluded events belong to the categories: i) sexual assault; ii) mobilize or increase police power; iii) mobilize or increase armed forces; iv) demonstrate military or police power; v) torture; vi) expel or withdraw; and vii) expel or withdraw peacekeepers.

[32] The excluded events belong to the categories: i) protest violently, riot; and ii) engage in violent protest for leadership change.

Amodio et al. (2021). Finally, column 5 confirms that the results for both sales and total costs remain consistent when using the ACLED dataset to measure conflict exposure, with the effect on profits being negative and marginally significant. These findings are reassuring given that ACLED covers a very different set of countries and conflict-related events compared to ICEWS. While all our results are consistent across these alternative definitions of conflict exposure, they also suggest that firms respond differently depending on the type of event, with the negative effects of conflict being more pronounced when the events are more violent.

Finally, Table A17 shows that our results remain unchanged when accounting for spatial correlation in the error terms, using the arbitrary clustering correction for standard errors proposed by Colella et al. (2023) to re-estimate our baseline model.

4.2 Mechanisms

Our results show that conflict exposure reduces a firm's sales and total costs. These effects are likely driven by a combination of production-side and market-level channels. One mechanism relates to the availability of production inputs. Higher conflict exposure may constrain access to domestic and foreign inputs —such as raw materials, intermediate goods, labor, or electricity— forcing firms to adopt a sub-optimal input mix. This reduction in input quantity and quality lowers overall output and, in turn, depresses sales. At the same time, conflict may reduce sales by changing the level and type of competition faced by firms, such as increasing competition from informal firms that are less regulated and more adaptable in unstable environments. Finally, conflict may lower sales by decreasing demand or increasing the severity of the obstacles to a firm's activity. In what follows, we take these hypotheses to the data to examine the mechanisms driving the negative effect of conflict on firms' sales and production input expenditures.

4.2.1 Raw materials and intermediate inputs

Table 3, column 1, shows that conflict exposure leads to a decline in firms' expenditures on raw materials and intermediate inputs. This reduction could reflect either lower input

prices or a decrease in the quantity of inputs purchased. As the WBES does not report prices and quantities separately, we cannot test this directly. However, given that conflict is unlikely to lower the unit cost of inputs, we read these findings as evidence of a conflict-induced reduction in the quantity of raw materials and intermediate inputs used in production.[33] Consistent with this interpretation, conflict also appears to reduce electricity use, another key production input. Firms more exposed to conflict events report lower expenditures on electricity (column 2) and a higher likelihood of experiencing power outages (column 3), suggesting that conflict constrains the availability of electricity and forces firms to cut its usage. Furthermore, conflict decreases the share of imported intermediate inputs used in production (column 4) and, for the same argument above, likely reduces their quantities as well. Importantly, the results in Table A18 suggest that the decline is driven by reduced availability of imported inputs, rather than by a decrease in firms' demand. Conflict appears to increase the difficulty of importing, thereby limiting access to foreign production inputs. Column 1 shows that, among firms using imported inputs, those more exposed to conflict are more likely to report customs procedures as a significant obstacle to their operations. Column 2 further indicates that higher conflict exposure is associated with a longer number of days required to clear customs for imported goods. These findings align with previous single-country studies indicating that conflict hampers firms' access to imported inputs due to increased trade costs or uncertainty.[34] Given that imported inputs are crucial for firm productivity (see, e.g., Topalova and Khandelwal, 2011; Halpern et al., 2015), a negative shock to their availability likely leads to a significant reduction in firms' economic activity (Amodio and Di Maio, 2018; Boehm et al., 2019; Carvalho et al., 2021). Taken together, these findings suggest that conflict reduces the availability of both raw materials and intermediate inputs, especially imported ones, resulting in a decline in the firm's overall output, and consequently, sales.

[33] This is line with evidence from previous studies for West Bank, India, Libya, and Ukraine (Amodio and Di Maio, 2018; Couttenier et al., 2022; Del Prete et al., 2023; Korovkin et al., 2024).

[34] Firms exporting to firms in conflict-affected countries may react to higher local violence by decreasing volume or raising prices to cover export risk (Amodio and Di Maio, 2018; Korovkin et al., 2024).

4.2.2 Labor

Turning to the labor channel, Table 3, column 5, shows that conflict exposure leads to a decline in total labor costs. To shed light on the mechanisms behind this effect, columns 6-8 explore changes in employment levels, workforce composition, and average wages. Conflict exposure does not appear to affect the total number of workers employed by the average firm (column 6).[35] However, conflict does have a significant impact on the composition of the workforce, resulting in a sizable increase in the share of unskilled workers (column 7). Consistent with this substitution effect, higher conflict exposure also leads to a reduction in the average wage (column 8), which helps explain the observed decrease in total labor costs.[36]

Table A19 presents additional results for the labor mechanism. Column 1 shows that the null effect of conflict exposure on employment masks significant heterogeneity. Specifically, higher conflict exposure reduces employment for smaller firms, with this effect diminishing with firm size. We also provide suggestive evidence supporting our interpretation that total labor costs fall due to a substitution of skilled workers with unskilled ones. As shown in column 2, this substitution effect is more pronounced among firms producing non-differentiated products, which are typically more reliant on cost efficiency and less dependent on specialized labor.[37] This is expected, as producing differentiated goods relies more heavily on higher-skilled labor, limiting firms' ability to substitute toward lower-skilled workers. Additionally, column 3 indicates that the magnitude of the substitution effect varies with a country's labor legislation. In contexts where firing costs are higher (proxied by severance pay levels), the substitution of skilled workers for un-

[35] The number of workers includes both full-time permanent employees and temporary workers expressed in full-time equivalents. Temporary workers are defined as "full-time seasonal or temporary employees," referring to individuals employed for short periods (i.e., less than a year), without a guarantee of contract renewal, and working full-time hours. The variable *Number of workers* is computed by adjusting temporary workers according to their actual months of employment.

[36] Individual wage data at the firm level is not reported by the WBES. We compute the average wage as the ratio of the total labor cost to the total number of employees (full-time equivalents).

[37] Drawing from the Rauch (1999) classification, we define a product as differentiated if its trade neither occurs on an organized exchange nor relies on a reference price. As Rauch and Trindade (2002) highlights, goods with a reference price are sufficiently homogeneous, as prices convey all relevant information for international trade; this is not the case for more differentiated goods lacking reference prices. The original classification is available at: `https://econweb.ucsd.edu/~jrauch/rauch_classification.html`.

skilled ones due to conflict exposure is smaller.[38] These results align with the idea that worker substitution is a defensive strategy employed by firms to reduce labor costs when affected by conflict: when sales decrease due to lower output from reduced availability of production inputs, firms adjust their workforce to lower costs.

The remaining columns of Table A19 help rule out alternative explanations for the observed reduction in total labor costs. Column 4 shows that the decline is not driven by an increased reliance on temporary workers, suggesting that changes in contract structure are not a key mechanism. Column 5 further indicates that the substitution away from skilled labor is not due to supply-side constraints —such as a conflict-induced decline in the availability of skilled workers. Specifically, firms do not report a worsening of the skilled labor supply as a more serious obstacle to their operations. Taken together, these results provide suggestive evidence that the substitution of skilled with unskilled workers is driven primarily by demand-side adjustments —namely, firms responding to conflict-induced reductions in their production possibilities.

4.2.3 Market competition

An important determinant of firm sales is the level of market competition. In Table 3, columns 9–11, we examine how conflict affects competitive pressure. Column 9 shows that increased conflict intensity does not change the total number of competitors reported by the firm.[39] However, conflict does significantly affect the nature of competition, particularly with respect to informality.[40] Column 10 shows that conflict exposure increases the likelihood that firms report informal competition as an obstacle. Column 11 complements this result by showing that conflict also heightens the perceived intensity of the threat posed by informal competitors.[41]

[38] Data on severance pay for redundancy dismissal (in weeks of salary) are from the ILO database or the World Bank Employing Workers project dataset: https://www.worldbank.org/en/research/employing-workers/data/redundancy-cost. We define a country to have low firing costs if its average severance pay is below the cross-sectional median of the distribution.

[39] This refers to the self-reported number of competitors for the firm's main product in its main market, categorized in quartiles. For details, see Table C1.

[40] A large informal sector is a common feature of developing economies, where formal and informal firms often operate in the same sectors and produce similar goods (Ulyssea, 2020).

[41] In column 10, the outcome is a binary variable equal to 1 if the firm reports informal competition as a (major or very severe) obstacle, and 0 otherwise. In column 11, the outcome is a continuous measure

These results indicate that while conflict does not affect the total number of competitors, it increases informal competition.[42] This pattern aligns with prior studies showing that informality tends to rise in response to negative shocks, as firms seek to exploit the cost advantages of non-compliance with formal regulations (Ulyssea, 2018). This is also the case for an increase in conflict exposure. For instance, Neri-Lainé (2024) documents that —in the context of Afghanistan— an increase in insecurity reduces formal firms' existence, increasing informality. At the same time, evidence shows that competition of informal firms can reduce the output of formal firms (Rozo and Winkler, 2021), lower their employment (Amin, 2023), weaken innovation incentives (Avenyo et al., 2021), and dampen demand for credit, thereby curbing investment (Brancati et al., 2024). More broadly, the expansion of informality poses challenges for the functioning of the overall economy. It contributes to resource misallocation, reduces total factor productivity, and is often associated with broader welfare losses (Ulyssea, 2018). Taken together, this evidence suggests that the conflict-induced increase in informal competition contributes to explaining the reduction in firms' sales documented in Table 2.

4.2.4 Demand

An alternative explanation for the observed reduction in sales and total costs is that conflict reduces consumer demand. In this case, firms would respond to falling consumer expenditure by scaling back production and, consequently, reducing input purchases —leading to lower sales. While we cannot directly test this channel due to the lack of consumer demand data in the WBES, Table A20 provides suggestive evidence that this mechanism alone is unlikely to fully account for our results.

Column 1 examines whether the effect of conflict exposure on sales varies by the firm's main destination market —local, national, or international. If the decline in sales were driven solely by a conflict-induced drop in domestic demand, we would expect firms

of the self-reported intensity of the obstacle posed by informal competitors. See Table C1 for details.

[42] This finding may reflect two dynamics: previously formal firms shifting into the informal sector, or the entry of new informal firms. Since the WBES includes only formal firms, we cannot directly observe these transitions. However, because conflict is associated with higher exit rates among formal firms (see Table A4), and the overall level of competitive pressure remains unchanged, our evidence is more consistent with the first explanation.

selling primarily to international markets to be less affected. However, this is not what we find: the effect of conflict exposure on sales does not differ significantly across the three market categories. This suggests that the negative impact on sales is not confined to firms serving domestic customers, making it unlikely that a reduction in consumer demand alone explains our results.

