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Macroeconomics of War and Recovery

Prepared by Hippolyte Balima, Colombe Ladreit, Andresa Lagerborg, Flavien Moreau, Andrea F. Presbitero and Evgenia Weaver

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Macroeconomics of War and Recovery*

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Abstract

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1 Introduction

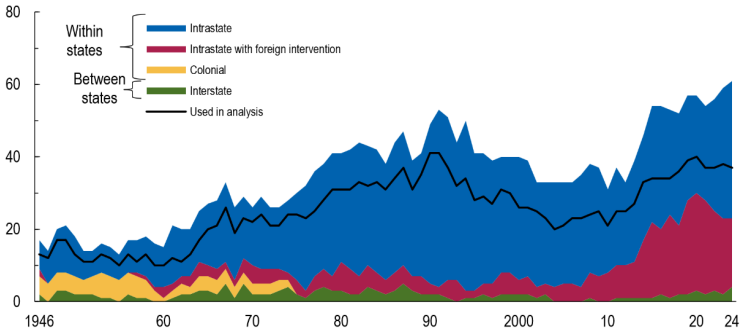
Conflicts have re-emerged as a defining feature of the global landscape. After decades of relative calm following the end of the Cold War, the number of active conflicts has surged to levels not seen since the end of World War II (Figure 1). The outlook offers little reassurance. Measures of geopolitical tensions continue to rise, pointing to elevated conflict risks, while defense spending is projected to increase across many countries. Beyond their immediate human costs, wars can carry profound macroeconomic consequences. The economic costs of wars at the conflict site are substantial, spill over to belligerent and third-party countries, and generate severe fiscal pressures for wartime governments (Chupilkin and Kóczán, 2022; Benmelech and Monteiro, 2025; Federle et al., 2026). Yet, important questions remain regarding how the macroeconomic effects of wars compare with those of other major shocks, how external-sector dynamics shape wartime adjustment, and how economies recover once conflicts end. Moreover, conventional growth theory suggests that the destruction of capital should generate rapid investment-led recoveries once peace is restored. Yet many post-conflict recoveries appear surprisingly slow. Why?

This paper provides a unified analysis of the macroeconomics of war and recovery, with particular emphasis on external-sector adjustment during conflict—considering key balance of payments accounting principles, and factor reallocation after conflict termination. We assemble a global dataset covering 194 countries over the period 1946–2024, including 170 conflict onsets and 158 conflict terminations in conflict-site economies. To distinguish conflict-site economies from belligerent countries, we combine existing sources with large language models (LLMs) and manual validation aimed at improving the precision of conflict locations. Given our focus on macroeconomic adjustment and balance-of-payments dynamics, we also use LLMs to identify a subset of macro-critical conflicts—those most likely to generate aggregate macroeconomic disruptions. This refinement allows us to focus on conflicts that are more likely to trigger external-sector adjustments and policy responses aimed at managing the associated balance-of-payments pressures.

Given the central role of external-sector adjustment in our narrative, it is useful to illustrate these dynamics with a concrete example. The Russian invasion of Ukraine in February 2022 provides a case in point. At the outbreak of the war, the National Bank of Ukraine swiftly pegged the hryvnia's exchange rate and imposed extensive foreign exchange and capital controls to preserve reserve buffers, channel foreign exchange toward priority imports, and reduce uncertainty regarding the value of the currency (IMF, 2022a; Becker et al., 2022).[1] Nonetheless, war-induced uncertainty quickly led to a loss of market access and a sharp decline in private capital inflows, forcing the government to rely increasingly on countercyclical external financing. In particular, aid, grants and remittances—recorded primarily in the primary and secondary income components of the current account—contributed to a temporary current account surplus in 2022 despite a sharp collapse in goods exports caused by disruptions to logistics and domestic productive capacity (National Bank of Ukraine, 2022). As these exceptional inflows moderated, the current account subsequently returned

[1] See also the National Bank of Ukraine webpage: https://bank.gov.ua/en/markets/liberalization.

Figure 1: Global Post-WWII Conflicts



Note: The chart shows the number of active conflicts per year, aggregated by conflict type. The black line indicates the subset of conflicts of focus in the chapter's analysis—those deemed more likely to affect macroeconomic outcomes based on a systematic reading and coding of individual events, as discussed later in Section 2 of the paper.

to deficit (IMF, 2022a; IMF, 2022b; IMF, 2026b).

These developments generated growing tensions in both official and parallel foreign exchange markets, forcing the central bank to sell FX to defend the value of the hryvnia (Gorodnichenko, 2022). Pressures on the hryvnia and reserve buffers however intensified through mid-2022, with international reserves declining by US$2.3 billion despite a cumulative 1,500-basis-point monetary policy rate hike (IMF, 2022b; IMF, 2026b). These pressures ultimately prompted the central bank to devalue the exchange-rate peg by 25% in July 2022. Yet even after the devaluation, the gap between the official and parallel exchange rates widened substantially, with the premium averaging about 9.4% between August and December 2022, compared with only 0.2% in February (Ilzetzki et al., 2019).[2] These external-sector pressures likely contributed to rising inflation and further weakened domestic demand.[3] A central objective of this paper is to determine whether these external-sector dynamics are unique to Ukraine or instead constitute a more systematic amplification channel of macroeconomic challenges during wartime.

Our empirical strategy at the macro level builds on recent developments in local-projection difference-in-differences (LP-DiD) methods (Dube et al., 2025; Rinner et al., 2026), which, to our knowledge, has not yet been applied to study conflicts, where heterogenous treatment timing can be an important challenge. The framework combines standard local projections à la Jordà (2005) with clean-control restrictions to trace the dynamic evolution of macroeconomic aggregates around conflict onset and termination. Relative to conventional local projections and standard difference-in-differences estimators, LP-DiD is particularly well-suited to settings characterized by heterogeneous treatment timing, in which countries enter and exit treatment status at different points in time. In the context of conflicts, heterogeneous timing creates two important challenges. First, treated

[2]We thanks Carmen Reinhart for kindly sharing their updated parallel exchange rate dataset.

[3]For instance, Oki and Bello (2026) show that in economies with dual exchange rates, prices respond strongly to movements in the parallel exchange rate but only weakly, if at all, to changes in the administered official rate.

observations may be compared primarily to never-treated countries that differ systematically from conflict-affected economies, potentially biasing estimated treatment effects *upward*.[4] Second, treated observations may be compared to countries that experience conflict within the evaluation window, potentially biasing estimated effects *downward* by contaminating the control group. Our LP-DiD framework addresses these concerns by comparing newly treated observations—defined by either conflict onset or conflict termination—only to valid control observations within a specified event window. By restricting comparisons to clean controls, the approach improves identification of the conflict shock and complements recent empirical strategies by Benmelech and Monteiro (2025) and Federle et al. (2026).

To better understand how economies recover after conflicts end, we further complement the macro-level analysis with firm-level evidence. Specifically, we construct geocoded firm-level data from *Orbis* and match them to geocoded conflict events, allowing us to study firm adjustment during the post-conflict period. Beyond shedding light on the mechanisms underlying recovery, the firm-level analysis provides an additional source of identification by exploiting variation in post-conflict exposure across firms within the same country and year while controlling for firm-, sector-year, and country-year factors. This helps mitigate concerns that the aggregate post-conflict recovery patterns are driven by country-level confounders or compositional effects, especially the endogenous selection of countries into post-conflict settings. Moreover, it allows us to assess whether the recovery observed at the macro level is also present at the micro level and to explore competing explanations for the post-conflict recovery.

Our results add to the growing literature on the macroeconomics of conflict. We begin by confirming previous findings that wars generate large and persistent output losses in conflict-site economies and spill over to neighboring and trading partners. On average, output in conflict-site economies declines sharply at conflict onset—by approximately 3%—and continues to fall in subsequent years, reaching cumulative losses of about 7% within five years. Losses tend to be larger for major, shorter, and within-state conflicts, and remain visible even after a decade. These findings are broadly consistent with those reported in previous studies (Chupilkin and Kóczán, 2022; Benmelech and Monteiro, 2025; Federle et al., 2026). Our estimates of output losses from conflicts are however somewhat smaller in absolute value—roughly half the magnitude of those reported by Benmelech and Monteiro (2025) at comparable time horizon. One possible explanation is methodological, considering that the LP-DiD framework helps mitigate concerns arising from heterogeneous treatment timing.

Our analysis also uncovers four novel findings. First, we use the LP-DiD framework to benchmark the output costs of conflicts against those associated with financial crises—banking, currency, and sovereign debt crises—as well as severe natural disasters. Such comparisons are particularly informative, given that all shocks are estimated using the same empirical framework and identification strategy, thereby providing a unified benchmark for assessing their relative macroeconomic costs.

[4] Rinner et al. (2026) discusses a related issue in the context of IMF program evaluation.

We find that output losses following conflicts typically exceed those observed after financial crises or severe natural disasters. Notably, even lower-intensity conflicts generate output losses of magnitude comparable to those associated with currency crises, while major conflicts substantially exceed the costs of all other shocks considered.

Second, our estimates highlight how external-sector dynamics amplify wartime macroeconomic challenges, suggesting that Ukraine's experience is closer to the rule than the exception. Focusing on major and macro-critical conflicts, we find that output losses are accompanied by sustained contractions in investment and private consumption, while government consumption remains broadly stable, consistent with a reallocation toward defense spending. Imports contract sharply, but exports decline even more, resulting in a deterioration of the trade ratio. Consistent with these dynamics, the trade balance relative to prewar GDP initially widens. This widening, however, is confined to the first four years of conflict, pointing to subsequent import compression as foreign exchange reserves become increasingly scarce. At the same time, gross national savings decline by more than investment, consistent with the accounting identity linking domestic investment to available domestic and foreign savings. Heightened uncertainty triggers capital outflows, with both foreign direct investment and portfolio flows declining, thereby forcing wartime governments to rely increasingly on countercyclical sources of external financing, particularly aid. Governments also respond to these pressures by tightening capital controls. Despite these measures, wartime dynamics contribute to sustained exchange-rate depreciation, declining reserve buffers, and rising inflationary pressures. Consumer prices rise steadily, reaching roughly 35% above prewar levels five years after conflict onset, prompting monetary authorities to increase short-term nominal policy rates. Pressures in official foreign exchange markets and growing reserve scarcity also contribute to the emergence of dual-rate systems, with the premium between parallel and official exchange rates almost doubling within five years.

Third, we turn to conflict termination to examine how economies recover once wars end. Standard neoclassical growth models predict that capital destruction raises the marginal product of capital, suggesting an investment boom and a capital-led recovery following conflict termination. Our findings provide little support for this prediction. Instead, post-conflict recoveries are typically slow, uneven, and critically dependent on the durability of peace. In cases where countries relapse into conflict, output fails to recover. By contrast, when peace is sustained, output gradually rebounds, but remains modest relative to wartime losses. Importantly, these recoveries are primarily labor-led, while capital accumulation and productivity remain subdued.

Fourth, empirical findings based on the geocoded firm-level data reinforce the aggregate finding that recoveries are primarily labor-led. On average, surviving firms expand employment modestly after conflict ends, while capital stocks remain depressed and productivity exhibits little improvement. At the same time, adjustment dynamics vary considerably across sectors and firm characteristics. Firms operating in capital-intensive sectors, exporters, and those with stronger balance sheets experience stronger gains in employment and productivity, whereas financially constrained firms

face persistent capital shortfalls. These findings suggest that lingering uncertainty and financing constraints hinder capital rebuilding in the aftermath of conflict, slowing the recovery of productive capacity.

