The Price of War

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Abstract

We assemble a new data set spanning 150 years and 60 countries to study the economic toll of war. A war of average intensity is associated with an output drop of close to 10 percent in the war-site economy, while consumer prices rise by approximately 20 percent. The capital stock, total factor productivity, and equity returns all decline sharply. The economic ramifications of war are not confined to the war site. The evidence points to adverse economic outcomes in other belligerent and third-party countries if they are exposed to the war site through trade linkages or share a common border.

JEL Classification: E30, F40, F50
Keywords: Wars, Interstate Wars, Civil Wars, Business Cycles, Spillovers, Trade exposure, Supply Shocks, International Transmission

*We wish to dedicate this paper to Philippe Martin who discussed it at the Kiel-CEPR Geoeconomics Conference in Berlin before his untimely death in December 2023. Federle: Kiel Institute (email: jonathan.federle@ifw-kiel.de); Meier: Tudor Capital Europe LLP (email: Andre.Meier@alumni.eui.eu); Müller: Department of Economics, University of Tübingen, CEPR and CESifo (email: gernot.mueller@uni-tuebingen.de). Mutschler: Department of Economics, University of Tübingen, and Dynare Team (email: willi@mutschler.eu). Schularick: Kiel Institute, Sciences Po, and CEPR (email: moritz.schularick@sciencespo.fr). Mutschler acknowledges financial support from the Deutsche Forschungsgemeinschaft (DFG) through Grant No. 411754673. We thank Yuriy Gorodnichenko, four anonymous referees, and our discussants Alessandra Bonfiglioli, Luca Dedola, Philippe Martin, Valerie Ramey, and Ansgar Rannenberg, as well as Ralph Luetticke, Thierry Mayer, Timothy Meyer, Mathias Thoenig, and Christian Wolf for very helpful comments. Sven Eis, Kevin Klein, Teun de Koning, Maximilian Reinhard, and Georg Rösel provided excellent research assistance. The usual disclaimer applies.

You may not be interested in war, but war is interested in you.
Leon Trotsky

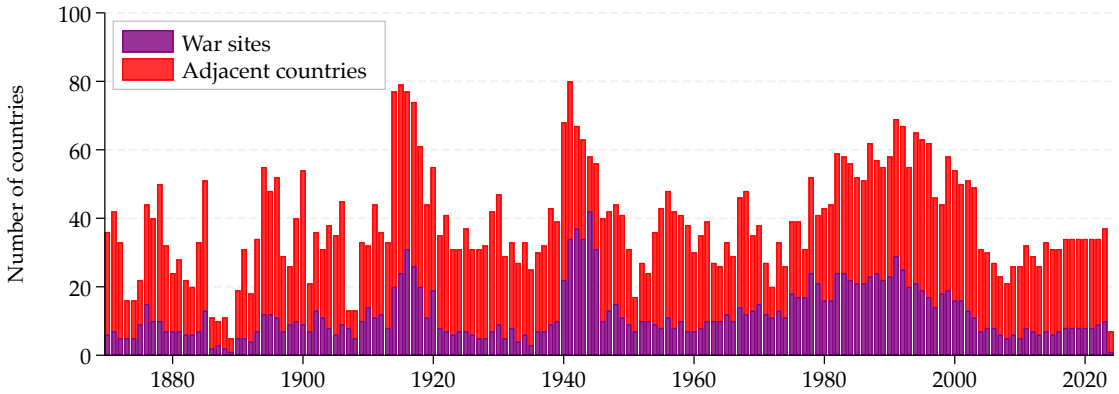
1 Introduction

The global political and economic landscape is undergoing profound changes. Economic fragmentation is increasing (Fernández-Villaverde, Mineyama and Song, 2024; Gopinath et al., 2025). Geopolitical tensions and conflict risks have risen sharply. Wars cause death and destruction, disrupt trade, and wreak havoc on public finances. Many of the large economic disasters of the last century are related to wars on a country's own soil (Barro, 2006). However, as we show in this paper, adverse economic outcomes after the start of war are not confined to the war site. The economies of other belligerent countries and, importantly, those of third countries are affected by war, too. Many countries pay *The Price of War*.