Next, we explicitly examine heterogeneities based on firms' export status. Results in column 2 show that the negative impact of conflict on sales is smaller for exporting firms, and column 3 indicates that this mitigating effect increases with the export share in total sales. However, even for firms that export 100% of their output, the effect remains economically and statistically significant. Our estimates suggest that a one-standard-deviation increase in nearby conflict events reduces median sales by approximately 2.7% for a firm exporting *all* of its output. This implies that even firms with no domestic customer base are substantially affected by conflict occurring close to their location.[43]

Finally, we analyze the effect of conflict on capacity utilization to further assess whether changes in demand could explain our findings. Capacity utilization is defined as the firm's actual output expressed as a percentage of its maximum potential output using all available resources, and it provides a useful proxy for identifying demand-driven fluctuations in production. Since a firm's production capacity is directly tied to input availability, a drop in capacity utilization would suggest that firms are producing less in response to weaker demand. However, the results in Table A20, column 4, show that capacity utilization is not significantly affected by conflict exposure. This suggests that lower sales are unlikely to be the results of a limited use of installed production capacity, which would be the case if the firm is responding to a conflict induced reduction in demand.[44] Taken together, these results indicate that while demand-side factors may contribute in some cases, they are unlikely to be the main driver of the observed decline in firm performance.

[43] While it is possible that foreign buyers reallocate their sourcing away from firms located in conflict-affected countries (Korn and Stemmler, 2025), the finding that the negative effect on sales diminishes with the export share suggests that this type of substitution likely plays only a limited role in explaining the overall decline in sales among exporting firms.

[44] While it is theoretically possible that firms fully anticipate conflict-induced demand shocks and adjust input purchases accordingly, our findings —particularly the sharp (forced) decline in imported input use and electricity consumption (see Section 4.2.1)— suggest that conflict directly restricts firms' ability to procure essential inputs.

4.2.5 Other obstacles to firms' activity

Next, we explore whether the decline in firms' sales could be explained by a conflict-induced worsening of various obstacles to business activity. Table A21 provides little support for this explanation. Columns 1-4 report results for several potential barriers, including access to finance, corruption, transportation, and land availability. None of these are more likely to be reported as severe obstacles by firms more exposed to conflict, suggesting they do not significantly contribute to the observed decline in performance. Similarly, columns 5-7 show that the reduction in sales is not driven by conflict-induced increased exposure to crime (measured by losses due to theft), the decision to invest in security, or higher spending on protective measures. Overall, these results indicate that broader business environment constraints are unlikely to be the main channel through which conflict affects firm outcomes.

Summing up Taken together, our results suggest that conflict restricts firms' access to key intermediate inputs —particularly imported goods and electricity— leading to a reduction in output. Conflict also increases the pressure from informal competition. In response, firms adjust by cutting production costs, including substituting skilled labor with unskilled workers. As a result, sales decline due to both lower output and heightened informal competition, while profits remain stable thanks to firms' cost-cutting adjustments.

4.3 Heterogeneity

In this section, we test the heterogeneous effect of conflict exposure on our main outcomes across several firm-level and country-level characteristics.

Firm-level characteristics Table A22 presents heterogeneity tests based on several firm characteristics, including size, age, government ownership, and foreign ownership. The results show no significant differences in the effect of conflict exposure on sales or profits across these characteristics, except for firm size. The negative impact on sales is more pronounced for smaller firms and gradually diminishes as firm size increases. This pattern

is consistent with the evidence in Table A19, column 1, which shows that conflict has a stronger adverse effect on employment among small firms, leading to reduced output and sales. Overall, our findings suggest that larger firms are more resilient to conflict, likely due to greater resources or operational flexibility, while other firm-level attributes play a limited role in mediating the effects of conflict.

We also examine whether the effect of conflict exposure varies across sectors. Table A23 shows that the impact on sales is negative across all sectors, with the exception of *Other manufacturing* and *Other services*, where the effect is statistically insignificant. Conflict exposure significantly reduces profits in only two sectors —*Furniture* and *Transportation*— where the negative effect is also the largest among all industries.[45]

Country-level characteristics Figure 3 presents heterogeneity results for the effect of conflict exposure across a range of country-level characteristics.[46] The figure displays the estimated coefficients and 95% confidence intervals for the interaction terms between conflict exposure and each country characteristic. Results are reported separately for both sales and profits.

We begin by examining the country-level characteristics that do not appear to shape the effect of conflict exposure on firms. As shown in Figure 3, being located in a high-income country does not, in itself, mitigate the negative effect of conflict on firm outcomes. Likewise, firms in fragile countries are not more vulnerable to conflict events. This suggests that, although the fragility index is often used to assess the risk of conflict (see, for example, Chami et al., 2021; Verwimp et al., 2019), it is not a strong predictor of the severity of conflict's effects on firms in a given country. Similarly, the adverse effects of conflict do not differ between countries with high dependence on agriculture or natural resources. This is consistent with the idea that the mechanisms identified in Section 4.2 operate largely independently of a country's sectoral specialization.

[45] This analysis is constrained by the level of sectoral detail available in the WBES. The sector classifications do not allow us to isolate firms that might benefit from conflict —such as those involved in arms production, supplying intermediates to the defense industry, offering security services, or natural resources and precious materials (e.g., Guidolin and La Ferrara, 2007). These firms are grouped with others less likely to benefit from conflict, potentially masking meaningful variation across more narrowly defined activities.

[46] Definitions for all variables used in this analysis are provided in Section 2.3 and Table C1.

Figure 3 also highlights several country-level characteristics that significantly influence the effect of conflict exposure on firm performance. First, the negative effect of conflict is more pronounced in countries with greater exposure to international trade. Two complementary mechanisms help explain this result. As discussed in Section 4.2.1, conflict disrupts access to imported inputs. Firms in countries that rely heavily on foreign inputs are therefore more exposed to the conflict-induced breakdown of international supply chains (Del Prete et al., 2023). In addition, as shown in Section 4.2.4, the export activity of firms in conflict-affected areas declines with conflict intensity (Chauvin and Rohner, 2009). Since firms in more trade-open countries often depend heavily on foreign demand, they are particularly vulnerable to conflict-related disruptions in export markets.[47] In sum, greater dependence on international trade —on both the supply and demand sides— amplifies firms' vulnerability to conflict.

Second, the effect of conflict is also more severe in countries with low economic complexity. Because economic complexity reflects the number and strength of domestic production linkages (Hausmann and Hidalgo, 2011; Gloria et al., 2024), this result suggests that firms operating in less complex economies face greater difficulty in substituting disrupted inputs or reorganizing supply chains in response to conflict. As a result, they experience sharper declines in output and performance.

The quality of institutions also plays a critical role in shaping how firms are affected by conflict. We find that conflict exposure has a more detrimental impact in countries with low-quality bureaucracy. This supports the idea that, *ceteris paribus*, more efficient governments are better positioned to help firms absorb and respond to negative shocks. Similarly, firms in countries with higher levels of corruption are more negatively impacted by conflict events. As discussed in Section 4.2.1, conflict reduces access to essential inputs and services, such as imported goods and electricity. In settings where access to these resources is already mediated by corruption, the disruption caused by conflict is likely to be magnified.[48] The presence of organized criminal activity further compounds these

[47] In some contexts, foreign demand may substitute for weakened domestic demand, as observed during the Mexican Drug War (Utar, 2024).

[48] Fisman et al. (2024) argue that in highly corrupt environments, officials have greater discretionary power over which firms gain access to key inputs and services.

effects. Our results show that firms located in countries involved in the production and trafficking of illegal drugs are more adversely affected by conflict and political violence, suggesting that the presence of criminal networks can amplify the economic consequences of such events.

Finally, we find that the impact of conflict events depends on the peace vs conflict status of the country at both the beginning and throughout the period of analysis. For firms in countries that are initially at peace, increased conflict exposure leads to a significantly larger decline in sales. The effect on profits is also substantial: a one-standard-deviation increase in nearby political violence events reduces median profits by approximately 3.9%. We find similar results when comparing firms in countries that experienced continuous conflict throughout the entire period to those in countries where conflict was not consistently present.[49] Taken together, these findings suggest that the cross-country heterogeneity in the effects of conflict exposure likely reflects differences in firms' adjustment costs across varying institutional and economic environments. Firms in countries already experiencing conflict —or consistently exposed to it— are more likely to have already adapted to operating in a conflict-ridden environment. By contrast, firms in countries with no prior exposure may be less prepared and, therefore, more vulnerable to a sudden increase in conflict intensity.

Country-level characteristics also shape the relative importance of the mechanisms through which conflict affects firm performance. To illustrate this, we examine how these mechanisms differ depending on a country's conflict status at the beginning of the period. Table 4 presents the results. Columns 1-4 show that conflict exposure reduces spending on intermediate inputs and electricity, and increases the likelihood of power outages in both groups. However, with the exception of the imported input share, these effects are consistently more severe in countries initially at peace. Columns 5-8 turn to labor market responses. In countries initially at peace, firms tend to substitute skilled workers

[49] In practice, we compare firms in countries that experienced periods of peace or intermittent conflict with those in countries affected by continuous conflict. As noted by Custodio et al. (2025), it is not obvious, *ex ante*, how the impact of additional conflict events might vary depending on the frequency of past conflict and expectations about future instability. In persistently violent contexts, firms may adapt by developing strategies to cope with conflict, potentially softening its impact. Alternatively, prolonged exposure could compound the negative effects of new conflict events, leading to even greater harm.

with unskilled labor, leading to a decline in average wages and overall labor costs. On the other hand, in countries initially in conflict, an increase in conflict exposure leads to a reduction in the number of workers. Finally, columns 9-11 examine the impact on market competition. While conflict intensity does not appear to affect the total number of competitors (column 9), it increases both the probability of facing informal competitors and the perceived threat posed by such competition (columns 10-11) in countries initially at peace. These differences in underlying mechanisms are consistent with the sharper declines in sales and profits observed among firms in countries initially at peace (see Figure 3), reinforcing the idea that the effects of conflict exposure —and the channels through which they operate— vary significantly with broader country characteristics.

5 Conclusions

Our analysis, covering 89 developing and developed countries from 2006 to 2019, provides a unique perspective on the effects of conflict on firm activity worldwide.

We document several empirical facts. We show that higher exposure to conflict reduces both firm sales and production costs. Conflict disrupts access to both domestic and imported intermediate inputs and raw materials, forcing firms to scale down production and resulting in a decline in sales. Sales are further depressed by intensified competition from informal firms. While lower output leads to a (mechanical) reduction in production costs, firms also respond by substituting skilled workers with unskilled labor to cut costs. As a result of these adjustments in both sales and costs, the average effect of conflict exposure on profits is null.

Our analysis also highlights significant cross-country heterogeneity. The impact of conflict exposure is more severe for firms located in countries with high trade openness, low economic complexity, weak bureaucratic quality, illicit drug production, and that are initially at peace. In these countries, conflict exposure leads to substantial declines in both sales and profits. Finally, we also show that the relative importance of the various mechanisms through which conflict affects firm performance varies with the country's characteristics.