The remainder of the paper proceeds as follows. The rest of this section discusses the paper's relationship to the existing literature. Section 2 describes the data. Section 3 studies the macroeconomic effects of conflicts, while Section 4 examines macroeconomic effects following conflicts termination. Section 5 turns to firm-level evidence on post-conflict adjustment. Section 6 concludes.

Related literature.—Our work relates to two broad strands of the literature. A first strand examines the macroeconomic effects of conflict onset. This literature documents sizable output losses (Barro and Lee, 1994; Abadie and Gardeazabal, 2003; Acemoglu et al., 2005; Chupilkin and Kóczán, 2022; Benmelech and Monteiro, 2025; Federle et al., 2026; Gorodnichenko and Vasudevan, 2025), significant spillovers to belligerent and third countries (Novta and Pugacheva, 2021; Brunet, 2024; Federle et al., 2026), and important fiscal and monetary implications of wars (Gupta et al., 2002; Besley and Persson, 2008; Barrett, 2022; Benmelech and Monteiro, 2025). Within this strand, Cerra and Saxena (2008) document large output losses from civil wars but find them to be less persistent than those associated with banking and currency crises. By contrast, Mueller (2012) argues that, after correcting for measurement issues in Cerra and Saxena (2008), civil wars are substantially more damaging. A second strand focuses on post-conflict recovery dynamics (Collier and Hoeffler, 2004; Chen et al., 2008; David et al., 2011; Mueller et al., 2017), emphasizing the heterogeneity of recovery paths and the role of institutions, aid, and policy frameworks.

Contribution to the literature.—Our paper is closely related to recent contributions by Federle et al. (2026) and Benmelech and Monteiro (2025). Federle et al. (2026) examine the economic costs of wars using a long historical sample spanning 150 years and covering 60 countries, a much smaller sample compared to our study. Their local projection analysis focuses primarily on output losses in conflict-site economies, belligerents, and third countries. Benmelech and Monteiro (2025) study a sample of 145 countries using a difference-in-differences framework and primary emphasize the role of government budget constraints, inflationary finance, and domestic financial frictions in shaping wartime outcomes. Building on these contributions, our paper broadens the analysis of wartime adjustment and post-conflict recovery in four key ways. *First*, we place the macroeconomic costs of wars in comparative perspective by estimating the effects of conflicts, banking crises, currency crises, sovereign debt crises, and severe natural disasters within a common LP-DiD framework. This provides a unified benchmark for assessing the economic costs of wars relative to other major macroeconomic shocks. *Second*, motivated by the Ukraine example, we provide a comprehensive characterization of wartime external-sector adjustment. Whereas existing work has focused primarily on output losses, spillovers, government's budget constraint and domestic financial frictions, we jointly study current-account dynamics, the interaction between domestic and external savings, capital flows, official and parallel FX markets, and FX reserves dynamics, together with policymakers' attempts to contain the balance-of-payment pressures. In doing so, we show how external-sector

dynamics amplify wartime macroeconomic challenges and shape the adjustment process during conflicts.[5] *Third*, we study recovery after conflict termination as a distinct macroeconomic phase. In contrast to most existing work, which focuses primarily on the consequences of conflict onset, we distinguish between recoveries under sustained peace and those interrupted by conflict relapse, and decompose recovery dynamics into the contributions of labor, capital, and productivity. *Fourth*, we complement the aggregate analysis with geocoded firm-level evidence. This allows us to connect macroeconomic recovery patterns to underlying microeconomic adjustment mechanisms and to examine how post-conflict environments shape employment, capital accumulation, and productivity dynamics across firms. Taken together, these contributions shift the focus from the economic costs of war alone to the broader macroeconomic adjustment process during war and the constraints that shape recovery once wars end.

2 Data

Our primary conflict data source is the UCDP/PRIO Armed Conflict Dataset (Davies et al., 2025; Gleditsch et al., 2002). The dataset covers state-based conflicts with at least 25 battle-related deaths over the period 1946–2024, and includes interstate, intrastate with and without foreign interventions, and extra-systemic conflicts. The dataset further reports the two main sides of each conflict, as well as parties providing support to either side. Although a conflict location is reported, it does not always correspond to the precise geographic areas where hostilities occurred. In some cases, the reported location corresponds to the government side of the conflict rather than to the geographic area in which hostilities took place (Pettersson, 2025).[6]

We use LLMs to achieve two primary objectives. A first objective is to accurately distinguish conflict-site economies from other belligerents that participate in the conflict without hosting it on their own territory. We prompt ChatGPT to identify the precise geographic location of each conflict event in a given year, therefore allowing to single out the conflict-site economies from the belligerent economies.[7] The responses from ChatGPT were further reviewed manually, verifying that location is based on modern-day country borders and that various cases (such as cross-border skirmishes or global anti-terrorist operations) are treated consistently. As a further check, the location was

[5] Benmelech and Monteiro (2025) also look at some external sector indicators, but their story is much less incomplete compared to ours, which focuses on the broader external sector dynamics from a balance of payments standpoint, while keeping in mind key accounting identities.

[6] For example, in the conflict between the Government of Egypt and the Government of the United Kingdom over the Suez Canal in 1951–52, the location is recorded as "Egypt, United Kingdom."

[7] For each conflict-year, the following question was posed to ChatGPT version 4.1 with a temperature setting of 0.3 (for more focused and factual answers): "You are an expert in history and geography. You will be given a description of a conflict between two parties in a given year. Reply whether the provided location of the conflict is geographically accurate (the events or battles took place on the country's soil). Provide information only for the exact year asked, even if the conflict lasted over several years. Sometimes you will be given several countries for location, provide information for each of them. Provide additional comments to justify your answer. Example: In the conflict between the Government of Egypt and the Government of United Kingdom over Suez in 1951 is the following geographical location of the conflict accurate: Egypt, United Kingdom?"

compared to the UCDP Georeferenced Event Database (GED) that has precise location data over 1989–24, focusing on state-based conflicts with at least 25 deaths in a given year. This procedure allows us to improve the precision of conflict locations relative to the primary data source. For instance, the match in locations reported in the annual UCDP/PRIO dataset and the GED dataset was improved by 5%. All in all, compared to the original UCDP/PRIO dataset, 11% of the location entries were modified to better reflect conflicts' actual geographical location.

Second, given the paper's focus on macroeconomic adjustments during conflicts, a key challenge is to focus on conflicts likely to be macro-critical. For that purpose, we again prompt ChatGPT on each conflict episode to further exclude from the conflict sample those events unlikely to generate aggregate effects at the macro level. We exclude events such as isolated skirmishes, localized terrorist attacks, or highly fragmented violence with very low intensity or very marginal economic disruptions. We again cross-check ChatGPT coding through a systematic reading of each individual conflict description.[8] This refinement, in spirit, is broadly consistent with standard practice in the literature, which frequently relies on casualty thresholds to define wars (such as a 1,000 battle-related deaths in Federle et al. (2026)). At the same time, rather than relying mechanically on fatality counts alone, our approach allows us to include some conflicts that may seem marginal based on battle-related deaths, but can nonetheless trigger sizable macroeconomic disruptions. Robustness checks make use of the full conflict sample for comparison.

[8]For each conflict, the following prompt was posed to ChatGPT (version 5.1) with a temperature setting of 0.3 to elicit focused and factual responses: "You are an expert in history and wars. You are given a description of a conflict, including information on participants, start and end year, and location. Provide a description of the conflict and indicate whether the conflict had a large impact on the economy." Explanations of the reasons behind the classification were also extracted along with a link to the source of information used. The model outputs were manually reviewed for each conflict to verify accuracy and ensure consistency in classification across episodes.

Table 1: Example of Mapping Between Conflict-Level Data and Country-Year Panel

	Separate Conflict IDs					*Combined*	
Year	13219	13243	13246	13247	13306	All Conflict	Major Conflict
2014	Minor		Major	Major	Major	1	1
2015					Major	1	1
2016					Minor	1	0
2017					Minor	1	0
2018					Minor	1	0
2019					Minor	1	0
2020					Minor	1	0
2021					Minor	1	0
2022		Major			Minor	1	1
2023		Major				1	1
2024		Major				1	1

Note: The table illustrates how multiple conflict-level entries from the UCDP/PRIO dataset are aggregated into a single country-year indicator. The example corresponds to conflicts in which Ukraine is identified as a conflict site. "Minor" indicates conflicts with 25–999 battle-related deaths; "Major" indicates conflicts with 1,000 or more battle-related deaths.

The assembled conflict-level data are transformed into a country-year panel in which an indicator variable takes the value of 1 if a conflict occurs on a country's territory (the "conflict-site") in a given year, and 0 otherwise. Table 1 illustrates this mapping. Separate indicators are constructed for minor (with 25-999 battle-related deaths) and major (with 1,000 or more battle-related deaths) conflicts, as well as for conflicts between states (interstate) and within states (including intrastate, intrastate with foreign intervention, and extrasystemic conflicts). Belligerent countries are treated analogously: an indicator variable takes the value of 1 if a country participates in a conflict in a given year but the conflict does not occur on its territory, and 0 otherwise.

Our assembled data on conflicts is complemented with other macroeconomic data. The primary sources of macroeconomic data are the Global Macro Database (Müller et al., 2025), the IMF's World Economic Outlook database (October 2025 vintage), the World Bank's World Development Indicators database, and the Penn World Table. When the same concept is available from multiple sources, data are combined to improve country and year coverage. Series are reviewed to eliminate structural breaks, and for most variables only the most recent continuous vintage is used to minimize data gaps. Appendix A1 presents the data sources used in the paper, while Appendix A2 reports the list of countries included in the various parts of the paper's analysis, including the onset of conflict and postconflict episodes.

3 Macroeconomic effects of conflicts

This section estimates the macroeconomic effects of conflicts. After describing our empirical approach, we present the average and heterogeneous output costs of conflicts and how these compare to other shocks, before moving to document the associated spillovers and external sector-related macroeconomic challenges.

3.1 Empirical strategy

We estimate the impact of conflict onset using the LP-DiD approach proposed by Dube et al. (2025). The core feature of this approach is that it combines the flexibility of local projections (Jordà, 2005) with clean-control restrictions. This limits estimation sample to treated observations (countries experiencing conflict onset) and unaffected observations, or *clean controls*, that are unlikely to be affected by conflict in a defined window around the treatment. The clean-control restriction allows to exclude country-year observations that may still be subject to lingering effects of past conflicts, ensuring that such observations are not used as comparators to treated observations in the regressions.

Specifically, for horizons $h = 0, \ldots, 5$, we estimate the following equation:

$$\ln(y_{i,t+h}) - \ln(y_{i,t-1}) = \beta^h \, \text{ConflictOnset}_{i,t} + \delta^h \, X_{i,t} + \theta_t^h + \mu_i^h + \varepsilon_{i,t}^h, \tag{1}$$

where the left-hand side denotes cumulative growth of the outcome variable y for country i between year $t-1$ and $t+h$. The indicator $\text{ConflictOnset}_{i,t}$ equals one in the first year a country becomes a conflict site, provided that the preceding five years were conflict-free, i.e.,

$$\Delta\text{Conflict}_{i,t} = 1 \quad \text{and} \quad \text{Conflict}_{i,t-j} = 0 \quad \text{for} \quad 1 \leq j \leq L,$$

with $L = 5$, and zero otherwise. The vector $X_{i,t}$ includes three lags of the dependent variable. The terms θ_t^h denote year fixed effects, controlling for global shocks common to all countries, while μ_i^h are country fixed effects that absorb time-invariant country characteristics. $\varepsilon_{i,t}^h$ denotes the error term. The clean controls restriction imposes that a country-year observation is not exposed to conflict within a symmetric window around treatment, that is,

$$\text{Conflict}_{i,t-j} = 0 \quad \text{for} \quad -h \leq j \leq L,$$

here to, with $L = 5$ in the baseline specification. The identifying assumption is that, conditional on fixed effects and controls and within the clean-control sample, conflict onset is as-good-as-random with respect to the evolution of outcomes absent treatment. While conflicts are clearly not random events, the clean-control restriction and fixed-effects structure help mitigate biases arising from persistent differences between conflict-prone and other countries, as well as contamination from conflict episodes over time. We further probe our identification strategy holds through robustness

checks that extend the clean window (from $L = 5$ to $L = 10$), exclude conflicts motivated by economic considerations based on a narrative classification of conflicts' motives, and apply alternative conflict definitions based on battle-deaths-per-capita thresholds.