We present the first comprehensive analysis of the economic developments after war onset, based on a novel data set for wars between and within countries since 1870, along with macroeconomic time series data for 60 economies. We study the evolution of economies after the outbreak of war and differentiate between economic trends in the war site, in other belligerent countries, and in third countries. We aim to establish new stylized facts while refraining from causal interpretation, as there may be a two-way link between economic developments and the outbreak of war. Our subsequent analysis actually confirms that economic factors appear to play a role in the outbreak of civil wars. Yet for *interstate* wars that have not been started for short-run economic reasons according to narrative records, economic dynamics after war onset do not differ systematically from other wars.

Our key finding is that in a war of average intensity, output falls by almost 10 percent in the war-site economy while consumer prices rise by some 20 percent (relative to pre-war trends). The capital stock, total factor productivity (TFP), and equity returns all decline sharply. These economic outcomes are not confined to the war site. The economies of belligerent countries and even those of third countries witness similarly unfavorable dynamics if they are exposed to the war site through trade linkages. Output in close trading partners falls by 2 percent relative to trend. The fall in output is accompanied by higher inflation and weak equity returns. Equities are even weaker in third countries sharing a border with the war site. By contrast, third countries that have negligible trade and no common border with the war site seem largely unaffected.

Figure 1: War sites and adjacent countries



Notes: Figure based on data for all countries and all interstate, intrastate, and extrastate wars, as classified by the Correlates of War project (Stinnett et al., 2002); see Section 2 for details on war sites and borders.

Understanding the global economic repercussions of war is important: although war on a country's territory is fairly rare, economies are much more often exposed to potential negative spillovers from war in their neighborhood. Figure 1 illustrates this basic fact. The underlying data imply that the frequency with which a country is a war site in a given year is only 6%. In contrast, the frequency with which a country is adjacent to a war site is much higher at 20%, and about five times as high as the (unconditional) frequency of financial crises (Schularick and Taylor, 2012). Despite this, exposure to war has been largely overlooked as a source of shocks to the global economy.

Our data set brings together information from the *Correlates of War* (COW) project and macroeconomic time series from the *Jordà-Schularick-Taylor Macrohistory Database* (Jordà, Schularick and Taylor, 2017), augmented in Funke, Schularick and Trebesch (2023). We supplement these data by geolocating the sites of interstate wars using granular battle-level information, which we aggregate to the country level. This approach yields 225 country-year observations that mark the onset of interstate wars in specific countries. For intrastate and extrastate wars, which we denote as "other wars" for short, we geolocate 469 war sites, drawing on a range of sources. Both the classification of countries as war sites and the macroeconomic indicators are based on present-day borders for consistency. Interstate wars tend to be more severe, with casualties averaging 3.5 percent of the pre-war population in the war site, considerably larger than the figure we document for other wars (0.74 percent). The distribution of war intensity is also broader and more right-skewed for interstate wars.

To study the economic dynamics after the outbreak of war, we examine a range of indicators including output and prices, the capital stock, TFP, long-term interest rates, equity returns, military spending, and the number of military personnel. We find that the most pronounced changes occur in the war site. In addition to output, the capital stock declines sharply in the war site and TFP drops substantially. On average, the capital stock falls only moderately, both in belligerent and third countries, while TFP remains flat in third countries and even increases marginally in non-war site belligerents. Prices increase strongly in the war site and more moderately elsewhere, interest rates rise moderately across the board, while equity markets decline markedly.

Yet average adjustment dynamics in third countries mask considerable differences: We allow outcomes to vary with pre-war trade exposure and find that it is key to account for adverse economic developments during war. In a third country that traded heavily with the war-site economy prior to the conflict—importing goods equivalent to 3 percent of its GDP—output falls by 2 percent relative to trend following the outbreak of the war. Consistent with this, we find that trade flows contract as both exports from, and—to a lesser extent—imports by the war-site economy decline (see also Glick and Taylor, 2010).

The patterns in the data are consistent with the notion that war represents an adverse supply shock for the war-site economy. While this is an unsurprising finding given the destructive nature of war, two aspects are noteworthy. First, the supply shock generates spillovers via trade, putting upward pressure on prices and lowering economic activity in third countries, too. Second, the military expansion typically accompanying war does not change this picture in the war site materially. By contrast, other belligerents exhibit no output contraction, presumably because fiscal stimulus linked to military spending offsets the contractionary spillovers of war. For instance, the economic expansion in the U.S. and U.K. during both World Wars has been attributed to the sharp increase in military expenditure (Braun and McGrattan, 1993; Ilzetzki, 2024).