Two key policy implications emerge from our analysis. First, the quality of government institutions is key in helping firms cope with the adverse effects of conflict. These findings highlight the importance of investing in state capacity and bureaucracy capabilities, particularly in contexts characterized by high conflict risk. Second, while interventions to maintain peace may be costly and complex, they provide sizable economic dividends. Our results show that the cost of political violence events is larger for firms in countries that are initially at peace, highlighting the economic value of policy measures to prevent the onset or escalation of conflict within and across countries.

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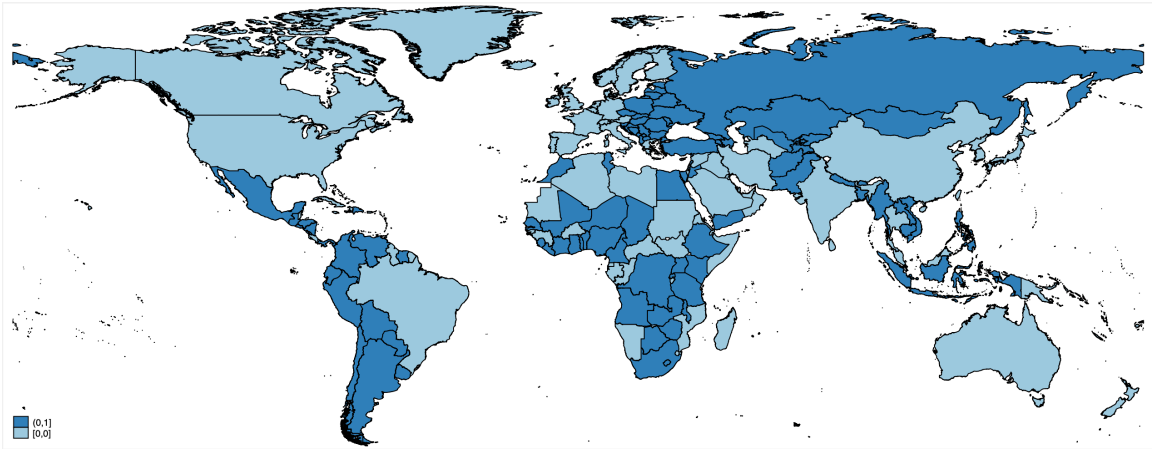
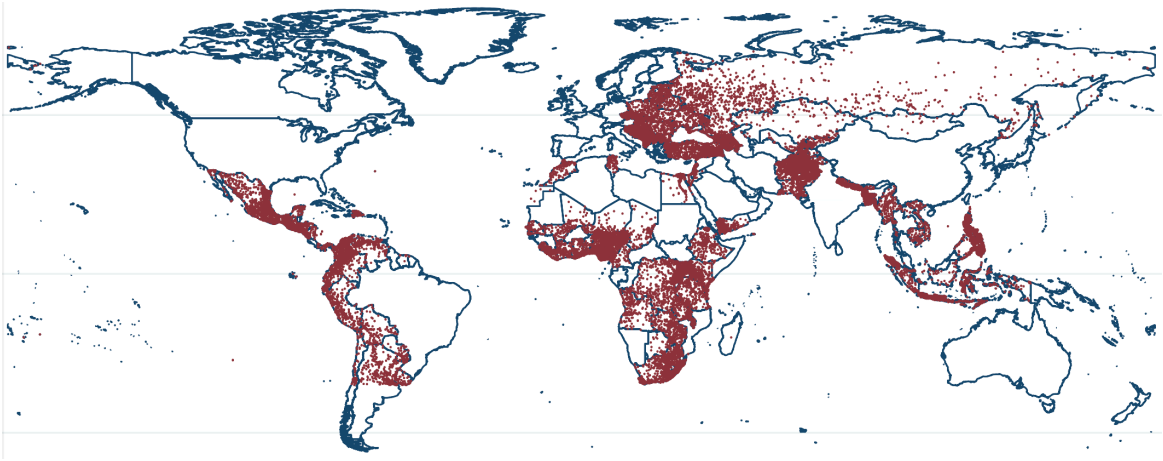
Figures

Figure 1: Coverage of the panel component of the WBES dataset



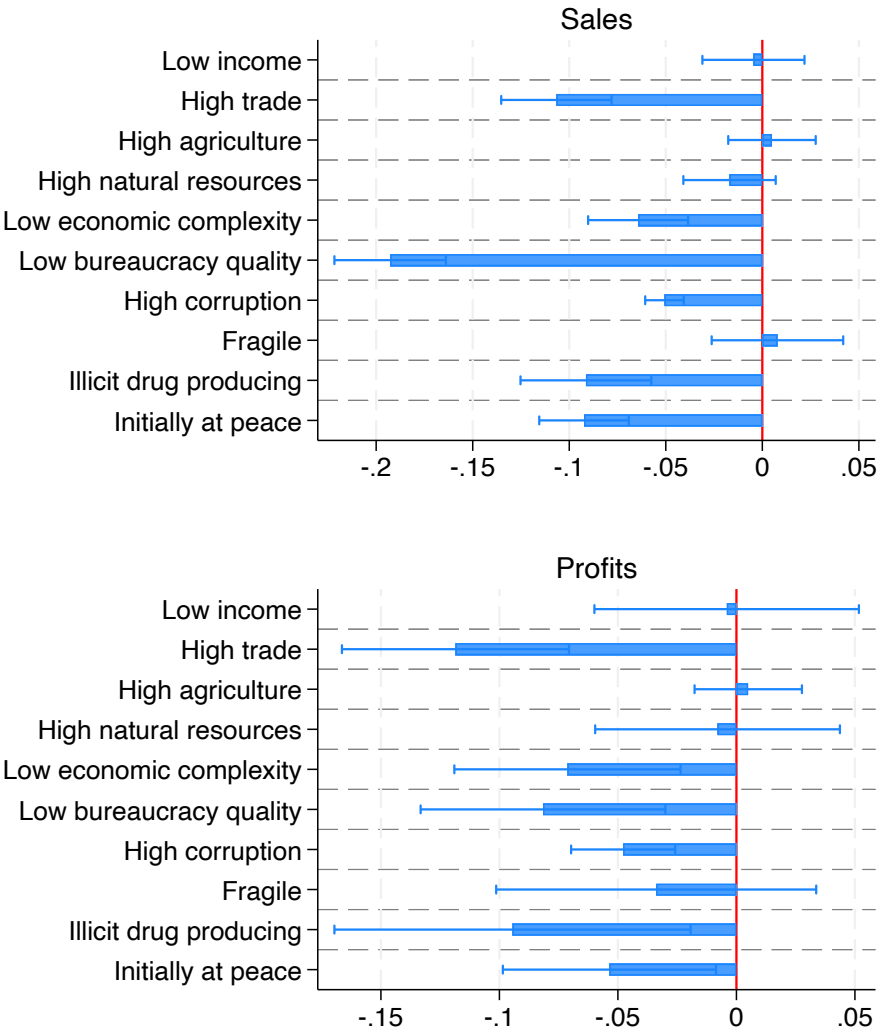
Notes: The countries included in our sample are those shown in dark blue.

Figure 2: Geolocalization of conflict events in the ICEWS dataset (2006-2019)



Notes: Red dots indicate political violence events in our sample of countries.

Figure 3: Main results — Heterogeneity by country characteristics



Notes: This figure shows heterogeneity in the effects of conflict exposure across a range of country-level characteristics. The estimating equation is identical to that of the baseline specification, augmented with the interaction term between conflict exposure and country characteristics. Each bar represents the estimated coefficient on this interaction term, with horizontal lines indicating 95% confidence intervals. The top panel presents results for *Sales*, while the bottom panel shows results for *Profits*. All variables are defined in Table C1.

Tables

Table 1: Descriptive statistics

	Mean	Median	SD	Min	Max	N
Sales	12.539	12.721	2.829	0.172	29.205	36087
Total costs	11.451	11.433	2.652	0.000	21.642	27557
Profits	11.580	12.513	5.377	-18.715	25.788	27557
Conflict exposure	2.718	2.773	2.101	0.000	7.193	36087
Age	2.772	2.833	0.795	0.000	5.352	35716
Size	3.296	3.045	1.386	0.000	12.044	33848
Export	0.157	0.000	0.363	0.000	1.000	35859
Raw mat & interm costs	11.262	11.574	3.273	0.000	23.431	16891
Electric expenditure	8.027	8.066	2.749	0.000	19.576	30535
Power outages	0.547	1.000	0.498	0.000	1.000	35901
Imported inputs	32.312	20.000	36.179	0.000	100.000	18208
Labor costs	10.563	10.696	2.604	0.000	21.574	33175
Number workers	3.383	3.135	1.320	0.000	8.700	35651
% unskilled workers	29.009	20.000	31.449	0.000	100.000	16887
Unit wage	7.251	7.565	1.939	0.000	15.716	32808
Number competitors	2.589	3.434	1.003	0.000	3.434	15247
Informal (dummy)	0.501	1.000	0.500	0.000	1.000	34503
Informal (intensity)	1.587	2.000	1.397	0.000	4.000	34503
Low income	0.656	1.000	0.475	0.000	1.000	36087
High trade	0.492	0.000	0.500	0.000	1.000	34530
High agriculture	0.506	1.000	0.500	0.000	1.000	35721
High natural resources	0.495	0.000	0.500	0.000	1.000	36087
Low economic complexity	0.520	1.000	0.500	0.000	1.000	33625
Low bureaucracy quality	0.331	0.000	0.471	0.000	1.000	31081
High corruption	0.224	0.000	0.417	0.000	1.000	31081
Fragile	0.121	0.000	0.327	0.000	1.000	36087
Illicit drug producing	0.176	0.000	0.381	0.000	1.000	36087
Initially at peace	0.677	1.000	0.468	0.000	1.000	36087

Notes: Descriptive statistics for the main variables employed. All variables are defined in Table C1.

Table 2: Conflict exposure — Sales, total costs, and profits

Dependent variable:	Sales		Total costs		Profits	
	(1)	(2)	(3)	(4)	(5)	(6)
Conflict exposure	-0.167***	-0.170***	-0.122***	-0.137***	-0.0853	-0.0999
	[0.0174]	[0.0177]	[0.0196]	[0.0193]	[0.0645]	[0.0682]
Size		0.654***		0.686***		0.635***
		[0.0240]		[0.0253]		[0.0814]
Age		0.0748**		0.0667**		0.178
		[0.0328]		[0.0334]		[0.109]
Export		0.000847		0.00160***		-0.00123
		[0.000530]		[0.000572]		[0.00216]
Firm FE	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes
Adj R-squared	0.685	0.709	0.758	0.782	0.217	0.222
Sample	36,087	33,402	27,557	25,855	27,557	25,855

Notes: High-dimensional fixed effect estimates. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. All variables are defined in Table C1.