3.2 Main Results

Baseline findings.—We begin by presenting baseline estimates of the dynamic effects of conflicts on output using the LP-DiD framework. The results, reported in Figure 2a, indicate that conflicts impose large and persistent economic costs on conflict-site economies, with losses that deepen over time. In the baseline specification, output declines immediately at conflict onset—by about 3%—and continues to fall in subsequent years, reaching cumulative losses of roughly 7% within five years. Additional results presented in the robustness section indicate that these losses do not dissipate even after a decade, cumulating to approximately 11% after ten years. These findings suggest that wars operate as shocks that damage productive capacity and leave lasting scars on the economy. The estimated losses are somewhat smaller in absolute value—roughly half the magnitude of those reported by Benmelech and Monteiro (2025) at comparable time horizon. One possible explanation is methodological. By restricting comparisons to clean-control observations, our LP-DiD framework helps mitigate concerns arising from heterogeneous treatment timing and from comparisons between conflict-affected countries and never-treated economies. As such, the smaller estimates are consistent with the possibility that conventional difference-in-differences specifications could overstate the economic costs of conflicts.

Heterogeneity of conflict effects.—We examine heterogeneity in the output effects of conflict along three dimensions: conflict type (interstate versus within-state conflicts), duration (short versus long conflicts), and intensity (minor versus major conflicts). Our definition of within-state conflicts includes civil wars and colonial conflicts. Minor conflicts are defined as those involving 25–999 battle-related deaths, while major conflicts involve 1,000 or more battle-related deaths. Short conflicts last two years or less, whereas long conflicts persist for more than two years. The results, reported in Figures 2b-2g, indicate that output losses are broad-based across all conflict characteristics. Nevertheless, the magnitude and persistence of losses vary systematically across conflict types. Major conflicts generate the largest output losses, while minor conflicts still produce statistically significant declines at short horizons but effects tend to dissipate after two years. Output losses also tend to be larger for short conflicts than for long conflicts. One possible interpretation is that shorter conflicts may be associated with more intense episodes of destruction concentrated over a limited period, whereas economies exposed to prolonged conflicts may partially adapt to persistent conflict conditions. Finally, losses are more persistent following within-state conflicts than interstate conflicts, consistent with the view that civil wars and related forms of internal violence inflict deeper and more lasting damage on domestic institutions, productive capacity, and economic activity. Taken together, these findings suggest that even relatively limited conflicts impose meaningful macroeconomic costs, while severe and domestically rooted conflicts generate

particularly large and persistent output losses.

Comparison to other shocks.—We next benchmark the output costs of conflicts against those associated with banking, currency, and sovereign debt crises, as well as severe natural disasters, using the same LP-DiD framework. Banking, currency, and sovereign debt crises, as well as severe natural disasters, are defined using standard classifications from the literature.[9] The results, reported in Figures 3b–3e, indicate that output losses associated with conflicts exceed those observed following banking, currency, and sovereign debt crises, as well as severe natural disasters. The comparison is particularly informative because all shocks are estimated using the same empirical framework and identification strategy, thereby providing a unified benchmark for assessing their relative macroeconomic costs. Notably, even lower-intensity conflicts generate output losses comparable in magnitude to those associated with currency crises, while major conflicts substantially exceed the costs of all other shocks considered. Nonetheless, our estimates obtained within the LP-DiD framework are not directly comparable to those derived using alternative methodologies. It is nevertheless reassuring that the magnitudes reported here are broadly consistent with previous findings in the literature.[10]

3.3 Spillovers and Macroeconomic Dynamics

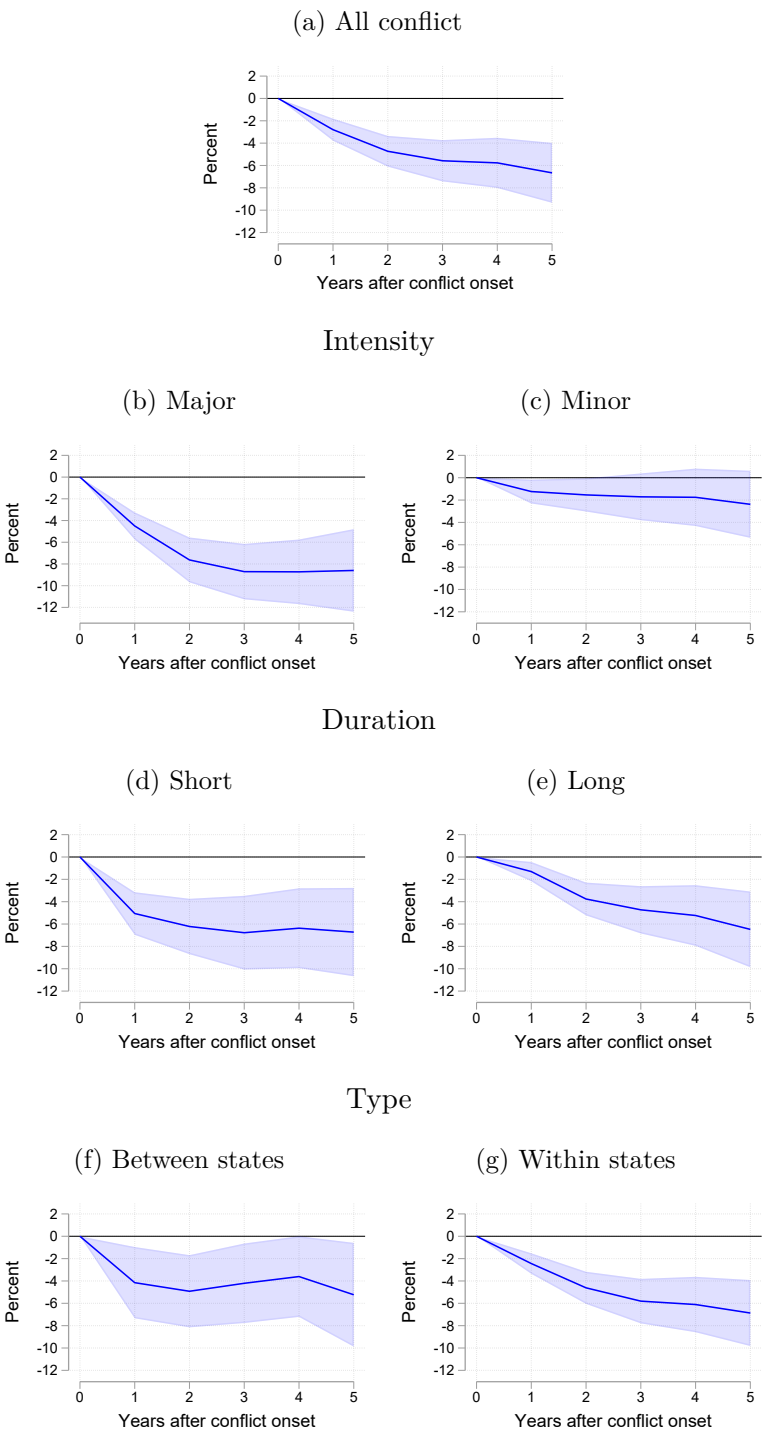
Given that output losses in conflict-site economies are substantially larger for major conflicts than for minor ones—and that losses from minor conflicts tend to dissipate after two years—we focus in this section on major conflicts. This allows us to examine their spillovers to other countries and, more importantly, to analyze the macroeconomic dynamics they generate, with particular emphasis on the external sector and the policy responses deployed to contain the associated pressures.

Spillovers.—The economic consequences of conflicts extend beyond conflict-site economies and spill over to other countries. Here, we focus on two groups: belligerents and third countries. Belligerents are defined as parties to a conflict whose territory is not the conflict site, while third countries are economies that are not parties to the conflict but are indirectly exposed through geographic proximity or trade linkages. We define neighboring countries using shared land border with a conflict-site economy, and trade partners as countries whose share of imports from the conflict-site economy exceeds the 90th percentile of the distribution. Focusing on major conflicts, the estimates reported in Figure 4 indicate that output effects for belligerents are negative on average but not statistically significant. One possible explanation is that, while belligerents may bear

[9] Specifically, a banking crisis is defined as the combination of significant financial distress (bank runs, insolvencies, or liquidations) and major policy interventions (bank rescues, closures, or deposit freezes). A currency crisis is defined as a nominal depreciation vis-à-vis the U.S. dollar of at least 30% that is also at least 10 percentage points larger than the rate of depreciation in the previous year. A sovereign debt crisis occurs when a country fails to meet principal and/or interest payments when due or restructures its debt on less favorable terms. Following Cavallo et al. (2013), a major natural disaster is defined as an event in which the share of the population killed exceeds the 99th percentile of the global distribution.

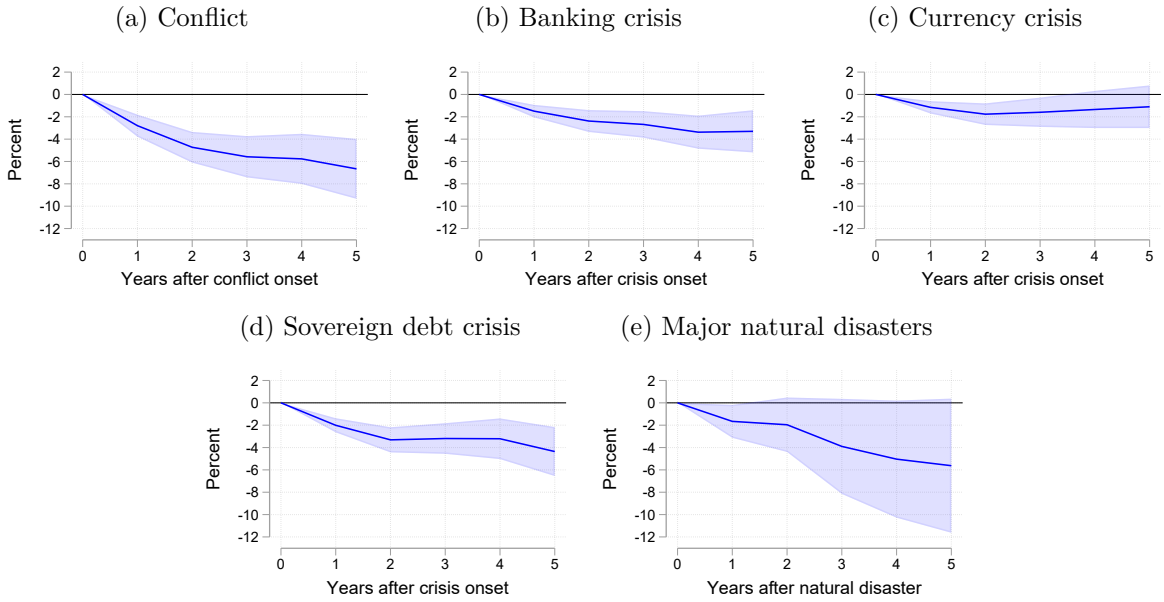
[10] See, for example, Kuvshinov and Zimmermann (2019) for sovereign defaults; Devereux and Dwyer (2016) for banking crises; Hong and Tornell (2005) for currency crises; Cavallo et al. (2013) for natural disasters; and Cerra and Saxena (2008) and Mueller (2012) for related comparisons between civil wars and currency/banking crises.