While we aim to provide a granular picture of economic dynamics in wars, we acknowledge that our metrics cannot fully capture the economic costs of war and its implications for human welfare. In fact, welfare and output may diverge during wartime, e.g., when output rises due to labor mobilization, even as overall welfare declines. We also lack sufficient data on private consumption, which Goldin and Lewis (1975) used in an early study to quantify the "indirect" costs of the American Civil War—alongside the direct costs associated with increased government spending and the destruction of capital.

We also widen the lens and document broader societal changes after the onset of war. Other than increased mortality and a decline in population, we observe significant and persistent declines in media freedom, judicial independence, electoral fairness, and institutional quality. These changes are largely confined to the war site, with the exception of measured media freedom, which also declines somewhat in belligerent countries.

In the final part of the paper, we narrow the focus of our analysis to study causal effects. We exploit a key difference between interstate wars and other types of war. Prior to the onset of interstate wars, output and inflation do not exhibit systematic changes. In contrast, other wars are preceded by rising inflation and slowing output growth, suggesting prima facie that these wars may be the result, and not just the cause, of economic fluctuations. Restricting the sample to interstate wars, we identify narratively, for each individual case and based on a variety of sources, the *casus belli*, or the primary causes and motives behind each war. The large majority of interstate wars are linked to nationalist, ideological, or historical causes that are plausibly exogenous to the state of the business cycle. Economic motivations that are relevant in certain cases—for instance, in disputes over natural resources or wars of colonial expansion, as famously argued by Lenin (1917)—appear orthogonal to the business cycle, insofar as they relate to medium- to long-term economic objectives. In total, we find only six interstate wars for which shorter-term economic motivations emerge as important from the historical record. To avoid related endogeneity issues we drop these from the sample.

We then formally test whether wars are Granger-caused by lags of output growth and inflation in our restricted sample of interstate wars and find that they are not—unlike in the case of other wars. When we re-run our baseline regressions on the restricted sample, the estimates remain remarkably similar to those obtained for the unrestricted full sample. This lends support to a causal interpretation, whereby war causes the business cycle dynamics we have documented, subject to a few remaining caveats discussed below.

The paper is structured as follows. The remainder of this section clarifies the contribution of our study in the context of the existing literature. Section 2 details the construction of the data set, notably the specification of war sites. Section 3 introduces our empirical framework and presents the main results on how economies (and societies) change in the context of war. It also summarizes the results from an extensive robustness analysis, detailed in the online appendix. In Section 4, we move toward establishing causality. Section 5 concludes.

Related literature. First, there is work on the fiscal and monetary implications of specific wars (e.g., Oliver, 1941; Rockoff, 1984; Hall and Sargent, 2021) and their costs in the belligerent countries (e.g., Davis and Weinstein, 2002; Broadberry and Harrison, 2005; Rockoff, 2012). There are also narrative histories of the economy in the two World Wars and various conflicts during the Cold War (e.g., Harrison, 1998; Westad, 2005; Eichengreen, 2008; Tooze and Fertik, 2014). Our study is less rich in detail, aiming instead for a broader perspective.

Second, there are studies of how war affects growth (Barro and Lee, 1994; Acemoglu, Johnson and Robinson, 2005).[1] Cerra and Saxena (2008) document sizeable output losses due to civil wars but find them to be less persistent than in the case of banking and financial crises. Limiting the analysis to belligerent countries, Caplan (2002) distinguishes the growth effect of domestic and foreign wars: domestic wars lower growth, whereas wars fought abroad are mildly expansionary. Likewise, Chupilkin and Kóczán (2022) document that wars on a country's territory reduce economic activity. Auray and Eyquem (2019) estimate a DSGE model on time series data for the two World Wars. Our analysis is broader in scope, notably by also considering the spillovers of war.

A third strand of the literature investigates the adverse impact of war on trade and production networks (Qureshi, 2013; Couttenier, Monnet and Piemontese, 2022; Korovkin and Makarin, 2023). Our results are consistent with the findings of this literature, although we do not constrain spillovers to operate only via trade. Complementary work relates the probability of conflict to trade and trade agreements (Martin, Mayer and Thoenig, 2008, 2012; Lee and Pyun, 2016).[2]

A fourth strand links conflict spillovers to geographic distance, with a distinct focus on civil war and ethnic conflict (Murdoch and Sandler, 2002, 2004; Mueller, Rohner and Schönholzer, 2022). Finally, the market response to conflict has been analyzed in some detail, also with a view to the role of geographic distance (Leigh, Wolfers and Zitzewitz, 2003; Guidolin and La Ferrara, 2007; Zussman, Zussman and Nielsen, 2008; Verdickt, 2020; Caldara and Iacoviello, 2022; Federle et al., 2025). While our focus is on macroeconomic spillovers, we also consider how equity prices and long-term interest rates behave.