Table 3: Mechanisms — Raw materials and intermediate inputs, labor, and market competition

Dependent variable:	Raw materials and intermediate inputs				Labor				Market competition		
	Raw mat & interm costs	Electricity expenditure	Power outages	Imported inputs (%)	Labor cost	Number workers	Unskilled workers (%)	Unit wage	Number competitors	Informal (dummy)	Informal (intensity)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Conflict exposure	-0.188***	-0.175***	0.0161***	-1.151**	-0.186***	-0.00306	1.285***	-0.158***	0.00234	0.00862*	0.0293**
	[0.0329]	[0.0210]	[0.00478]	[0.471]	[0.0177]	[0.00410]	[0.458]	[0.0162]	[0.0191]	[0.00484]	[0.0146]
Firm controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Adj R-squared	0.676	0.650	0.243	0.495	0.725	0.930	0.221	0.594	0.206	0.207	0.269
Sample	15,916	28,550	33,256	17,071	30,914	33,073	15,920	30,616	14,192	31,976	31,976

Notes: High-dimensional fixed effect estimates. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. All variables are defined in Table C1.

Table 4: Mechanisms — Heterogeneity by conflict status

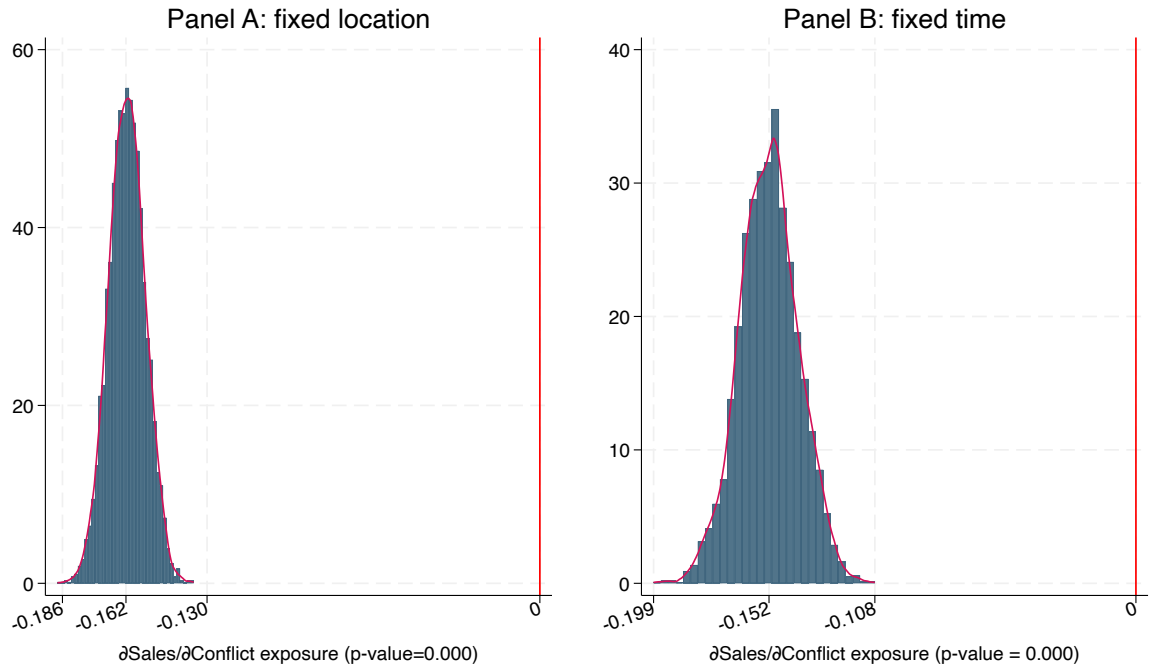
Dependent variable:	Raw materials and intermediate inputs				Labor				Market competition		
	Raw mat & interm costs	Electricity expenditure	Power outages	Imported inputs (%)	Labor cost	Number workers	Unskilled workers (%)	Unit wage	Number competitors	Informal (dummy)	Informal (intensity)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Conflict exposure × Initially in conflict	-0.0670	-0.0712***	0.00650	-1.799**	-0.0107	-0.0109*	0.205	0.0179	0.0150	-0.00310	-0.000584
	[0.0414]	[0.0256]	[0.00624]	[0.873]	[0.0207]	[0.00564]	[0.562]	[0.0196]	[0.0232]	[0.00701]	[0.0218]
Conflict exposure × Initially at peace	-0.326***	-0.283***	0.0245***	-1.141	-0.348***	0.00376	2.741***	-0.319***	-0.0151	0.0176***	0.0523***
	[0.0477]	[0.0316]	[0.00664]	[0.728]	[0.0269]	[0.00551]	[0.727]	[0.0242]	[0.0299]	[0.00617]	[0.0180]
Firm controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Adj R-squared	0.677	0.651	0.244	0.545	0.728	0.930	0.221	0.599	0.206	0.207	0.270
Sample	15,916	28,550	33,256	7,590	30,914	33,073	15,920	30,616	14,192	31,976	31,976

Notes: High-dimensional fixed effect estimates. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. All variables are defined in Table C1.

A Appendix

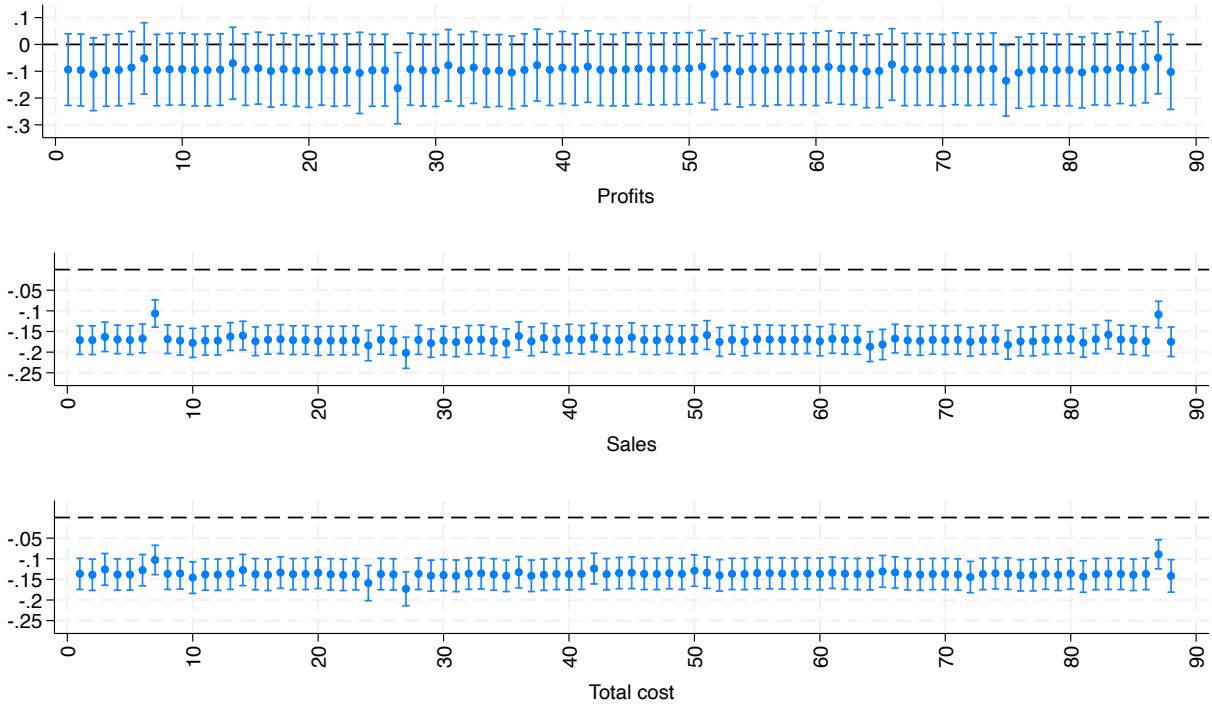
A.1 Appendix Figures

Figure A1: Placebo



Notes: In this plot, we perform two exercises in the spirit of Borusyak and Hull (2023). In the left panel, we fix the location while randomizing the timing of the conflict events. Specifically, each firm is assigned its actual conflict exposure from $t-2$ to $t-5$, using values drawn from a uniform distribution. This is used as an additional control in the baseline sales regression to account for the predictable component of conflict, as captured by past events. We repeat this randomization 5000 times and graph the marginal effects of our main measure of conflict exposure, together with its empirical distribution (Epanechnikov kernel density). In the right panel, we conduct the same exercise but use as a control the number of conflict events affecting other firms in the same year and geographical area of the firm.

Figure A2: Baseline — Excluding one country per time



Notes: This figure presents the estimates from our baseline specifications in columns 2, 4, and 6 of Table 2, obtained by sequentially excluding one country at a time from the estimation sample. 95% confidence intervals are reported.

A.2 Appendix Tables

Table A1: List of countries and waves included in the analysis

Country	Waves	Country	Waves
Afghanistan	2008–2014	Mali	2007–2010–2016
Albania	2013–2019	Mexico	2006–2010
Argentina	2006–2010–2017	Moldova	2009–2013–2019
Armenia	2009–2013–2020	Mongolia	2009–2013–2019
Azerbaijan	2009–2013–2019	Montenegro	2009–2013–2019
Bangladesh	2007–2013	Morocco	2013–2019
Belarus	2008–2013–2018	Myanmar	2014–2016
Benin	2009–2016	Nepal	2009–2013
Bhutan	2009–2015	Nicaragua	2006–2010–2016
Bolivia	2006–2010–2017	Niger	2009–2017
Bosnia and Herzegovina	2009–2013–2019	Nigeria	2007–2014
Bulgaria	2007–2009–2013–2019	North Macedonia	2009–2013–2019
Cambodia	2013–2016	Pakistan	2007–2013
Cameroon	2009–2016	Panama	2006–2010
Chad	2009–2018	Paraguay	2006–2010–2017
Chile	2006–2010	Peru	2006–2010–2017
Colombia	2006–2010–2017	Philippines	2009–2015
Croatia	2013–2019	Poland	2009–2013–2019
Czech Republic	2009–2013–2019	Romania	2009–2013–2019
Cote d'Ivoire	2009–2016	Russia	2009–2012–2019
Dominican Republic	2010–2016	Rwanda	2006–2011–2019
Democratic Republic of Congo	2006–2010–2013	Senegal	2007–2014
Ecuador	2006–2010–2017	Serbia	2009–2013–2019
Egypt	2013–2016–2020	Sierra Leone	2009–2017
El Salvador	2006–2010–2016	Slovakia	2009–2013–2019
Estonia	2009–2013–2019	Slovenia	2009–2013–2019
Ethiopia	2011–2015	South Africa	2007–2020
Georgia	2008–2013–2019	Suriname	2010–2018
Ghana	2007–2013	Tajikistan	2008–2013–2019
Guatemala	2006–2010–2017	Tanzania	2006–2013
Honduras	2006–2010–2016	Timor-Leste	2009–2015
Hungary	2009–2013–2019	Togo	2009–2016
Indonesia	2009–2015	Tunisia	2013–2020
Jordan	2013–2019	Turkey	2008–2013–2019
Kazakhstan	2009–2013–2019	Uganda	2006–2013
Kenya	2007–2013–2018	Ukraine	2008–2013–2019
Kosovo	2009–2013–2019	Uruguay	2006–2010–2017
Kyrgyz Republic	2009–2013–2019	Uzbekistan	2008–2013–2019
Lao PDR	2009–2012–2016–2018	Venezuela	2006–2010
Latvia	2009–2013–2019	Vietnam	2009–2015
Lebanon	2013–2019	West Bank and Gaza	2013–2019
Lesotho	2009–2016	Yemen	2010–2013
Liberia	2009–2017	Zambia	2007–2013–2019
Lithuania	2009–2013–2019	Zimbabwe	2011–2016
Malawi	2009–2014		

Notes: List of countries with panel and geolocated information, along with their available waves.