Figure 2: Baseline result and heterogeneity of conflict effects on output



Note: The figure presents LP-DiD estimates of the effect of conflict on output, reporting the baseline specification and heterogeneity by conflict intensity, duration, and type. The dependent variable is the cumulative change in output up to horizon h, where $h = 1$ corresponds to the year of conflict onset. Major conflicts involve at least 1,000 battle-related deaths; minor conflicts involve 25–999. Short conflicts last up to two years; long conflicts last longer. Between-state conflicts are interstate; within-state include intrastate and related types. The shaded areas indicate 90% confidence intervals. The corresponding regression results are reported in Table A3.

Figure 3: Comparison to other shocks

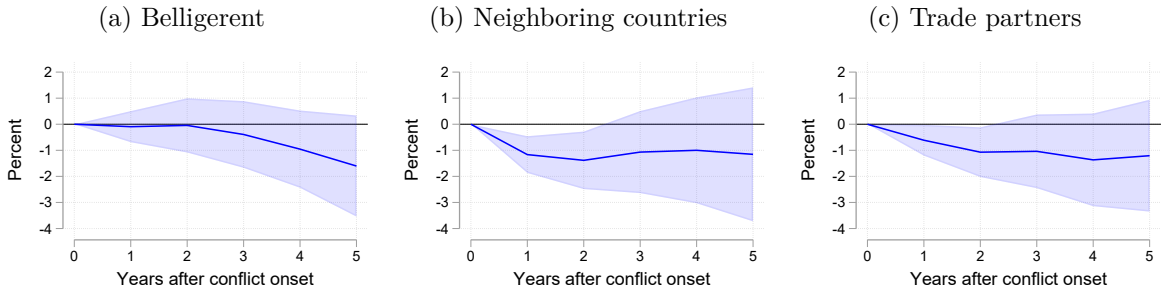


Note: The figure presents LP-DiD estimates of the effects of conflict and other shocks (banking, currency, and sovereign debt crises, as well as severe natural disasters) on output. The dependent variable is the cumulative change in output up to horizon h, where $h = 1$ corresponds to the year of conflict onset or the onset of other shocks. Definitions of banking, currency, and sovereign debt crises follow standard dummy-variable classifications used in the literature. Major natural disasters are identified when disaster-related deaths exceed the 99th percentile. The shaded areas indicate 90% confidence intervals. The corresponding regression results are reported in Table A4.

some of the economic costs of war, they are less directly exposed to the destruction of productive capacity occurring at the conflict site and may simultaneously experience offsetting demand effects associated with higher military spending. By contrast, both neighboring countries and trade partners experience modest but statistically significant output losses of about 1% or less during the first two years of conflict. These losses gradually dissipate thereafter, consistent with adjustment mechanisms such as reorientation of trade routes, adaptation of supply chains, and policy responses aimed at mitigating the initial conflict-exposure disruptions (see also Qureshi, 2013).

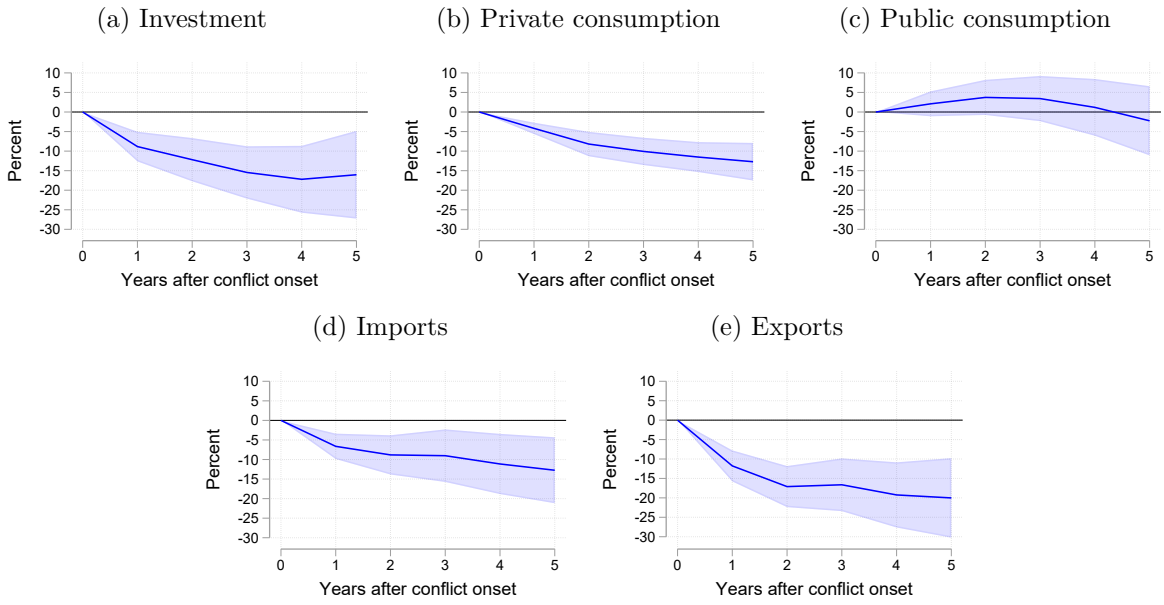
Macroeconomic dynamics.—We next examine the macroeconomic dynamics generated by major conflicts, with a particular focus on external-sector adjustment. To better understand the nature of the wartime shock and its implications for the balance of payments, we begin by analyzing expenditure-side dynamics in Figures 5a–5e, before turning to a broader set of external-sector indicators reported in Table 2. The estimates point to pronounced macroeconomic adjustments in conflict-site economies. On the expenditure side, output losses are driven by sustained contractions in both investment and private consumption, while government consumption remains broadly stable (Figures 5a–5c). This muted response of public consumption likely reflects a compositional shift toward defense-related expenditures, consistent with evidence that military spending rises sharply

Figure 4: Output cost spillovers from major conflicts



Note: The figure presents LP-DiD estimates of the spillover effects of major conflict on output for belligerent countries, neighboring countries, and trade partners. The dependent variable is the cumulative change in output up to horizon h, where $h = 1$ corresponds to the year of conflict onset. Belligerent countries are defined as those that participate in a conflict without experiencing hostilities on their own territory. Neighboring countries are those sharing a land border with a conflict-site economy. Trade partners are defined as countries whose import share from conflict-site economies exceeds the 90th percentile of the distribution. The shaded areas indicate 90% confidence intervals. The corresponding regression results are reported in Table A5.

Figure 5: Effects of major conflicts on demand-side of output



Note: The figure presents LP-DiD estimates of the effect of conflict on the demand-side components of output. The dependent variable is the cumulative change in each component up to horizon h, where $h = 1$ corresponds to the year of conflict onset. The regressions control for three lags of the dependent variable and include country and year fixed effects. The shaded areas indicate 90% confidence intervals. The corresponding regression results are reported in Table A6.

following conflict onset (IMF, 2026a).[11] The contraction in domestic demand is accompanied by

[11]Despite the relative stability of government consumption, additional results not reported here indicate that fiscal

substantial external adjustment. Imports decline sharply following conflict onset but exports fall by even more (Figures 5d–5e), resulting in a deterioration of the trade ratio defined here as exports-to-imports ratio (column 1 of Table 2). Consistent with these developments, the trade balance relative to prewar GDP initially widens (column 2). However, this widening is confined to the first four years of conflict, suggesting that wartime economies subsequently compress imports in response to increasing foreign-exchange scarcity and limited access to external financing. Financial-account dynamics reinforce these pressures. Heightened uncertainty triggers capital outflows, with both foreign direct investment and portfolio flows declining significantly (columns 5-6). As private external financing becomes scarce, governments increasingly rely on official development assistance and, to a lesser extent, remittances to finance external deficits, although the estimates for remittances are less precisely measured (columns 3-4). At the same time, gross national savings decline by more than investment, consistent with the accounting identity linking domestic investment to available domestic and foreign savings (column 7). Taken together, these patterns suggest that wartime economies face a dual challenge: a collapse in external earnings and a reduction in access to private external financing. Governments respond to these pressures through a range of policy measures. Capital controls tighten as authorities attempt to contain capital flight and preserve foreign-exchange reserves (column 8). Nevertheless, reserves continue to decline and official exchange rates depreciate persistently (columns 9-10). These external pressures are accompanied by rising inflation, with consumer prices reaching roughly 35% above prewar levels five years after conflict onset (column 11). In addition, foreign-exchange shortages give rise to parallel exchange-rate markets, with the premium of the parallel rate over the official rate increasing by 94% within five years (column 12). Monetary authorities respond to these developments by raising short-term nominal policy rates (column 13). Taken together, these findings suggest that wars generate much more than a decline in output. They trigger broad balance-of-payments pressures that propagate through the current, capital, and financial accounts, forcing governments to rely on a combination of exchange-rate adjustment, reserve losses, capital-flow management measures, and official external financing. In this sense, external-sector dynamics do not merely accompany wartime economic distress; they constitute a central mechanism through which conflicts amplify macroeconomic challenges.

3.4 Robustness checks

We assess the robustness of the baseline output effects of conflicts along several dimensions and report the results in Table A7. The specifications are designed to evaluate the sensitivity of the baseline findings to alternative treatment definitions, clean-control restrictions, conflict classifications, and sample selections.

Column (1) estimates the cumulative effect of conflict over a 10-year horizon while maintaining the baseline definition of conflict onset (that is, no conflict in the preceding five years). Column (2)

positions deteriorate, with public debt increasing during the initial years of conflict. This reflects both weaker fiscal balances and denominator effects associated with lower output.

Table 2: External sector dynamics and policy responses

	Trade-ratio	Trade Balance	Remittances	ODA	FDI	Portfolio	Savings	Fin. Open.	Exchange Rate	Reserve	CPI	Parallel premium	Policy rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
h = 1	-5.946***	-0.946	3.446	11.848*	-3.610**	-23.957**	-7.031	-0.012	-4.664***	-13.215*	1.252	0.337***	-0.588
	(1.774)	(0.958)	(7.539)	(7.048)	(1.651)	(9.260)	(5.041)	(0.010)	(1.570)	(7.333)	(1.210)	(0.110)	(0.834)
h = 2	-8.393***	-3.420**	-1.082	17.818*	-9.240***	-57.703***	-10.878*	-0.031**	-7.327**	-26.509***	8.789***	0.445**	1.392
	(2.539)	(1.401)	(11.131)	(9.665)	(2.970)	(21.408)	(6.422)	(0.013)	(2.957)	(8.111)	(2.256)	(0.189)	(0.933)
h = 3	-7.104**	-2.230*	7.595	21.389*	-14.990***	-81.091**	-14.688*	-0.046**	-13.288***	-26.813**	15.170***	0.697***	1.520
	(3.275)	(1.248)	(14.994)	(11.760)	(3.995)	(36.689)	(8.719)	(0.021)	(4.787)	(12.603)	(4.248)	(0.240)	(1.700)
h = 4	-8.130*	-0.399	13.237	31.369**	-22.816***	-77.762**	-16.895*	-0.036	-16.844***	-31.643	23.386***	0.775***	3.879*
	(4.269)	(1.803)	(16.479)	(14.102)	(5.599)	(31.070)	(9.063)	(0.029)	(6.445)	(20.331)	(6.467)	(0.251)	(2.093)
h = 5	-9.345*	0.448	25.564	43.303***	-18.697*	-31.609	-19.020**	-0.033	-20.233***	-36.405	32.673***	0.941***	4.931**
	(5.577)	(2.152)	(17.882)	(16.465)	(9.672)	(32.368)	(9.621)	(0.037)	(7.575)	(27.425)	(8.491)	(0.301)	(1.988)
Number of Observations	10,205	10,205	5,737	6,730	7,208	3,765	6,630	6,673	11,219	4,561	10,734	3,015	4,637
Number of Countries	194	194	181	167	192	172	168	179	191	184	193	140	136
Number of Conflict Onsets	102	102	41	84	63	27	59	61	103	30	104	35	24
R^2	0.07	0.05	0.08	0.13	0.17	0.11	0.12	0.07	0.43	0.13	0.71	0.16	0.12