[1] Participation in external wars does not seem to affect growth, but conflict more broadly defined harms economic activity (see, for instance, Abadie and Gardeazabal, 2003; Novta and Pugacheva, 2021; de Groot et al., 2022), specifically in the case of very large and global wars (Rasler and Thompson, 1985; Thies and Baum, 2020). Blomberg and Hess (2012) document that consumption drops strongly in response even to small wars, whether initiated at home or abroad.

[2] Konrad and Morath (2025) emphasize in their theory of alliance formation that the collateral damage of war is greatest in front-line states—those most at risk of becoming war sites.

2 Data and basic facts

We study the economic effects of war using annual data from 1870 to 2023. The start of our sample is determined by the availability of comprehensive time-series data on macroeconomic outcomes, which we examine for an unbalanced panel of 60 countries. The wars in our analysis, however, are not restricted to involve these countries, as we also investigate the economic spillovers of war. In this section, we introduce our data and present some basic facts.

2.1 War sites

In our analysis, the notion of the "war site" takes center stage. We define it as a country that experiences military action on its own soil. Following the Correlates of War (COW) project (Sarkees and Wayman, 2010; Dixon and Sarkees, 2016), we distinguish between different types of wars: those fought between states, that is, *interstate* wars and *other* wars. The latter category includes *intrastate* wars, such as civil wars, and *extrastate* wars, such as colonial wars and wars of independence. Based on this classification, we geolocate the war sites for each war in our sample, as we explain in detail below.

COW defines an interstate war as combat between the armed forces of at least two states that causes a minimum of 1,000 battle-related combatant deaths within any twelve-month period and provides data on interstate wars for the period from 1816 to 2007. For the more recent years in our sample period, there are no interstate wars that meet this criterion except for the Russian invasion of Ukraine in 2022. We verify this using the database of the *Uppsala Conflict Data Program* (UCDP), see Gleditsch et al. (2002); Davies, Pettersson and Öberg (2022).

However, while COW provides information on the belligerents of each war, it does not specify in which country a given war was fought. In order to identify the war sites among the belligerent countries of interstate wars, we proceed in two steps. First, we disaggregate wars to the battle level based on information in the warfare encyclopedia by Clodfelter (2017) and a number of other historical sources. As a result, we are able to identify 1,737 different battles for which we code geolocations.[3] Using the same sources, we obtain—for each battle—estimates for the number of casualties. These include the number of dead, missing, wounded, and prisoners of war captured in the battles. The largest

[3]In some instances, the available information is less granular than what we would ideally like to have. For instance, for the Kargil war, we only have aggregate numbers of casualties for the border area of Pakistan and India.

battle in our sample is the Brusilov Offensive during World War I, which is associated with more than 1 million casualties. Other well-known battles, such as the Battle of Wuhan and the Battle of Verdun, also rank among the bloodiest in our sample. We aggregate battle-level casualties to the country level, thereby identifying war-site countries based on present-day borders.[4] In aggregating to the country level, we follow Conte, Cotterlaz and Mayer (2022) and use ISO3 codes as our primary classification system, supplemented by CIA World Factbook territorial definitions in case geographic boundaries or state membership are ambiguous.

To measure the intensity of war in war site j, $Site_{j,t}$, we divide the number of casualties by the population of the war site prior to the start of the war. Note that casualties are not confined to the local population and that we compute casualties for the entire war such that $Site_{j,t}$ is a forward-looking measure, comparable to the fiscal news shocks compiled by Ramey (2011). Consider the Gulf War as an example of how we determine war sites. The conflict began in 1990 with Iraq's invasion of Kuwait. In 1991, a U.S.-led coalition intervened, shifting much of the fighting to Iraqi territory. Next to Saudi Arabia—particularly during the Battle of Khafji—Israel also became target of limited military action.[5] Accordingly, Iraq, Kuwait, Saudi Arabia, and Israel are classified as the war sites of the Gulf War.