Table A2: Sample selection

	Firm included in the panel component of the WBES				
	(1)	(2)	(3)	(4)	(5)
Conflict exposure	0.00278	0.00284	0.00344	0.00311	0.00311
	[0.00229]	[0.00218]	[0.00224]	[0.00210]	[0.00210]
Sales			-0.00446	-0.00162	-0.00153
			[0.00364]	[0.00306]	[0.00294]
Total costs				0.00203	0.00202
				[0.00383]	[0.00373]
Profits					-0.0000196
					[0.000510]
Firm Controls	no	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes
Adj R-squared	0.330	0.335	0.347	0.373	0.373
Sample	80,477	70,985	62,432	46,981	46,970

Notes: High-dimensional fixed effect estimates. The dependent variable is *Firm included in the panel component of the WBES*, a dummy that equals one if the firm belongs to the panel component of the WBES, and zero, otherwise. *Firm Controls* include: age, size, and export status. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. Other variables are defined in Table C1.

Table A3: Conflict and missing values

	Missing Sales	Missing Sales	Missing Raw mat & interm	Missing Labor costs
	(1)	(2)	(3)	(4)
Conflict exposure	0.00126	0.000460	0.00323	-0.000482
	[0.00203]	[0.00224]	[0.00208]	[0.00163]
Size		-0.00394**	0.000925	0.00122
		[0.00169]	[0.00188]	[0.00174]
Age		0.00196	-0.00381	0.00222
		[0.00257]	[0.00418]	[0.00310]
Export		-0.0000705	-0.000000825	-0.0000650*
		[0.0000535]	[0.0000734]	[0.0000371]
Area FE	yes	yes	yes	yes
Adj R-squared	0.140	0.134	0.661	0.136
Sample	40,881	37,267	33,410	33,410

Notes: High-dimensional fixed effects estimates. The dependent variable in columns 1-2 is a dummy equal to one if the firm does not report its sales, and zero otherwise. In columns 3 and 4, the dependent variables are dummies for missing values in i) the cost of raw materials and intermediates (including electricity expenditures), and ii) labor costs, respectively. *Area* refers to the most granular geographical unit available in the WBES (i.e., the sampling region). Our dataset includes 314 areas, which are typically larger than the 20 km radius buffer used to construct the conflict exposure measure. Robust standard errors in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Other variables are defined in Table C1.

Table A4: Attrition — Conflict exposure and firm exit

	Firm exit		
	(1)	(2)	(3)
Conflict exposure$_{average}$	0.00861**	0.00971***	0.0108***
	[0.00370]	[0.00375]	[0.00412]
Sales			-0.0103**
			[0.00517]
Size		-0.00553	0.00706
		[0.00474]	[0.00793]
Age		-0.0214**	-0.0199**
		[0.00911]	[0.0101]
Export		-0.00822	-0.0102
		[0.0177]	[0.0191]
Time FE	yes	yes	yes
Country FE	yes	yes	yes
Adj R-squared	0.075	0.076	0.072
Sample	6,216	6,091	5,198

Notes: High-dimensional fixed effects estimates. The dependent variable is *Firm exit*, a dummy equal to one if the firm exits the panel and zero otherwise. The sample includes the cross-section of firms interviewed in the first year of each country's survey. *Conflict exposure*$_{average}$ is defined as the yearly average number of conflict events (log 1+) within a 20 km radius of the firm between the two survey waves used to compute *Firm exit*. All other regressors capture firm characteristics measured prior to the exit period. Robust standard errors in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Other variables are defined in Table C1.

Table A5: List of political violence events or conflict-related events 2006-2019

Type of event	Number of events
ICEWS	
Arrest, detain, or charge with legal action	442349
Use conventional military force	203460
Use unconventional violence	186438
Abduct, hijack, or take hostage	35723
Protest violently, riot	29332
Physically assault	23750
Expel or deport individuals	21141
Use tactics of violent repression	20798
Mobilize or increase armed forces	18346
Employ aerial weapons	17757
Sexually assault	13919
Conduct suicide, car, or other non-military bombing	12284
Carry out suicide bombing	11587
Coerce	10386
Kill by physical assault	7817
Mobilize or increase police power	6265
Assassinate	5887
Demonstrate military or police power	5229
Torture	4048
Seize or damage property	1623
Destroy property	1388
Engage in mass killings	1203
Attempt to assassinate	551
Expel or withdraw peacekeepers	528
Expel or withdraw	412
Engage in violent protest for leadership change	206
Use chemical, biological, or radiological weapons	202
Carry out car bombing	129
Carry out roadside bombing	41
Engage in ethnic cleansing	40
Engage in mass expulsion	29
ACLED	
Battles	91683
Violence against civilians	74565
Explosions/Remote violence	43563
Protests	1312
Riots	7932
Strategic developments	915

Notes: List and number of political violence and hostile events recorded in the ICEWS and ACLED datasets between 2006 and 2019, across countries included in our sample.

Table A6: Threats to identification — Interacted time fixed effects and leads

	Sales	Profits	Sales	Profits
	(1)	(2)	(3)	(4)
Conflict exposure	-0.0469**	-0.0909	-0.0604**	-0.152
	[0.0208]	[0.0847]	[0.0277]	[0.113]
Future conflict exposure			0.0211	0.0933
			[0.0267]	[0.0984]
Firm controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Area × Time FE	yes	yes	yes	yes
Adj R-squared	0.838	0.241	0.838	0.241
Sample	33,402	25,855	33,402	25,855

Notes: High-dimensional fixed effect estimates. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. All variables are defined in Table C1.

Table A7: Endogeneity of conflict events — Level economic activity and conflict intensity

PANEL A	Number conflict events (per area): $t+1$				
	(1)	(2)	(3)	(4)	(5)
Level of firms' economic activity in the area	-0.0832	0.263	-0.0723	-0.0780	-0.0761
	[0.0743]	[0.293]	[0.0567]	[0.0672]	[0.102]
Time FE	yes	yes	yes	yes	yes
District FE	yes	yes	yes	yes	yes
Definition of firms' economic activity:	Sales	Labor	Profits	Sales growth	Labor growth
Adj R-squared	0.342	0.455	0.334	0.337	0.449
Sample	2,288	2,295	2,140	2,207	2,302
PANEL B	Number conflict events (per area): $t+1:t+2$				
	(1)	(2)	(3)	(4)	(5)
Level of firms' economic activity in the area	-0.0279	0.302	-0.0645	-0.0357	-0.0262
	[0.0266]	[0.314]	[0.0476]	[0.0319]	[0.0481]
Time FE	yes	yes	yes	yes	yes
District FE	yes	yes	yes	yes	yes
Definition of firms' economic activity:	Sales	Labor	Profits	Sales growth	Labor growth
Adj R-squared	0.712	0.782	0.710	0.711	0.775
Sample	1,789	1,792	1,677	1,729	1,796
PANEL C	Number conflict events (per area): $t+1:t+3$				
	(1)	(2)	(3)	(4)	(5)
Level of firms' economic activity in the area	-0.0128	0.440	-0.0655	-0.0423	-0.0823
	[0.0326]	[0.415]	[0.0539]	[0.0367]	[0.0778]
Time FE	yes	yes	yes	yes	yes
District FE	yes	yes	yes	yes	yes
Definition of firms' economic activity:	Sales	Labor	Profits	Sales growth	Labor growth
Adj R-squared	0.711	0.798	0.710	0.710	0.788
Sample	1,499	1,501	1,415	1,445	1,503

Notes: High-dimensional fixed effects estimates. This table tests for potential endogeneity in conflict by examining whether the level of economic activity in a given area is correlated with the number of conflict events occurring in subsequent years. Areas are defined at the second-level administrative unit (GID2). We consider several proxies for economic activity —average sales, employment, profits, sales growth, and employment growth— reported in columns 1 to 5, respectively, using WBES survey data. In Panels A, B, and C, the dependent variable is the number of conflict events occurring in the area over the next one, two, and three years, respectively. Robust standard errors in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Table C1.

Table A8: Endogeneity of conflict events — Local composition of the economic activity and conflict intensity

Density of conflict events:	$t+1$ (1)	$t+1:t+2$ (2)	$t+1:t+3$ (3)
Food	-0.624	-0.104	0.134
	[0.472]	[0.299]	[0.301]
Textiles, Garments, Leather	-0.151	-0.238	-0.0485
	[0.182]	[0.245]	[0.252]
Metals & Machinery	-0.635	-0.664	-0.466
	[0.477]	[0.525]	[0.486]
Electronics	-0.621	-1.346	-0.932
	[0.476]	[1.146]	[0.982]
Chemicals	-0.410	-0.245	-0.0410
	[0.344]	[0.292]	[0.313]
Furniture	0.704	0.735	1.710
	[0.725]	[0.844]	[1.718]
Non-metallic	-0.254	0.108	0.296
	[0.396]	[0.324]	[0.428]
Autos	-0.214	-0.365	0.511
	[0.243]	[0.433]	[0.889]
Other Manufacturing	-0.918	-0.164	0.511
	[0.598]	[0.469]	[0.860]
Retail & Wholesale	-0.355	0.0256	0.113
	[0.463]	[0.210]	[0.309]
Hospitality & Tourism	-0.308	-0.801	-0.910
	[0.449]	[0.902]	[1.038]
Construction	-1.036	-0.834	-0.125
	[0.692]	[0.552]	[0.384]
Transportation	0.682	1.253	2.128
	[0.913]	[1.576]	[1.956]
Education & Cultural	-1.521	-11.99	-13.52
	[1.991]	[9.603]	[14.06]
Time FE	yes	yes	yes
District FE	yes	yes	yes
Adj R-squared	0.443	0.774	0.785
Sample	2,334	1,817	1,521

Notes: High-dimensional fixed effects estimates. This table tests for potential endogeneity in conflict by examining whether the density of conflict events in a given area is correlated with the sectoral composition of firms operating there. Areas are defined at the second-level administrative unit (GID2). The dependent variable in columns 1–3 is the log number of conflict events occurring in the area over the next one, two, and three years, respectively. Each independent variable represents the share of firms in the area operating within a given sector. Robust standard errors in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Table C1.