Note: The table presents LP-DiD estimates of the effect of conflict on external sector dynamics as well as wartime policymakers' attempts to contain the war shock. The dependent variable is the cumulative change in the indicator listed at the top of each column up to horizon h, where $h = 1$ corresponds to the year of conflict onset. The regressions control for three lags of the dependent variable and include country and year fixed effects. The number of observations and R^2 are reported for $h = 1$. Standard errors, clustered at the country level, are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

adopts a more restrictive definition of conflict onset by requiring a 10-year conflict-free window and excludes from the clean-control group observations exposed to conflict during that period. Columns (3) and (4) relax the clean-control restrictions and estimate the effects over 5- and 10-year horizons, respectively, while including additional lags of the conflict-onset indicator. Column (5) restricts the sample to conflicts lasting at least five consecutive years, thereby focusing on sustained conflict episodes. In column (6), we exclude conflicts motivated by economic considerations based on a narrative classification of conflict motives à la Federle et al. (2026). The narrative classification helps isolate conflicts whose motives relate to domestic business cycle developments, and as such, it strengthens the causal interpretation of the estimated conflict effect. Column (7) reports estimates using the full conflict sample rather than the subset of macro-critical conflicts employed in the baseline analysis. Finally, columns (8) and (9) apply alternative conflict definitions based on battle-related deaths per capita, using thresholds of 50 and 100 deaths per million inhabitants, respectively. There, conflict onset is defined as the first year in which the threshold is crossed following five consecutive years below it.

Across all specifications, the results consistently indicate a negative and economically meaningful effect of conflict onset on output. The estimated losses remain statistically significant and broadly comparable in magnitude across alternative specifications, underscoring the robustness of the baseline findings. Notably, estimates based on the full conflict sample are somewhat smaller than those obtained using the subset of macro-critical conflicts, lending support to our classification strategy.[12] Additionally, excluding conflicts motivated by economic considerations leaves the results largely unchanged, suggesting that reverse causality from underlying economic conditions is unlikely to drive the findings. By contrast, alternative conflict definitions based on battle-related deaths per capita produce larger estimated losses, consistent with the notion that more intense conflicts inflict greater economic damage. For example, when conflicts are defined using a threshold of 100 deaths per million inhabitants, the estimated output costs are approximately twice as large as those reported in the baseline specification.

4 Macroeconomic dynamics after conflict

In this section, we examine macroeconomic dynamics in the aftermath of conflicts. We begin by briefly outlining the empirical approach before turning to recovery dynamics. We focus on output and subsequently examine the supply-side components of GDP to better understand the drivers of postconflict recovery.

[12]The smaller effects obtained using the full conflict sample also suggest that including less economically consequential conflicts attenuates the estimated average treatment effect.

4.1 Empirical strategy

The analysis of the macroeconomic dynamics after conflict termination follows the same LP-DiD framework used to analyze dynamics during conflicts, with however, a definition of clean controls tailored to conflict termination. Specifically, for horizons $h = 0, \ldots, 5$, we estimate the following equation:

$$\ln(y_{i,t+h}) - \ln(y_{i,t-1}) = \beta^h \, \text{ConflictTermination}_{i,t} + \delta^h \, X_{i,t} + \theta_t^h + \mu_i^h + \varepsilon_{i,t}^h, \tag{2}$$

where the left-hand side again denotes cumulative growth of the outcome variable y for country i between year $t-1$ and $t+h$. The indicator $\text{ConflictTermination}_{i,t}$ equals one in the first year of peace provided that (i) the country was in conflict in the preceding year and (ii) the subsequent five years remain conflict-free, that is,

$$\text{Conflict}_{i,t-1} = 1, \quad \text{Conflict}_{i,t} = 0, \quad \text{and} \quad \text{Conflict}_{i,t+j} = 0 \quad \text{for} \quad 1 \leq j \leq L-1,$$

with $L = 5$. As such, a conflict in our sample is defined as terminated when battle-related deaths fall below the baseline threshold of 25 deaths per calendar year and remain below that level for at least five consecutive years. Our baseline definition of conflict termination therefore focuses on nonfragile postconflict episodes: the cases where the end of conflict marks the beginning of relatively lasting peace. The estimation sample is restricted to treated observations and clean controls defined as country-year observations not exposed to conflict within a symmetric window around termination, i.e.,

$$\text{Conflict}_{i,t-j} = 0 \quad \text{for} \quad -h \leq j \leq L,$$

The LP-DiD regressions then trace the evolution of macroeconomic outcomes (output and production factors) over the first five years of postconflict peace relative to a control group of countries not experiencing conflict nor postconflict peace during the same period, excluding observations exposed to conflict.[13] As in equation (1), the vector $X_{i,t}$ includes three lags of the dependent variable, and the terms θ_t^h, μ_i^h, and $\varepsilon_{i,t}^h$ denote year fixed effects, country fixed effects, and the error term, respectively.

4.2 Main Results

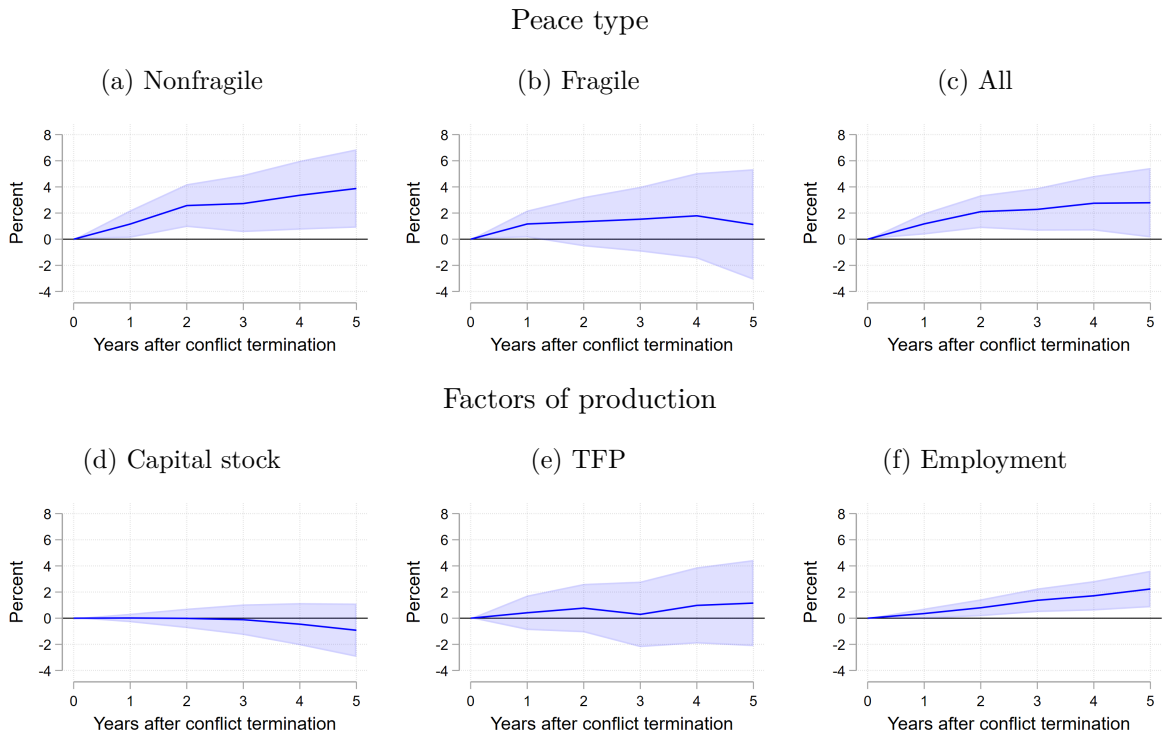
Baseline findings.—We report estimates of output dynamics following conflict termination in Figure 6 under three alternative settings. Figure 6a presents the baseline estimates corresponding to equation (2), focusing on our baseline definition of *nonfragile* postconflict episodes, again, defined as cases in which peace lasts for at least five years. Figure 6b reports dynamics for *fragile* episodes,

[13] We further distinguishes between "non-fragile" postconflict episodes, in which peace lasts at least five years, and "fragile" episodes, in which conflict restarts within five years. Robustness checks using a 10-year window to define non-fragile and fragile postconflict recoveries yield broadly similar results.

where conflict resumes within five years. Finally, Figure 6c presents unconditional estimates that combine both groups. The results indicate that postconflict recoveries are slow, uneven, and critically dependent on the durability of peace. When peace is sustained, output gradually rebounds, although the pace of recovery varies considerably across countries. On average, output increases steadily and reaches approximately 3.8% above its level at conflict termination five years later. Relative to the cumulative output losses observed five years after conflict onset, this implies that only about half of wartime losses are recovered over the same horizon. Confidence intervals widen substantially at longer horizons, highlighting considerable cross-country heterogeneity in recovery experiences. By contrast, when peace proves fragile and conflict resumes within five years, output fails to recover, indicating that relapse sharply undermines recovery prospects. As expected, the unconditional estimates lie between the *fragile* and *nonfragile* cases, reflecting the mixture of recovery experiences observed across postconflict episodes. Our findings suggest that conflict termination alone is insufficient to generate a robust economic recovery. Rather, the durability of peace emerges as a necessary condition for recovery, even if sustained peace does not guarantee a rapid return to preconflict economic conditions.

Underlying mechanisms.—The slow recovery observed in *nonfragile* postconflict episodes raises an important question: what accounts for the modest pace of recovery? Standard neoclassical growth models predict that the destruction of capital raises its marginal product, creating incentives for investment and a gradual rebuilding of the capital stock once hostilities cease. In turn, stronger investment should stimulate labor demand, support the return of displaced workers, and foster a broad-based recovery in economic activity. The rebuilding of productive capacity and the reallocation of resources could also contribute to productivity gains. Under this logic, postconflict recoveries should be characterized by rebounds in capital, labor, and productivity. To shed light on these mechanisms, we turn to the supply side and estimate the effects of *nonfragile* peace onset on the dynamics of capital, labor, and productivity. The results are reported in Figures 6d–6f. Contrary to the predictions of the standard neoclassical framework, we find little evidence of a robust recovery in either capital accumulation or productivity. On average, the postconflict dynamics of both variables are not statistically different from those observed in countries that did not experience conflict. By contrast, labor input recovers more rapidly, suggesting that post–WWII recoveries have typically been labor-led. This pattern is consistent with workers being reallocated from military to civilian activities and with displaced populations, including refugees, gradually returning home and re-entering the labor force. This muted response of capital and productivity is however somewhat surprising given the potentially high marginal product of capital in postconflict environments. One possible explanation is that persistent uncertainty continues to depress investment incentives even after hostilities end. In addition, domestic firms may face binding financing constraints that limit their ability to rebuild productive capacity during the reconstruction phase. We next turn to firm-level evidence to better understand the constraints that hinder the rebuilding of capital and productivity in postconflict economies.