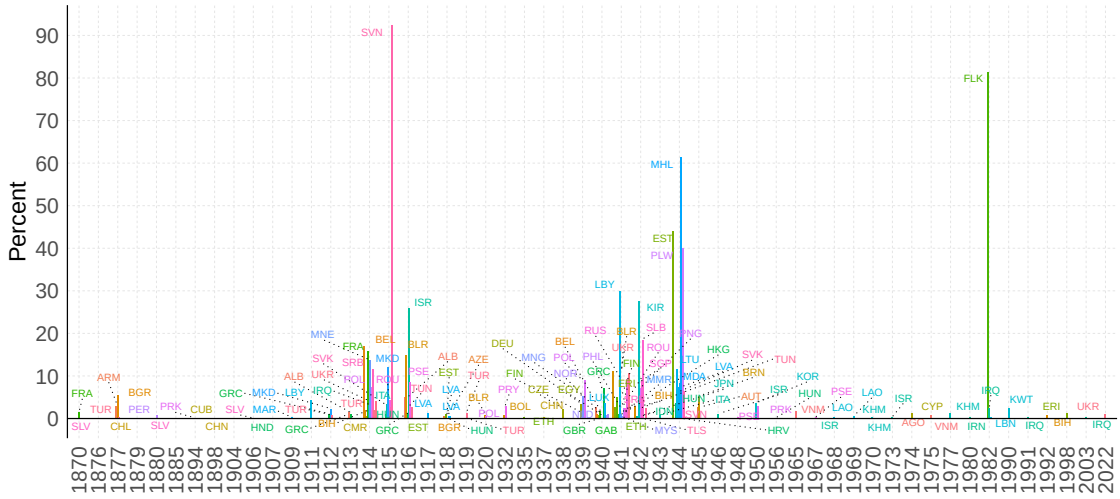
The top panel of Figure 2 illustrates how $Site_{j,t}$ evolves over time for interstate wars, showing only those cases where the number of casualties exceeds 0.1 percent of the pre-war population. Unsurprisingly, the most severe cases are clustered around the two World Wars. Since casualties are not restricted to the local population but are scaled relative to it, small countries may at times suffer very intensive wars according to our measure. This is plausible insofar as wars in which foreign armies fight ferociously on a smaller country's soil are likely to be particularly devastating. Examples include Belgium in World War I and Poland in World War II. The single most severe war according to this measure occurred in Slovenia, which hosted a series of very bloody battles between Italy and the Austro-Hungarian Empire during World War I, particularly along the Isonzo Front. Another example includes the Falklands War of 1982, which involved troops of Argentina and the United Kingdom that together exceeded

[4] In case a battlefield extends over the territory of several countries, we assign the casualties in equal shares to all countries.

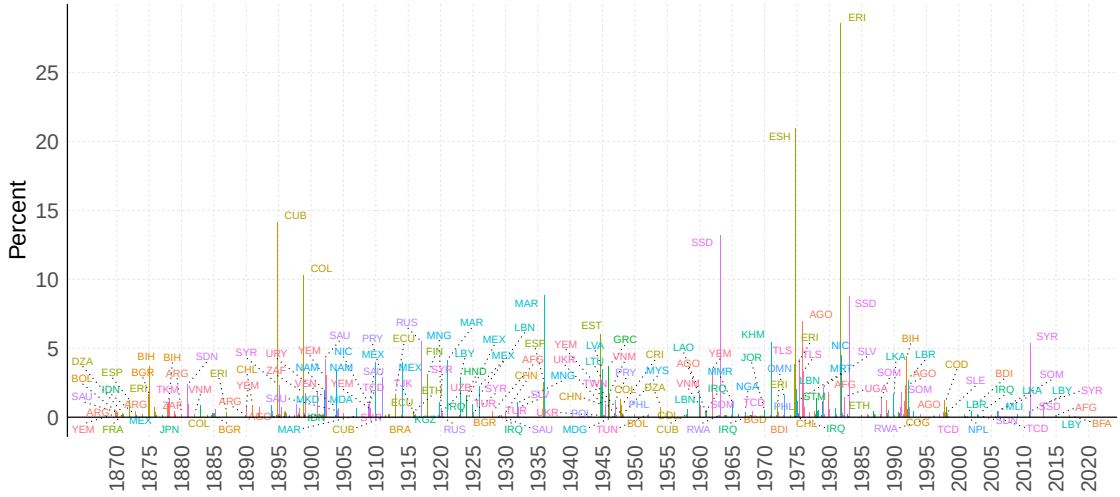
[5] In retaliation for the coalition's military actions, Iraqi President Saddam Hussein launched 42 Scud missiles at Israeli cities. The example shows that in some cases, war sites only experience limited destruction. Our quantitative measure of war intensity accommodates for this. Additionally, Section 3.3 shows that the results are unchanged when we exclude sites with fewer than 1,000 casualties; Israel and Saudi Arabia drop out under that threshold.