Table A9: Threats to identification — Excluding large firms, firms with high sales or profits, and located in capital cities

Panel A	Excluding large firms		
	Sales	Total costs	Profit
	(1)	(2)	(3)
Conflict exposure	-0.220***	-0.156***	-0.109
	[0.0298]	[0.0312]	[0.103]
Firm controls	yes	yes	yes
Firm FE	yes	yes	yes
Time FE	yes	yes	yes
Adj R-squared	0.582	0.679	0.202
Sample	17,483	13,455	13,455
Panel B	Excluding firms in capital cities		
	Sales	Total costs	Profit
	(1)	(2)	(3)
Conflict exposure	-0.202***	-0.190***	-0.161
	[0.0264]	[0.0293]	[0.0982]
Firm controls	yes	yes	yes
Firm FE	yes	yes	yes
Time FE	yes	yes	yes
Adj R-squared	0.726	0.782	0.219
Sample	17,153	13,347	13,347
Panel C	Excluding firms with high sales		
	Sales	Total costs	Profit
	(1)	(2)	(3)
Conflict exposure	-0.176***	-0.138***	-0.119
	[0.0200]	[0.0225]	[0.0848]
Firm controls	yes	yes	yes
Firm FE	yes	yes	yes
Time FE	yes	yes	yes
Adj R-squared	0.682	0.770	0.204
Sample	25,189	19,646	19,646
Panel D	Excluding firms with high profits		
	Sales	Total costs	Profit
	(1)	(2)	(3)
Conflict exposure	-0.177***	-0.151***	-0.137
	[0.0262]	[0.0241]	[0.0886]
Firm controls	yes	yes	yes
Firm FE	yes	yes	yes
Time FE	yes	yes	yes
Adj R-squared	0.687	0.771	0.177
Sample	19,622	19,359	19,359

Notes: High-dimensional fixed effect estimates. Panel A restricts the sample to companies below 20 employees. Panel B excludes firms located in capital cities. Panel C excludes firms with sales above the 75th percentile of the country-year distribution. Panel D excludes firms with profits above the 75th percentile of the country-year distribution. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. All variables are defined in Table C1.

Table A10: Threats to identification — Clean controls analysis

	Sales		
	(1)	(2)	(3)
$\mathbb{I}$(Conflict exposure $\geq$ p(25))	-0.299***	-0.245***	
	[0.0428]	[0.0848]	
Conflict exposure			-0.112***
			[0.0416]
Sample	Full	Clean	Clean
		$t-2$	$t-2$
Conflict measure	Dummy	Dummy	Continuous
Adj R-squared	0.708	0.708	0.709
Sample	33,402	13,682	13,682
	Profits		
	(1)	(2)	(3)
$\mathbb{I}$(Conflict exposure $\geq$ p(25))	-0.129	-0.444	
	[0.172]	[0.351]	
Conflict exposure			-0.102
			[0.159]
Sample	Full	Clean	Clean
		$t-2$	$t-2$
Conflict measure	Dummy	Dummy	Continuous
Adj R-squared	0.222	0.233	0.233
Sample	25,855	10,497	10,497
Firm controls	yes	yes	yes
Firm FE	yes	yes	yes
Time FE	yes	yes	yes

Notes: Column 1 replicates the baseline analysis from Table 2 using a dummy variable as an alternative measure of conflict exposure. The indicator $\mathbb{I}$(Conflict exposure $\geq$ p(25)) equals one if the firm is exposed to a number of conflict events above the 25th percentile of the country-year distribution, and zero otherwise. Columns 2 and 3 restrict the sample to a "clean" set of movers and stayers, using both the dummy and continuous measures of conflict exposure. In this framework, movers are defined as firms experiencing substantial treatment intensity —those ranked at or above the 25th percentile of the treatment intensity distribution— while quasi-stayers are firms with negligible exposure, falling below that threshold. Firms already significantly exposed to conflict prior to treatment are excluded. This design allows for comparisons between firms experiencing conflict exposure for the first time and those that remain untreated or are yet to be treated. To further refine the sample, we exclude firms that were exposed to conflict in the two years preceding the survey wave. This additional restriction ensures that treatment effects are not confounded by prior conflict exposure. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Other variables are defined in Table C1.

Table A11: Threats to identification — Confounding factors

	$\hat{\beta}$	$\delta =$ 0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	$\hat{\delta}$ for $\beta = 0$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sales	-0.170	-0.174	-0.180	-0.187	-0.193	-0.199	-0.206	-0.212	-0.219	-0.226	-0.233	-3.596
Total costs	-0.137	-0.143	-0.150	-0.158	-0.165	-0.173	-0.181	-0.189	-0.197	-0.206	-0.215	-2.447

Notes: In this table, we employ the Oster (2019) approach to test the stability of our estimates with respect to unobservable confounding factors. $\hat{\beta}$, in column 1, is the estimated coefficient of *Conflict exposure* in the regressions of Table 2, columns 2 and 4 (only significant estimates are reported). In columns 2-11, we report the estimated β for values of δ in the $[0; 1]$ interval. As suggested by Oster (2019) calculations are based on $R^2_{max} = 1.3R^2$. $\hat{\delta}$ for $\beta = 0$, in column 12, is the estimated value of δ that would reduce the effect of conflict exposure to zero.

Table A12: Threats to identification — Spillover effects

	Sales (1)	Total costs (2)	Profit (3)
Conflict exposure	-0.112***	-0.115***	-0.0442
	[0.0254]	[0.0233]	[0.0749]
Conflict exposure suppliers	-0.0989	0.122	-0.00776
	[0.118]	[0.149]	[0.345]
Conflict exposure buyers	-0.0413	-0.216	-0.165
	[0.116]	[0.155]	[0.366]
Firm controls	yes	yes	yes
Firm FE	yes	yes	yes
Time FE	yes	yes	yes
Adj R-squared	0.720	0.783	0.220
Sample	32,762	25,458	25,458

Notes: This table replicates our main results while accounting for potential spatial spillovers of conflict transmitted through the production network within each country. We use OECD input-output matrices (predetermined as of the year 2000) and match them with firms' main product classifications (ISIC 4D) from the WBES. To ensure consistency across classifications, both the input-output tables and WBES sector codes are aggregated into 15 sectors. For each firm, we construct two weighting matrices: one for dependence on input purchases (supplier matrix) and another for output sales (buyer matrix). We then compute two interaction terms: *Conflict exposure suppliers* is defined as the interaction between the supplier matrix and the level of conflict exposure for each firm included in the supplier network; *Conflict exposure buyers* is the corresponding interaction using the buyer matrix and the conflict exposure of firms in the buyer network. These measures allow us to test whether firms —beyond their own direct exposure— are also indirectly affected by conflict through production linkages Standard errors in brackets are two-way clustered at the firm level and at the sector–country–year level, which correspond to the two dimensions along which the measures of conflict exposure are defined. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Other variables are defined in Table C1.

Table A13: Robustness — Balanced sample

	Sales		Total costs		Profits	
	(1)	(2)	(3)	(4)	(5)	(6)
Conflict exposure	-0.101***	-0.117***	-0.122***	-0.137***	-0.0853	-0.0999
	[0.0200]	[0.0201]	[0.0196]	[0.0193]	[0.0645]	[0.0682]
Size		0.657***		0.686***		0.635***
		[0.0258]		[0.0253]		[0.0814]
Age		0.0865**		0.0667**		0.178
		[0.0345]		[0.0334]		[0.109]
Export		0.000700		0.00160***		-0.00123
		[0.000577]		[0.000572]		[0.00216]
Firm FE	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes
Adj R-squared	0.743	0.766	0.758	0.782	0.217	0.222
Sample	27,557	25,855	27,557	25,855	27,557	25,855

Notes: High-dimensional fixed effects estimates. This table replicates the analysis from Table 2, restricting the sample to firms with non-missing information for all outcome variables. Robust standard errors in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Table C1.

Table A14: Robustness — Manufacturing sector only

Dependent variable:	Sales		Total costs		Profits	
	(1)	(2)	(3)	(4)	(5)	(6)
Conflict exposure	-0.183***	-0.198***	-0.196***	-0.206***	-0.0968	-0.136
	[0.0239]	[0.0236]	[0.0275]	[0.0279]	[0.100]	[0.105]
Size		0.639***		0.623***		0.637***
		[0.0319]		[0.0364]		[0.128]
Age		0.0606		0.0493		0.240
		[0.0417]		[0.0459]		[0.160]
Export		0.00148**		0.00195***		-0.000764
		[0.000644]		[0.000729]		[0.00283]
Firm FE	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes
Adj R-squared	0.731	0.751	0.762	0.778	0.185	0.189
Sample	19,247	17,960	15,496	14,650	15,496	14,650

Notes: High-dimensional fixed effect estimates. This table replicates the analysis in Table 2 restricting the sample to firms in the manufacturing sector. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. All variables are defined in Table C1.

Table A15: Robustness — Alternative definitions of conflict exposure: Different buffers and time horizons

	Sales			
	(1)	(2)	(3)	(4)
Conflict exposure	-0.0515***	-0.102***	-0.230***	-0.138***
	[0.0137]	[0.0149]	[0.0200]	[0.0165]
Adj R-squared	0.707	0.708	0.710	0.709
Sample	33,402	33,402	33,402	33,402
	Total costs			
	(1)	(2)	(3)	(4)
Conflict exposure	-0.0482***	-0.0766***	-0.183***	-0.0993***
	[0.0145]	[0.0163]	[0.0224]	[0.0177]
Adj R-squared	0.781	0.781	0.782	0.781
Sample	25,855	25,855	25,855	25,855
	Profits			
	(1)	(2)	(3)	(4)
Conflict exposure	-0.00341	-0.0292	-0.126	-0.0375
	[0.0538]	[0.0605]	[0.0856]	[0.0636]
Adj R-squared	0.222	0.222	0.222	0.222
Sample	25,855	25,855	25,855	25,855
Conflict	5km (log)	10km (log)	40km (log)	20km (log)
Period	Prev year	Prev year	Prev year	Prev 2 years
Firm controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Time FE	yes	yes	yes	yes

Notes: High-dimensional fixed effects estimates. This table replicates the baseline results from Table 2 using alternative definitions of conflict exposure. In columns 1-2, we use smaller buffers of 5 km and 10 km, respectively. Column 3 employs a larger buffer of 40 km, while column 4 considers conflict events occurring within a 20 km radius over the previous two years. Robust standard errors in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Table C1.