Figure 6: Macroeconomic dynamics after conflict



Note: The figure presents local-projection difference-in-differences (LP-DiD) estimates of the effect of postconflict peace on selected macroeconomic indicators. The dependent variable is the cumulative change in output and production factors up to horizon h, where $h = 1$ corresponds to the first year of peace. Peace is classified as nonfragile if conflict does not restart within the first five years, and as fragile if conflict restarts within that period. The shaded areas indicate 90% confidence intervals. The corresponding regression results are reported in Table A8.

5 Postconflict dynamics: firm-level evidence

To better understand the forces underlying postconflict recovery, we turn to firm-level evidence. While a growing literature has documented the effect of conflict on firms' performance (Amodio and Di Maio, 2018; Del Prete et al., 2023; Brancati et al., 2024), much less is known about how firms adjust once hostilities cease. Yet firms are the key agents through which reconstruction ultimately takes place. Decisions regarding hiring, investment, and production determine the pace at which capital stocks are rebuilt, displaced workers are reabsorbed into the labor force, and productivity recovers. Firm-level evidence can therefore help shed light on the mechanisms underlying the aggregate recovery patterns documented in the previous section, including why capital does not come back.

5.1 Empirical strategy

The firm-level analysis is also useful from an identification perspective. The macro-level analysis in Section 4 relies on country-level variation in postconflict transitions. And post-conflict recovery prospects may be endogenous to peace duration. By contrast, geocoded firm-level data allow us to exploit variation in post-conflict exposure at the firm-level, and control for potential confounders at the firm-, sector-year and country-year levels. Our identification strategy thus relies on within-firm variation, and therefore helps mitigate concerns that the aggregate results are driven by post-conflict selection effect. Furthermore, to further mitigate selection effect related to conflict exposure at the country-level, the analysis focuses on countries that experienced at least one conflict episode during 1989–2024, based on data from the UCDP Georeferenced Event Database (GED), and for which firm-level information is available in the *Orbis* database, provided by Bureau van Dijk.

We geolocate firms operating in 53 conflict-affected countries using postcode and city information reported in *Orbis* (see Table A2). For each firm with valid location information, city and postcode data—together with the country identifier—are submitted to the Photon geocoder[14] to obtain latitude and longitude coordinates for each unique city–postcode combination. The resulting coordinates are cross-checked against World Bank country shapefiles to ensure consistency with the reported country. Firm locations are then assigned through a hierarchical matching procedure based on postcode, city, and the combined city–postcode identifier. For countries with lower matching rates, the procedure is supplemented with alternative geocoders, including Nominatim, and validated coordinates are incorporated into the final dataset.[15]

The precise geolocated firms are subsequently matched to the GED using the geographic coordinates of conflict events and firms. For each firm-year observation, we identify and aggregate all conflict events—and associated battle-related deaths—occurring within a 20-kilometer radius of the firm's location. This procedure yields firm-level measures of conflict exposure that vary across both space and time and form the basis of the empirical analysis. Consistent with the macro-level approach, we exclude firm-year observations experiencing active conflict in order to focus on postconflict dynamics. This restriction allows us to isolate firm adjustment after hostilities cease,

[14]Photon geocoder is an open-source geocoding API available at the following address; https://photon.komoot.io/

[15]We rely on the *Orbis* database rather than the World Bank Enterprise Surveys for three reasons. First, the key advantage is that *Orbis* provides longitudinal detailed firm-level balance-sheet data, allowing us to track the same firms before and after conflict termination, construct measures of capital, labor, and productivity à la Hsieh and Klenow (2009), and estimate adjustment patterns using within-firm variations. By contrast, Enterprise Surveys are primarily repeated cross-sections, while the panel component available for the post-2006 period is depends on country-specific survey date. Second, *Orbis* contains detailed location-related information–postcode and city that can be geocoded with modern geocoding technics, as we do in this paper, and matched to conflict events at a fine spatial scale, which is central to our identification strategy. Third, its broad time coverage allows us to study post-conflict dynamics across a large number of conflict episodes spanning several decades. A limitation of *Orbis*, however, is that coverage varies across countries and tends to under-represent smaller firms, and informal firms in particular. As such, the estimates reported here should be interpreted as average effects for the average firm in our sample. We are nevertheless reassured by the fact that, as shown in the next subsection, the firm-level findings are broadly consistent with the macro-level evidence. Nonetheless, it is plausible that our estimates represent a lower bound of the effects on firms, as smaller and informal firms are likely to face more severe financing constraints during the post-conflict period and may therefore experience greater difficulties rebuilding productive capital.

rather than the direct effects of ongoing violence. We then estimate the following specification:

$$\ln Y_{fist} = \beta_1 PostConflict_{fist} + \beta_2 X_{fist} + \delta_f + \phi_{st} + \omega_{it} + \varepsilon_{fist}. \tag{3}$$

where $\ln Y_{fist}$ denotes the logarithm of the dependent variable: *capital*, measured as firm's tangible fixed assets; *labor*, measured a total number of employees; or *productivity* (TFPQ, total factor productivity quantity-based), computed following Hsieh and Klenow (2009)—for firm f in country i, sector s, and year t. The variable $PostConflict_{fist}$ is a dummy equal to one during the five years following a conflict episode occurring within a 20-kilometer radius of the firm's location, with at least 25 battle-related deaths, and zero otherwise. The vector X_{fist} includes firm-level controls, which in the baseline specification comprise firm age, size (log of firm's total assets, leverage (log of total debt-to-total assets), and export status (a dummy variable if the firm is exporter). The specification includes firm fixed effects (δ_f), sector-by-year fixed effects (ϕ_{st}), and country-by-year fixed effects (ω_{it}), thereby exploiting within-firm variation for identification. Moreover, we convert all nominal variables to 2015 US dollars and follow Díez et al. (2021) for the data cleaning and processing. Our final sample consists of 5,099,197 unique geocoded firms over the period 1983–2023.[16] Approximately 3.4% of firms in the sample experienced at least one conflict event within a 20-kilometer radius of their location during the sample period. This corresponds to about 4.1% of all firm-year observations.

5.2 Results

Columns (1)–(3) of Table 3 report the baseline estimates of equation (3). We find that, on average, surviving firms expand employment modestly after conflict ends, while capital stocks remain weak and productivity exhibits only limited improvement. These findings are broadly consistent with the aggregate evidence presented in the previous section pointing to a recovery driven primarily by labor rather than by capital accumulation or productivity gains.

To better understand the drivers of these dynamics, columns (4)–(6) of Table 3 examine heterogeneity by factor intensity, distinguishing between capital-intensive and labor-intensive firms. Table 4 further reports heterogeneous effects by firms' net worth (low versus high net worth) and export status (exporters versus nonexporters). The results reveal substantial differences across firms. Capital-intensive firms, exporters, and firms with stronger balance sheets experience gains in both employment and productivity, consistent with greater access to financing, stronger market opportunities, and a greater ability to leverage existing productive assets during the recovery process. By contrast, labor-intensive firms and nonexporters primarily expand employment, while capital stocks remain well below preconflict levels. Firms with weaker balance sheets experience particularly persistent capital shortfalls, pointing to the importance of financial constraints in shaping postconflict adjustment.

[16] Observations prior to 1997 are relatively sparse. The results reported below are, however, robust to restricting the sample to the post-1997 period.

Table 3: Firm Dynamics Postconflict: Baseline Estimates and Heterogeneity by Factors Intensity

	Baseline			Factor Intensity		
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Capital	Labor	TFP	Capital	Labor	TFP
Postconflict	-0.046*	0.059**	-0.016			
	(0.027)	(0.022)	(0.025)			
Capital Intensive				-0.005	0.075**	0.048***
				(0.031)	(0.036)	(0.016)
Labor Intensive				-0.046*	0.080***	-0.061*
				(0.024)	(0.008)	(0.030)
Number of Observations	16,577,311	21,140,632	4,923,758	16,577,311	21,140,632	4,923,758
R^2	0.96	0.96	0.86	0.96	0.96	0.86
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports the estimated impact of postconflict peace on firm dynamics. The dependent variables are the logarithms of firm-level capital, labor, and total factor productivity (TFP, measured as quantity-based TFP or TFPQ). The key independent variable is a postconflict peace dummy, which equals 1 during the five years following a conflict episode occurring within a 20-kilometer radius of the firm's location. Regressions control for firm age, size, leverage, and export status, and include firm, country-year, and sector-year fixed effects. Columns (1)–(3) report the baseline results. Columns (4)–(6) report heterogeneity by factor intensity. Robust standard errors are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 4: Firm Dynamics Postconflict: Heterogeneity by Firm Characteristics

	Net Worth			Export Status		
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Capital	Labor	TFP	Capital	Labor	TFP
Low Net Worth	-0.104**	-0.038***	-0.080***			
	(0.042)	(0.011)	(0.029)			
High Net Worth	-0.023	0.101***	0.050**			
	(0.045)	(0.014)	(0.018)			
Exporter				-0.019	0.060***	0.221***
				(0.026)	(0.015)	(0.023)
Non-Exporter				-0.047*	0.059**	-0.030
				(0.028)	(0.022)	(0.033)
Observations	16,577,311	21,140,632	4,923,758	16,577,311	21,140,632	4,923,758
R^2	0.96	0.96	0.86	0.96	0.96	0.86
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports the heterogeneity of the estimated impact of postconflict peace on firm dynamics. The dependent variables are the logarithms of firm-level capital, labor, and total factor productivity (TFP, measured as quantity-based TFP or TFPQ). The key independent variable is a postconflict peace dummy, which equals 1 during the five years following a conflict episode occurring within a 20-kilometer radius of the firm's location. Results are reported by firm net worth (columns (1)–(3)) and export status (columns (4)–(6)). Depending on the specification, regressions control for firm age, size, leverage, and export status. All regressions include firm, country-year, and sector-year fixed effects. Robust standard errors are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Overall, the evidence suggests that postconflict recovery is driven disproportionately by stronger firms that are better positioned to expand production and benefit from a reallocation of factors of production. In contrast, weaker firms appear to suffer persistent capital scarring, which contributes to subdued recovery in both capital and productivity. From a policy standpoint, these findings highlight the potential role of policies aimed at easing credit constraints and facilitating capital rebuilding in accelerating postconflict recovery, and suggest that conflicts can affect market structure. While this analysis has focused on formal firms, an important avenue for future research is the study of factor reallocation across the formal and informal sector in postconflict settings.

6 Conclusion

This paper studies the macroeconomic dynamics of wars and post-conflict recovery using a newly assembled global dataset covering the period 1946–2024. Combining a local-projection difference-in-differences framework with clean-control restrictions and complementary geocoded firm-level evidence, we provide a unified analysis of economic adjustment during wars and recovery after conflict termination.

Our findings yield four main contributions relative to the existing literature. First, wars impose large and persistent economic costs. Output losses not only deepen over time but typically exceed those associated with banking, currency, and sovereign debt crises, as well as severe natural disasters. Second, external-sector dynamics play a central role in shaping wartime adjustment. Trade disruptions, capital outflows, reserve losses, exchange-rate pressures, and reduced access to private external financing amplify the economic costs of war and constrain the policy options available to wartime governments. Third, post-conflict recoveries are slow, uneven, and critically dependent on the durability of peace. When conflict resumes, recoveries frequently stall. Even when peace is sustained, output rebounds only gradually and remains modest relative to wartime losses. The evidence further suggests that post–World War II recoveries have typically been labor-led, while capital accumulation and productivity remain subdued. Fourth, firm-level evidence points to persistent constraints in rebuilding capital, particularly among financially weaker firms, helping explain why aggregate recoveries often fall short of the predictions of standard neoclassical models.

Our findings suggest that the economic legacy of war extends well beyond the cessation of hostilities. Durable recoveries require not only peace, but also policies that restore confidence, facilitate access to finance and rebuild capital. More broadly, the results highlight that the challenges of post-conflict recovery involve overcoming frictions that hinder the allocation of resources toward productive uses in the aftermath of conflict.

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A Appendix

(For online publication only)

Table A1: Data Sources

Indicators	**Sources**
Conflict Data	
Yearly conflict incidence, intensity, duration, type (1946–2024)	UCDP/PRIO Armed Conflict Dataset, version 25.1 (Gleditsch and others 2002; Davies and others 2025)
Georeferenced yearly conflict incidence and battle-related deaths (1989–2024)	UCDP Georeferenced Event Dataset (GED) Global, version 25.1 (Sundberg and Melander 2013)
Yearly conflict incidence and type (1816–2007)	Correlates of War Non-State War data set, version 4.0; Intra-State War data set, version 5.1; Inter-State War data set, version 4.0; Extra-State War data set, version 4.0 (Sarkees and Wayman 2010)
Battle-related deaths (1946–2008)	PRIO Battledeaths Dataset, version 3.1 (Lacina and Gleditsch 2005)
Groups	
Neighboring countries	Geographic Distance Database (Pellegrino, Spolaore, and Wacziarg 2025)
Trading partners	CEPII, TRADHIST (Fouquin and Hugot 2016); IMF, *International Trade in Goods (by partner country)* (IMTS)
Commodity exporters	World Bank, *World Integrated Trade Solution* (WITS)
Macroeconomic Data	
Real GDP, private and public consumption, investment, imports, exports; GDP deflator; consumer price index; government debt and tax revenue; exchange rate; gross national savings	Global Macro Database (Müller and others 2025); IMF, *World Economic Outlook*; World Bank, *World Development Indicators*
Capital stock and total factor production (at constant national prices), employment	Penn World Table, version 11.0 (Feenstra, Inklaar, and Timmer 2015)
Population	Global Macro Database (Müller and others 2025); IMF, *World Economic Outlook*; World Bank, *World Development Indicators*; UN DESA, *World Population Prospects*
Defense spending	Gethin (2024); SIPRI Military Expenditure Database; World Human Capital Expenditure Database (Bharti and others 2026)
Foreign direct investment liabilities; portfolio debt and other liabilities	External Wealth of Nations (Lane and Milesi-Ferretti 2018)
Reserve assets	IMF, *World Economic Outlook*

Indicators	Sources
Official development assistance (ODA)	OECD, *DAC2A: Aid (ODA) disbursements to countries and region*
Personal remittances, received	World Bank, *World Development Indicators*
Informality (percent of GDP)	Medina and Schneider (2019)
Financial openness (Chinn–Ito Index)	Chinn and Ito (2006), updated
Debt restructurings	Horn, Reinhart, and Trebesch (2022)
Banking crisis, sovereign debt crisis, currency crisis	Global Macro Database (Müller and others 2025)
Natural disasters	Emergency Events Database (EM-DAT) (Delforge and others 2025)
Micro-Level Data	
Firm-level data on capital, labor, total factor production, and other firm-level covariates	Orbis

Table A2: Economies in the analysis

Category	Economies
Macroeconomic Dynamics During and After Wars End	
Advanced economies	Andorra; Australia; Austria; Belgium; Canada; Croatia (1991*, 1993^); Cyprus (1955*, 1960^, 1974*, 1975^); Czech Republic; Denmark; Estonia (1949^); Finland; France; Germany; Greece (1950^); Iceland; Ireland; Israel; Italy; Japan; Korea (1954^); Latvia; Liechtenstein; Lithuania; Luxembourg; Malta; Netherlands; New Zealand; Norway; Portugal; Puerto Rico; San Marino; Singapore; Slovak Republic; Slovenia; Spain (1978*, 1992^); Sweden; Switzerland; United Kingdom (1971*, 1992^, 1998*, 1999^); United States (2001*, 2002^).

Category	Economies
Emerging markets	Albania; Algeria (1954*, 1964^, 1991*); Angola (1961*, 2003^); Antigua and Barbuda; Argentina (1955*, 1956^, 1963*, 1964^, 1974*, 1978^); Armenia (1990*, 1992^); Aruba; Azerbaijan (1990*, 1999^, 2005*); Bahamas, The; Bahrain; Barbados; Belarus; Belize; Bolivia (1953^, 1967*, 1968^); Bosnia and Herzegovina (1992*, 1996^); Botswana; Brazil; Brunei Darussalam; Bulgaria; Cabo Verde; Chile (1973*, 1974^); China (1963^, 1969*, 1970^, 1980*, 1988^, 2020*); Colombia (1964*); Costa Rica (1949^); Dominica; Dominican Republic (1965*, 1966^); Ecuador (1995*, 1996^); Egypt (1951*, 1957^, 1967*, 1974^, 1993*, 1999^, 2014*); El Salvador (1969*, 1973^, 1979*, 1992^); Equatorial Guinea; Eswatini; Fiji; Gabon; Georgia (1991*, 1994^); Grenada; Guatemala (1955^, 1963*, 1996^); Guyana; Hungary (1956*, 1957^); India (1952^, 1962*, 1972^, 1983*); Indonesia (1962^, 1975*, 2006^); Iran (1947^, 1966*, 1969^, 1974*, 1997^, 2016*, 2019^); Iraq (1961*, 1997^, 2003*, 2004^); Jamaica; Jordan (1950^, 1967*, 1968^); Kazakhstan; Kosovo (1998*, 2000^); Kuwait (1990*, 1992^); Lebanon (1950^, 1958*, 1959^, 1975*, 1977^, 1982*, 2001^, 2006*, 2007^, 2023*); Libya (2011*); Malaysia (1967^, 1974*, 1976^, 1981*, 1982^); Maldives; Marshall Islands; Mauritius; Mexico; Micronesia; Mongolia; Montenegro, Rep. of; Morocco (1953*, 1964^, 1975*, 1990^); Namibia (1966*, 1990^); Nauru; North Macedonia; Oman (1957*, 1958^, 1969*, 1976^); Pakistan (1949^, 1965*, 1966^, 1971*, 1978^, 1990*); Palau; Panama (1989*, 1990^); Paraguay (1948^, 1954*, 1955^, 1989*, 1990^); Peru (1965*, 1966^, 1982*, 2000^, 2007*, 2011^); Philippines (1955^, 1969*); Poland; Qatar; Romania (1989*, 1990^); Russia (1969*, 1970^, 1994*, 2008^, 2015*); Samoa; Saudi Arabia; Serbia (1991*, 1993^, 1998*, 2000^); Seychelles; South Africa (1966*, 1990^); Sri Lanka (1971*, 1972^, 1984*, 2010^); St. Kitts and Nevis; St. Lucia; St. Vincent and the Grenadines; Suriname (1987*, 1988^); Syria (1950^, 1966*, 1968^, 1973*, 1974^, 1979*, 1983^, 2011*); Thailand (1951*, 1952^, 1974*, 1983^, 2003*); Tonga; Trinidad and Tobago (1990*, 1991^); Tunisia (1953*, 1957^, 1980*, 1981^, 2013*, 2016^); Turkmenistan; Tuvalu; Türkiye (1984*); Ukraine (1951^, 2014*); United Arab Emirates; Uruguay; Vanuatu; Venezuela; Vietnam (1988^); West Bank and Gaza.

Category	Economies
Low-income countries	Afghanistan (1978*); Bangladesh (1971*, 1992^); Benin (2022*); Bhutan; Burkina Faso (1987*, 1988^, 2018*); Burundi (1965*, 1966^, 1991*, 2009^, 2014*); Cambodia (1954^, 1967*, 1999^); Cameroon (1957*, 1960^, 1996*, 1997^, 2015*); Central African Republic (2001*); Chad (1966*); Comoros; Congo, Democratic Republic of the (1960*, 1968^, 1977*, 1979^, 1996*); Congo, Republic of (1993*, 2003^, 2016*, 2017^); Côte d'Ivoire (2002*, 2005^, 2011*, 2012^); Djibouti (1991*, 2000^); Eritrea (1964*, 1992^, 1998*, 2001^, 2016*, 2017^); Ethiopia (1960*, 1992^, 1998*, 2001^, 2016*); Gambia, The; Ghana (1966*, 1967^, 1981*, 1984^); Guinea; Guinea-Bissau (1963*, 1974^, 1998*, 2000^); Haiti (1989*, 1992^, 2004*, 2005^, 2024*); Honduras (1969*, 1970^); Kenya (1952*, 1957^, 2015*); Kiribati; Kyrgyz Republic; Lao P.D.R. (1954^, 1959*, 1974^, 1989*, 1991^); Lesotho (1998*, 1999^); Liberia (1980*, 1981^, 1989*, 1991^, 2000*, 2004^); Madagascar (1948^); Malawi; Mali (1990*, 1995^, 2007*); Mauritania (1957*, 1959^, 1975*, 1979^); Moldova (1992*, 1993^); Mozambique (1964*, 1993^, 2013*); Myanmar (1995^, 2013*); Nepal (1960*, 1963^, 1996*, 2007^); Nicaragua (1977*, 1991^); Niger (1991*, 1998^, 2007*, 2009^, 2021*); Nigeria (1966*, 1971^, 1996*, 1997^, 2009*); Papua New Guinea (1990*, 1997^); Rwanda (1990*, 2001^, 2009*, 2010^, 2019*); Senegal; Sierra Leone (1991*, 2002^); Solomon Islands; Somalia (1982*); South Sudan (2011*); Sudan (1963*, 1977^, 1983*); São Tomé and Príncipe; Tajikistan (1992*, 2001^, 2010*, 2012^); Tanzania (1978*, 1979^); Timor-Leste (1975*, 2000^); Togo; Uganda (1971*, 2017^); Uzbekistan; Yemen (1949^, 1962*, 1973^, 1979*, 1983^, 2009*); Zambia; Zimbabwe (1967*, 1980^).
Firm-Level Analysis	
Advanced economies	Croatia; Israel.
Emerging markets	Algeria; Argentina; Armenia; Brazil; Colombia; Ecuador; Egypt; El Salvador; India; Iran; Iraq; Jordan; Kosovo; Kuwait; Lebanon; Mexico; Morocco; North Macedonia; Pakistan; Peru; Philippines; Russia; Saudi Arabia; Serbia; South Africa; Sri Lanka; Syria; Thailand; Tunisia; Türkiye; Ukraine; Venezuela; West Bank and Gaza.
Low-income countries	Afghanistan; Bangladesh; Burkina Faso; Congo, Democratic Republic of the; Congo, Republic of; Côte d'Ivoire; Ethiopia; Honduras; Kenya; Liberia; Mali; Myanmar; Nepal; Nigeria; Sudan; Uganda; Uzbekistan.

Note: * indicates conflict onset (the first year when a country becomes a conflict-site as long as the preceding five years were without conflict); ^ indicates the first year of nonfragile peace (the first year of peace as long as all of the five years following conflict termination remain conflict-free).