Table A16: Robustness — Alternative definitions of conflict exposure: Different types of conflict events

	Sales				
	(1)	(2)	(3)	(4)	(5)
Conflict exposure	-0.173***	-0.176***	-0.162***	-0.209***	-0.320***
	[0.0179]	[0.0180]	[0.0179]	[0.0205]	[0.0438]
Adj R-squared	0.709	0.709	0.709	0.709	0.624
Sample	33,402	33,402	33,402	33,402	12,627
	Total costs				
	(1)	(2)	(3)	(4)	(5)
Conflict exposure	-0.137***	-0.141***	-0.103***	-0.101***	-0.0884**
	[0.0196]	[0.0195]	[0.0179]	[0.0196]	[0.0357]
Adj R-squared	0.782	0.782	0.781	0.781	0.755
Sample	25,855	25,855	25,855	25,855	10,469
	Profits				
	(1)	(2)	(3)	(4)	(5)
Conflict exposure	-0.110	-0.106	-0.0731	-0.0705	-0.207*
	[0.0690]	[0.0694]	[0.0677]	[0.0739]	[0.108]
Adj R-squared	0.222	0.222	0.222	0.222	0.254
Sample	25,855	25,855	25,855	25,855	10,469
Type of conflict events	Excluding non-economic	Excluding protests & riots	Intensity -8 : -10	Intensity -10	All
Source	ICEWS	ICEWS	ICEWS	ICEWS	ACLED
Firm controls	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes

Notes: High-dimensional fixed effect estimates. This table replicates the baseline results from Table 2 using alternative definitions of conflict exposure based on the type and intensity of events. Column 1 excludes events that are less likely to have a direct economic impact on firms (see footnote 31). Column 2 excludes events related to protests and riots. Columns 3 and 4 apply quantitative filters, restricting the sample to events with intensity scores between -8 and -10, and exactly -10, respectively. Column 5 uses the ACLED dataset as an alternative source for measuring conflict exposure. Robust standard errors in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Table C1.

Table A17: Robustness — Adjusting standard errors for spatial correlation

	Sales	Total costs	Profits
	(1)	(2)	(3)
Conflict exposure	-0.170***	-0.137***	-0.0999
	[0.0428]	[0.0363]	[0.0654]
Size	0.654***	0.686***	0.635***
	[0.0223]	[0.0205]	[0.0639]
Age	0.0748***	0.0667**	0.178**
	[0.0280]	[0.0273]	[0.0829]
Export	0.000847*	0.00160***	-0.00123
	[0.000513]	[0.000529]	[0.00155]
Firm controls	yes	yes	yes
Firm FE	yes	yes	yes
Time FE	yes	yes	yes
Adj R-squared	0.076	0.113	0.008

Notes: High-dimensional fixed effects estimates with standard errors corrected for spatial correlation, using the arbitrary clustering correction proposed by Colella et al. (2023). *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. All variables are defined in Table C1.

Table A18: Conflict and access to imported inputs

Dependent variable:	Obstacles custom & trade	Days to clear custom
	(1)	(2)
Conflict exposure	0.0355**	0.0554**
	[0.0156]	[0.0281]
Firm FE	yes	yes
Time FE	yes	yes
Adj R-squared	0.151	0.345
Sample	7,489	7,532

Notes: High-dimensional fixed effect estimates. *Obstacles: customs & trade* (column 1) is a dummy variable equal to 1 if the firm reports customs and trade regulations as a major or very severe constraint to its business activity. *Days to clear customs* (column 2) is the (log 1+) average number of days required for a firm's imported goods to clear customs. The sample in column 2 is restricted to importing firms with available customs data. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in Table C1.

Table A19: Labor mechanism — Additional results

Dependent variable:	Number workers	% unskilled workers		% temporary workers	Obstacle: supply skilled
	(1)	(2)	(3)	(4)	(5)
Conflict exposure	-0.0185*			-0.0328	0.0288
	[0.0101]			[0.123]	[0.415]
Conflict exposure × Size	0.00452*				
	[0.00269]				
Conflict exposure × Non differentiated		1.484***			
		[0.519]			
Conflict exposure × Differentiated		1.097**			
		[0.484]			
Conflict exposure × Low severance pay			2.557***		
			[0.970]		
Conflict exposure × High severance pay			1.003**		
			[0.498]		
Firm controls	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes
Adj R-squared	0.930	0.220	0.221	0.244	0.154
Sample	33,073	15,799	15,920	31,951	32,913

Notes: High-dimensional fixed effect estimates. This table explores alternative dependent variables. *% Temporary workers* is the proportion of temporary workers among all employees. *Obstacle: supply skilled* is a dummy variable equal to 1 if the firm reports an inadequately educated workforce as a major or very severe constraint to its business activity. Column 1 presents heterogeneity in the effect of conflict exposure by firm size. Column 2 explores heterogeneity based on whether the firm produces differentiated or non-differentiated goods, as defined by the Rauch (1999) classification. A product is considered differentiated if it is not traded on an organized exchange and does not have a reference price. In column 3, we examine whether the effect varies with the strength of a country's labor regulation, proxied by average severance pay for redundancy dismissal (in weeks of salary, ILO). Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Other variables are defined in Table C1.

Table A20: Other mechanisms — Demand channel

Dependent variable:	Sales	Dependent variable:	Sales		Capacity utilization
	(1)		(2)	(3)	(4)
Conflict exposure × Local	-0.184***	Conflict exposure	-0.180***	-0.182***	0.0379
	[0.0229]		[0.0182]	[0.0184]	[0.337]
Conflict exposure × National	-0.165***	Conflict exposure × Z	0.0578***	0.104***	
	[0.0224]		[0.0215]	[0.0392]	
Conflict exposure × International	-0.129***	Z	-0.0815	-0.0807	
	[0.0322]		[0.0857]	[0.144]	
		Z definition:	Export	Export %	
Firm controls	yes	Firm controls	yes	yes	yes
Firm FE	yes	Firm FE	yes	yes	yes
Time FE	yes	Time FE	yes	yes	yes
Adj R-squared	0.765	Adj R-squared	0.709	0.709	0.212
Indentifying obs.	17,623	Indentifying obs.	28,579	28,579	12,552
Total obs.	22,845	Total obs.	33,401	33,401	16,275

Notes: High-dimensional fixed effect estimates. This table tests for heterogeneities in the effect of conflict on sales based on the markets served by firms. In column 1, we allow the effect of conflict exposure to vary depending on the firm's main destination market: *Local*, *National*, or *International*. In column 2, we interact conflict exposure with *Export*, a dummy variable equal to 1 if the firm exports more than 10% of its sales. Column 3 uses the continuous variable *Export %*, which measures the share of sales derived from exports. In column 4, the dependent variable is *Capacity utilization*, defined as the establishment's actual output expressed as a percentage of its maximum potential output using all available resources. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Other variables are defined in Table C1.

Table A21: Other mechanisms — Obstacles to firms' activity

	Obstacle:				Losses due to theft	Pay for security	Cost of security
	finance	corruption	transport	land			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict exposure	0.000577	0.000000635	-0.00630**	0.000933	0.0176	0.00459	0.000816
	[0.00286]	[0.00383]	[0.00257]	[0.00240]	[0.0375]	[0.00467]	[0.000982]
Firm controls	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes	yes
Adj R-squared	0.077	0.152	0.048	0.067	0.081	0.322	0.039
Sample	33,402	33,402	33,402	33,402	32,360	33,402	24,007

Notes: High-dimensional fixed effect estimates. This table explores alternative dependent variables reflecting the main obstacles to the firm's activity. *Obstacles: finance*, *corruption*, *transport*, and *land* are dummy variables taking the value of one if the firm indicates, respectively, that i) access to finance, ii) corruption, iii) transport, or iv) access to land to be a major or very severe constraint to its business activity. *Losses due to thefts* is the estimated losses (in percentage of annual sales) from theft, robbery, vandalism, or arson that occurred on an establishment's premises. *Pay for security* is a dummy taking the value of one if the firm pays for security. *Cost of security* is a continuous variable measuring the average security costs as a percentage of total annual sales of the firm. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. Other variables are defined in Table C1.

Table A22: Main results — Firm-level heterogeneities

	Sales			
	(1)	(2)	(3)	(4)
Conflict exposure	-0.280***	-0.171***	-0.157***	-0.159***
	[0.0332]	[0.0385]	[0.0177]	[0.0178]
Conflict exposure × Z	0.0313***	0.000148	-0.0242	0.0132
	[0.00794]	[0.0119]	[0.0475]	[0.0160]
Z	*Size*	*Age*	*State owned*	*Foreign owned*
Adj R-squared	0.709	0.709	0.716	0.716
Sample	33,402	33,402	33,150	33,132
	Profits			
	(1)	(2)	(3)	(4)
Conflict exposure	-0.215*	-0.143	-0.0857	-0.0921
	[0.120]	[0.139]	[0.0689]	[0.0691]
Conflict exposure × Z	0.0322	0.0152	-0.0518	0.0428
	[0.0320]	[0.0432]	[0.137]	[0.0543]
Z	*Size*	*Age*	*State owned*	*Foreign owned*
Adj R-squared	0.222	0.222	0.223	0.223
Sample	25,855	25,855	25,754	25,741
Firm controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Time FE	yes	yes	yes	yes

Notes: High-dimensional fixed effect estimates. This table explores firm-level heterogeneities in our baseline analysis of Table 2. *State owned* is a dummy taking the value of one for firms with at least 10% of government/state ownership. *Foreign owned* is a dummy taking the value of one for foreign-owned firms. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. Other variables are defined in Table C1.

Table A23: Main results — Heterogeneity by sector

	Sales	Total costs	Profits
	(1)	(2)	(3)
Conflict exposure × Food	-0.206***	-0.194***	-0.118
	[0.0345]	[0.0392]	[0.145]
Conflict exposure × Metals & Machinery	-0.140**	-0.129	0.416
	[0.0695]	[0.0926]	[0.406]
Conflict exposure × Textiles, Garments, Leather	-0.191***	-0.166***	-0.0290
	[0.0389]	[0.0394]	[0.167]
Conflict exposure × Electronics	-0.179***	-0.213***	0.0465
	[0.0431]	[0.0505]	[0.242]
Conflict exposure × Chemicals	-0.141***	-0.165***	-0.184
	[0.0475]	[0.0501]	[0.207]
Conflict exposure × Furniture	-0.177***	-0.173***	-0.447**
	[0.0361]	[0.0450]	[0.184]
Conflict exposure × Non-metallic	-0.103	-0.373***	-0.515
	[0.0998]	[0.113]	[0.479]
Conflict exposure × Other Manufacturing	-0.113***	-0.0763*	-0.0666
	[0.0333]	[0.0403]	[0.135]
Conflict exposure × Retail & Wholesale	-0.174***	-0.121***	-0.0250
	[0.0260]	[0.0262]	[0.0769]
Conflict exposure × Hospitality & Tourism	-0.181***	-0.0955*	0.0494
	[0.0601]	[0.0511]	[0.183]
Conflict exposure × Construction	-0.0918**	-0.0564	-0.174
	[0.0454]	[0.0555]	[0.145]
Conflict exposure × Transportation	-0.201***	-0.0717	-0.454**
	[0.0625]	[0.0524]	[0.211]
Conflict exposure × Other services	0.0126	-0.113	-0.245
	[0.0741]	[0.0761]	[0.177]
Firm controls	yes	yes	yes
Firm FE	yes	yes	yes
Time FE	yes	yes	yes
Adj R-squared	0.721	0.788	0.226
Sample	32,763	25,459	25,459

Notes: High-dimensional fixed effect estimates. Robust standard errors in brackets. *, **, *** indicate statistical significance at the 10%, 5%, and 1%, respectively. All variables are defined in Table C1.