Table A3: Baseline finding and heterogeneity of conflict effects

		By Intensity		By Duration		By Type	
	All Conflict	Major	Minor	Short	Long	Between States	Within States
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
h = 1	-2.795***	-4.500***	-1.238*	-5.059***	-1.306**	-4.152**	-2.441***
	(0.599)	(0.760)	(0.647)	(1.155)	(0.522)	(1.932)	(0.557)
h = 2	-4.728***	-7.630***	-1.538*	-6.220***	-3.760***	-4.922**	-4.615***
	(0.834)	(1.252)	(0.901)	(1.501)	(0.890)	(1.956)	(0.872)
h = 3	-5.578***	-8.708***	-1.710	-6.777***	-4.727***	-4.207*	-5.805***
	(1.120)	(1.546)	(1.269)	(1.998)	(1.281)	(2.153)	(1.206)
h = 4	-5.766***	-8.725***	-1.753	-6.372***	-5.235***	-3.606	-6.109***
	(1.360)	(1.802)	(1.555)	(2.163)	(1.644)	(2.186)	(1.501)
h = 5	-6.659***	-8.598***	-2.380	-6.722***	-6.479***	-5.235*	-6.870***
	(1.624)	(2.304)	(1.819)	(2.393)	(2.053)	(2.812)	(1.789)
Number of Observations	10,648	12,062	10,585	10,551	10,585	10,519	10,625
Number of Countries	191	194	191	191	191	191	191
Number of Conflict Onsets	160	114	97	63	97	31	137
R^2	0.28	0.26	0.28	0.28	0.28	0.27	0.28

Note: The table presents local-projection difference-in-differences (LP-DiD) estimates of the effect of conflict on output, reporting the baseline specification (column 1) and heterogeneity by conflict intensity (columns 2–3), duration (columns 4–5), and type (columns 6–7). The dependent variable is the cumulative change in output up to horizon h, where $h = 1$ corresponds to the year of conflict onset. The regressions include three lags of the dependent variable and country and year fixed effects. Major conflicts involve at least 1,000 battle-related deaths; minor conflicts involve 25–999. Short conflicts last up to two years; long conflicts last longer. Between-state conflicts are interstate; within-state include intrastate and related types. Observations and R^2 are reported for $h = 1$. Standard errors (clustered at the country level) are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Comparison to other shocks

	Conflict	Banking Crises	Currency Crises	Sovereign Debt Crises	Natural Disasters
	(1)	(2)	(3)	(4)	(5)
h = 1	-2.795***	-1.493***	-1.157***	-2.009***	-1.659*
	(0.599)	(0.341)	(0.336)	(0.384)	(0.893)
h = 2	-4.728***	-2.379***	-1.766***	-3.311***	-1.960
	(0.834)	(0.594)	(0.584)	(0.681)	(1.477)
h = 3	-5.578***	-2.683***	-1.595**	-3.193***	-3.898
	(1.120)	(0.719)	(0.793)	(0.830)	(2.576)
h = 4	-5.766***	-3.377***	-1.346	-3.214***	-5.039
	(1.360)	(0.897)	(1.007)	(1.102)	(3.173)
h = 5	-6.659***	-3.301***	-1.101	-4.358***	-5.626
	(1.624)	(1.142)	(1.154)	(1.326)	(3.634)
Number of Observations	10,648	10,828	10,074	10,994	12,924
Number of Countries	191	158	158	158	194
Number of Crisis Onsets	160	295	392	200	36
R^2	0.28	0.18	0.18	0.18	0.24

Note: The table presents local-projection difference-in-differences (LP-DiD) estimates of the effects of conflict and other shocks (banking, currency, and sovereign debt crises, as well as severe natural disasters) on output. The dependent variable is the cumulative change in output up to horizon h, where $h = 1$ corresponds to the year of conflict onset or the onset of other shocks. The regressions control for three lags of the dependent variable and include country and year fixed effects. Column (1) reports the baseline estimate of the effect of conflicts. Columns (2)–(5) report the effects of banking crises, currency crises, sovereign debt crises, and severe natural disasters, respectively. Definitions follow standard dummy-variable classifications used in the literature. Major natural disasters are identified when disaster-related deaths exceed the 99th percentile. The number of observations and R^2 are reported for $h = 1$. Standard errors (clustered at the country level) are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Output Cost Spillovers from Conflicts

	Belligerent (1)	Neighboring Countries (2)	Trade Partners (3)
h = 1	-0.094	-1.165***	-0.613*
	(0.361)	(0.427)	(0.358)
h = 2	-0.044	-1.385**	-1.071*
	(0.630)	(0.667)	(0.577)
h = 3	-0.393	-1.068	-1.039
	(0.774)	(0.953)	(0.855)
h = 4	-0.955	-1.000	-1.367
	(0.897)	(1.229)	(1.075)
h = 5	-1.603	-1.154	-1.206
	(1.174)	(1.556)	(1.298)
Number of Observations	10,362	9,299	6,918
Number of Countries	194	189	189
Number of Conflict Exposure Onsets	196	190	289
R^2	0.25	0.27	0.27

Note: The table presents local-projection difference-in-differences (LP-DiD) estimates of the spillover effects of conflict on output for belligerent countries (column 1), neighboring countries (column 2), and trade partners (column 3). The dependent variable is the cumulative change in output up to horizon h, where $h = 1$ corresponds to the year of conflict onset. The regressions control for three lags of the dependent variable and include country and year fixed effects. Belligerent countries are defined as those that participate in a conflict without experiencing hostilities on their own territory. Neighboring countries are those sharing a land border with a conflict-site economy. Trade partners are defined as countries whose import share from conflict-site economies exceeds the 90th percentile of the distribution. The number of observations and R^2 are reported for $h = 1$. Standard errors, clustered at the country level, are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A6: Impact of Conflict on Demand-Side of Output

	Investment	Private Consumption	Public Consumption	Imports	Exports
	(1)	(2)	(3)	(4)	(5)
h = 1	-8.838***	-4.182***	2.091	-6.632***	-11.772***
	(2.300)	(0.883)	(1.931)	(1.968)	(2.423)
h = 2	-12.190***	-8.195***	3.732	-8.822***	-17.100***
	(3.345)	(1.890)	(2.718)	(3.040)	(3.206)
h = 3	-15.458***	-10.076***	3.432	-9.024**	-16.630***
	(4.055)	(2.113)	(3.501)	(4.070)	(4.113)
h = 4	-17.207***	-11.520***	1.187	-11.139**	-19.245***
	(5.178)	(2.322)	(4.391)	(4.662)	(5.077)
h = 5	-16.041**	-12.714***	-2.250	-12.745**	-20.013***
	(6.791)	(2.908)	(5.354)	(5.108)	(6.199)
Number of Observations	9,973	6,858	6,958	10,216	10,252
Number of Countries	194	177	178	194	194
Number of Conflict Onsets	98	65	65	102	103
R^2	0.06	0.11	0.09	0.11	0.11

Note: The table presents local-projection difference-in-differences (LP-DiD) estimates of the effect of conflict on the demand-side components of output. The dependent variable is the cumulative change in each component up to horizon h, where $h = 1$ corresponds to the year of conflict onset. The regressions control for three lags of the dependent variable and include country and year fixed effects. The number of observations and R^2 are reported for $h = 1$. Standard errors, clustered at the country level, are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A7: Robustness Checks

	Longer Horizon		Without Clean Controls		Sample Selection			Battle-Deaths / Population	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
h = 1	-2.739***	-2.764***	-2.963***	-3.076***	-1.437*	-2.565***	-2.097***	-4.937***	-6.531***
	(0.598)	(0.644)	(0.631)	(0.664)	(0.797)	(0.616)	(0.518)	(0.754)	(0.855)
h = 2	-4.981***	-5.016***	-4.863***	-5.985***	-5.383***	-4.400***	-3.751***	-7.543***	-9.938***
	(0.896)	(0.988)	(0.861)	(1.106)	(1.649)	(0.866)	(0.757)	(1.082)	(1.100)
h = 3	-5.945***	-6.525***	-5.553***	-8.005***	-6.598***	-5.316***	-4.141***	-9.088***	-11.357***
	(1.222)	(1.423)	(1.053)	(1.527)	(2.066)	(1.157)	(1.000)	(1.471)	(1.580)
h = 4	-6.211***	-6.778***	-5.622***	-8.218***	-7.289***	-5.738***	-4.108***	-8.147***	-10.924***
	(1.516)	(1.652)	(1.243)	(1.724)	(2.561)	(1.437)	(1.203)	(1.641)	(1.959)
h = 5	-7.225***	-7.875***	-6.361***	-9.327***	-9.068***	-6.716***	-4.876***	-8.717***	-11.090***
	(1.829)	(2.098)	(1.481)	(2.196)	(3.025)	(1.712)	(1.467)	(2.082)	(2.524)
h = 6	-7.813***	-8.668***		-10.221***					
	(2.079)	(2.351)		(2.439)					
h = 7	-8.756***	-9.802***		-11.067***					
	(2.372)	(2.686)		(2.715)					
h = 8	-9.906***	-11.044***		-11.996***					
	(2.688)	(3.007)		(2.961)					
h = 9	-10.779***	-12.685***		-13.476***					
	(2.967)	(3.318)		(3.204)					
h = 10	-11.135***	-14.250***		-14.406***					
	(3.245)	(3.701)		(3.336)					
Number of Observations	9,667	9,684	12,841	11,212	10,532	10,725	10,214	10,589	10,577
Number of Countries	191	191	194	194	191	191	189	192	192
Number of Conflict Onsets	149	125	158	106	44	148	196	118	100
R^2	0.28	0.29	0.24	0.25	0.28	0.27	0.27	0.28	0.29

Note: The table presents local-projection difference-in-differences (LP-DiD) estimates of the effect of conflict on output. The dependent variable is the cumulative change in output up to horizon h, where $h = 1$ corresponds to the year of conflict onset. The regressions control for three lags of the dependent variable and include country and year fixed effects. Column (1) reports the baseline estimate, while columns (2)–(10) present robustness checks using a longer horizon (columns 2–3), alternative clean-control restrictions (columns 4–5), alternative sample restrictions (columns 6–8), and alternative definitions of conflict (columns 9–10). The number of observations and R^2 are reported for $h = 1$. Standard errors, clustered at the country level, are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A8: Macroeconomic Dynamics after Conflict

	Peace Type			Factors of Production		
	Nonfragile	Fragile	All	Capital Stock	TFP	Employment
	(1)	(2)	(3)	(4)	(5)	(6)
h = 1	1.160*	1.165*	1.173**	0.016	0.416	0.357
	(0.639)	(0.625)	(0.495)	(0.199)	(0.796)	(0.232)
h = 2	2.572**	1.341	2.107***	-0.015	0.770	0.798**
	(0.992)	(1.145)	(0.760)	(0.451)	(1.119)	(0.393)
h = 3	2.727**	1.530	2.281**	-0.114	0.292	1.369**
	(1.325)	(1.503)	(0.991)	(0.708)	(1.514)	(0.548)
h = 4	3.361**	1.789	2.750**	-0.457	0.980	1.714**
	(1.595)	(1.978)	(1.264)	(0.977)	(1.757)	(0.683)
h = 5	3.879**	1.124	2.788*	-0.922	1.155	2.234***
	(1.817)	(2.562)	(1.611)	(1.236)	(1.990)	(0.846)
Number of Observations	10,007	9,979	10,122	7,302	4,799	6,727
Number of Countries	191	193	193	169	115	170
Number of Conflict Ends	142	114	257	119	70	112
R^2	0.28	0.28	0.28	0.82	0.23	0.46

Note: The table presents local-projection difference-in-differences (LP-DiD) estimates of the effect of postconflict peace on selected macroeconomic indicators. The dependent variable is the cumulative change in output (columns (1)–(3)) and production factors (columns (4)–(6)) up to horizon h, where $h = 1$ corresponds to the first year of peace. In columns (1)–(3), peace is classified as nonfragile if conflict does not restart within the first five years, and as fragile if conflict restarts within that period. Columns (4)–(6) focus on nonfragile peace episodes. The regressions control for three lags of the dependent variable and include country and year fixed effects. The number of observations and R^2 are reported for $h = 1$. Standard errors, clustered at the country level, are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

PUBLICATIONS

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