B Data Appendix

Table C1: Variable definition

Variable name	Variable definition
Age	log–age. Question b5 of the WBES questionnaire: "In what year did this establishment begin operations?". Age = ln(1+T–b5), where T is the last fiscal year in the survey wave. Source: WBES.
Size	log–employees. Question l2 of the WBES questionnaire: "Looking back, at the end of two fiscal years ago, how many permanent, full–time individuals worked in this establishment? Please include all employees and managers". Size= ln(1+l2). Source: WBES.
Export	dummy for exporting firms. Question d3 of the WBES questionnaire: "Coming back to the last fiscal year, what percentage of this establishment's sales were: national sales [d3a], indirect exports (sold domestically to a third party that exports products) [d3b], direct exports [d3c]?". Export=1 if d3b+d3c $\geq$ 10%. Source: WBES.
Sales	log–sales. Question d2 of the WBES questionnaire: "In the last fiscal year, what was this establishment's total annual sales for all products and services?". Sales=ln(1+d2). Values are expressed in 2000 constant USD. Source: WBES.
Labor costs	log–total labor costs. Question n2a of the WBES questionnaire: "From this establishment's income statement for the last fiscal year, please provide the following information: a. Total annual cost of labor including wages, salaries, bonuses, social security payments". Labor costs=ln(1+n2a). Values are expressed in 2000 constant USD. Source: WBES.
Electric expenditure	log–expenditure on electricity. Question n2b of the WBES questionnaire: "From this establishment's income statement for the last fiscal year, please provide the following information: b. Total annual cost of electricity". Electric expend=ln(1+n2b). Values are expressed in 2000 constant USD. Source: WBES.
Raw mat & interm costs	log–expenditure on raw materials and intermediates. Question n2e of the WBES questionnaire: "From this establishment's income statement for the last fiscal year, please provide the following information: e. Total annual cost of raw materials and intermediate goods used in production". Raw mat & interm=ln(1+n2e). Values are expressed in 2000 constant USD. Source: WBES.
Total costs	log of total production costs, defined as $\ln(1 + n2a + n2b + n2e)$ for manufacturing firms and $\ln(1 + n2a + n2b)$ for service firms, where $n2a$ is labor costs, $n2b$ is raw material costs, and $n2e$ is energy expenditures (not available for service firms, see footnote 9). Values are expressed in 2000 constant USD. Source: WBES.
Profits	inverse hyperbolic sine transformation of profits. Profits=$\sinh^{-1}$(Sales$-$Total costs). Source: WBES.
Power outages	dummy for firms experiencing power outages. Question c6 of the WBES questionnaire: "Over the last fiscal year, did this establishment experience power outages?". Power outages=1 if c6=Yes. Source: WBES.
Imported inputs	share of imported inputs. Question d12 of the WBES questionnaire: "In last fiscal year, as a proportion of all material inputs or supplies purchased that year, what percentage of this establishment's material inputs or supplies were: d12b. material inputs or supplies of foreign origin?". Imported inputs=d12b. Source: WBES.

Number workers	log–number of permanent and temporary workers. The number of temporary workers is adjusted for the number of months of their employment. Question l1 of the WBES questionnaire: "At the end of last fiscal year, how many permanent, full-time individuals worked in this establishment? Please include all employees and managers (Permanent, full-time employees are defined as all employees that are employed for a term of one or more fiscal years and/or have a guaranteed renewal of their employment and that work a full shift)". Question l6: "How many full-time seasonal or temporary employees did this establishment employ during the last fiscal year? (Full-time, temporary workers are all short-term (i.e. for less than a year) employees with no guarantee of renewal of employment and work full-time)". Question l8: "What was the average length of employment of all full-time temporary employees in the last fiscal year?" N workers=ln(1+permanent+temporary). Source: WBES.
% unskilled workers	share of of unskilled workers. Question l4 of the WBES questionnaire: "At the end of last fiscal year, how many permanent, full-time individuals working in this establishment were: l4a1. workers in highly skilled production jobs, professionals whose tasks require extensive theoretical and technical knowledge; l4a2. workers in semi-skilled production jobs, technicians whose tasks require some level of mechanical or technical knowledge; l4b. workers in unskilled production jobs, whose tasks involve no specialized knowledge?" % unskilled workers=l4b/(l4a1+l4a2+l4b). Source: WBES.
Unit wage	log–average unitary wage. Unit wage=ln[1+ n2a/(permanent+temporary)]. Source: WBES.
Number competitors	number of competitors of the firm (quartiles). Question e2 of the WBES questionnaire: "In the last fiscal year, for the main market in which this establishment sold its main product, how many competitors did this establishment's main product face?". We build quartiles considering firms declaring that competitors are "too many to count" to be in the top quartile. Source: WBES.
Informal (dummy)	dummy for firms facing competition from informal firms. Question e30 of the WBES questionnaire: "To what degree are practices of competitors in the informal sector an obstacle to the current operations of this establishment?". Available options: i) no obstacle; ii) minor obstacle; iii) moderate obstacle; iv) major obstacle; v) very severe obstacle. Informal (dummy) =1 if e30= iv-v. Source: WBES.
Informal (intensity)	measure for the intensity of the perceived obstacles generated by informal competitors. Informal (intensity)$\in (0;5)$ and increases linearly with the possible answers of question e30 (whereby i=0 and v=5). Source: WBES.
Conflict exposure	log-number of conflict events (+1) that occurred within a 20 km radius around the firm's location during the 12 months preceding the closure of the last fiscal year. Sources: WBES (firms' geolocalization), ICEWS or ACLED (geolocalized conflict events).
Low income	dummy for firms located in low or lower-middle (per capita) income countries at the beginning of the sample period. Source: The World Bank.
High trade	dummy for firms located in countries with high engagement in international trade. The dummy takes the value of one if, in the first survey wave, the country's merchandise trade (as a share of GDP) is above the cross-country median. Data source: World Bank, https://data.worldbank.org/indicator/TG.VAL.TOTL.GD.ZS.
High agriculture	dummy for firms located in countries with a high share of agriculture in GDP. The dummy takes the value of one if, in the first survey wave, the country's value added from agriculture (as a share of GDP) exceeds the cross-country median. Data source: World Bank, https://data.worldbank.org/indicator/NV.AGR.TOTL.ZS.
High natural resources	dummy for firms located in countries with a high share of natural resources in GDP. The dummy takes the value of one if, in the first survey wave, the country's value added from natural resources (as a share of GDP) exceeds the cross-country median. Data source: World Bank, https://data.worldbank.org/indicator/NY.GDP.TOTL.RT.ZS.

Low economic complexity	dummy for firms located in countries with a high level of economic complexity. Countries are classified based on the Economic Complexity Index (ECI) (Hidalgo and Hausmann, 2009). The dummy takes the value of one if, in the first survey wave, the country's ECI score is above the cross-country median. The index is computed by the Harvard Growth Lab and is available for 133 countries from 1995 to 2021. Data source: https://doi.org/10.7910/dvn/xtaqmc.
Low bureaucracy quality	dummy for firms located in countries with low-quality bureaucracy. The International Country Risk Guide (ICRG) provides an index assessing whether the bureaucracy has the strength and expertise to govern without drastic changes in policy or interruptions in government services. See https://epub.prsgroup.com/products/icrg. Our measure takes the value of one if the country indicator of bureaucracy quality is below the median distribution, and zero otherwise. Source: ICRG.
High corruption	dummy equal to 1 for firms located in countries with high levels of corruption (in the first survey wave) as measured by the ICRG index. The index captures perceptions of corruption within the political system, assessing the extent to which public power is used for private gain —through bribery, patronage, nepotism, and undue influence of elites or private interests over public policy. Scores range from 0 to 6, with lower values indicating higher levels of corruption. The dummy takes the value of 1 if, in the first survey wave, a country's ICRG corruption score is below the cross-country median. For methodology details, see: https://www.prsgroup.com/wp-content/uploads/2022/12/icrgmethodology.pdf. Source: ICRG.
Fragile	dummy for firms located in fragile countries at the beginning of the sample period. Fragile states are those experiencing deteriorating governance, prolonged political crises, post-conflict transition, or gradual but still fragile reform processes. Source: The World Bank.
Illicit drug producing	dummy equal to 1 for firms located in countries classified as major illicit drug-producing and/or drug-transit countries. Classification is based on the *2006 International Narcotics Control Strategy Report*, published by the U.S. Bureau for International Narcotics and Law Enforcement Affairs. The report is available at: https://2009-2017.state.gov/j/inl/rls/nrcrpt/2006/vol1/index.htm. Among the countries in our sample, the list includes: Afghanistan, Bolivia, Colombia, Dominican Republic, Ecuador, Guatemala, Haiti, Lao PDR, Mexico, Nigeria, Pakistan, Panama, Paraguay, Peru, and Venezuela.
Initially at peace	dummy equal to 1 for firms located in countries that were not experiencing major episodes of political violence at the time of the first survey wave. Major political violence includes civil conflict, ethnic violence, riots, popular protests, and repression of dissidents. Countries are classified based on data from the Systemic Peace War dataset. The list of countries with major episodes of political violence from 1945 to 2019 is sourced from the Systemic Peace War dataset, available at: https://www.systemicpeace.org/warlist/warlist.htm. Major episodes involve at least 500 "directly related" fatalities and sustained violence (a base rate of 100 deaths per year). Countries intervening in foreign conflicts are excluded. We expand the list to include countries involved in Operation Juniper Shield, a counter-terrorism mission launched in 2007 targeting armed groups in the Saharan and Sahel regions. This includes Algeria, Burkina Faso, Cameroon, Chad, Mali, Mauritania, Morocco, Niger, Nigeria, Senegal, and Tunisia (OECD/SWAC, 2020).
Initially in conflict	dummy equal to 1 for firms located in countries experiencing major episodes of political violence at the time of the first survey wave. Classification is based on the same criteria and sources as described above. Countries classified as *initially in conflict* include: Afghanistan, Cameroon, Chad, Colombia, Democratic Republic of the Congo, Egypt, Ethiopia, Kenya, Morocco, Myanmar, Niger, Nigeria, Pakistan, Philippines, Russia, Tunisia, Turkey, Uganda, West Bank and Gaza, and Yemen.