Figure 2: War sites

(a) Interstate wars



(b) Other wars



Note: Panels display $Site_{j,t}$: total casualties (dead, missing, wounded, prisoners of war) in war site j measured in percent of pre-war population of war site country. Year t refers to start of the war. Figure restricted to war sites with casualties exceeding 0.1% of local pre-war population.

the population of the Falklands Islands severalfold. In our analysis below, we thus need to control for country size when we study the spillovers from war in order to capture the notion that, even in cases where casualties relative to the local population are very high, spillovers to third countries may be limited if the war site is a small country.

As a caveat, we note that the number of human casualties, like other candidate statistics, is an imperfect measure of the intensity of war—whose crucial aspect is, above all, its destructive nature. As the nature of warfare keeps changing

over time, the association between casualties and the overall severity of war is also subject to change. We will revisit this issue below and provide robustness analyses employing an alternative measure of war intensity. A second concern is that countries' borders are subject to change, notably in the wake of wars. Against this background, we emphasize that our country classifications rely on present-day borders so that we can study the macroeconomic outcomes associated with war in a geographically consistent manner.[6] This also rules out the possibility that changes in our outcome variables (e.g., real GDP growth) are confounded by territorial changes.

To exemplify the issue, consider the Italian-Turkish War of 1911. It was fought between the Ottoman Empire and Italy but major warfare mostly took place in modern-day Libya rather than in Turkey or Italy. Since our macroeconomic indicators consistently refer to modern-day national borders, we code Libya as the war site and modern-day Italy and Turkey as belligerents. Below, we present a robustness analysis using a subsample of wars that did not entail any border changes. The U.S. also experienced combat on its own soil during World War II: There were several battles on the Aleutian Islands, a group of islands belonging to Alaska, as well as the Japanese attack on Pearl Harbor in Hawaii, and combat on U.S. territory in the Pacific. Such isolated incidents are unlikely to cause meaningful economic effects. In our baseline, we thus drop all naval battles as well as those battles taking place on remote islands when defining countries as war sites. In this way, we focus on countries experiencing material destruction on their core territory. We list all war sites in Table O-A.1 in the Online Appendix.

We further cross-check our war-site coding of interstate wars by consulting GPT-4. As a large language model, it is trained on huge corpora of texts, including historical accounts of wars. We leverage this fact and systematically consult the GPT-4 API to identify countries in which major battles took place and compare the outcomes with our own coding.[7] The Pearson correlation with our coding is 0.73 and highly significant (p-value < 0.001). In total, GPT-4

[6] Using current country definitions and borders to ensure geographical *consistency* is common practice when assembling historical economic data—see, for example, the *Jordà-Schularick-Taylor Macrohistory Database* (Jordà, Schularick and Taylor, 2017) or the *Global Macro Database* (Müller et al., 2025)—as well as when compiling institutional indicators, such as the *Varieties of Democracy* (V-Dem) project (Coppedge et al., 2025).

[7] For each war, we ask GPT-4 "*Which countries suffered major battles on their own territory during the war '*' which started in *? Consider modern-day borders. Specifically, even if a state did not exist at the time of the war, refer to it by its current name within today's borders. For example, if there was a war in 1870 within modern-day Libya, please refer to it as having taken place in Libya instead of referring to it as the Ottoman Empire. It is crucial that you only provide the ISO-3 codes of the countries and nothing else, as your response is being parsed as a CSV.*" Parameters of GPT-4 requests were: temperature (0), max_tokens (256), top_p (1), frequency_penalty (0), presence_penalty (0).

identifies 58 countries as war sites that we had not previously identified in our initial coding. Because large language models may hallucinate, we systematically searched for corroborating evidence on these countries and were able to find documentation of actual fighting in five of the proposed additional war sites. We include these countries in our war-site coding, see Table O-A.2.

We further distinguish war-site countries from "belligerents" and "third countries." Given the information available in COW, this is straightforward. We define belligerents as countries that are parties to the war but not war sites. Third countries, by contrast, are those that are not parties to the war. Consider again the Gulf War as an example: in this case, we classify the U.S. and 10 more countries that formed the international coalition as belligerents.

In addition to interstate wars, we consider wars within states and wars of states against non-state actors ("other wars", for short). These wars comprise, in particular, civil wars, colonial wars, and wars for independence. While locating war sites is less involved in this case, some of these wars are internationalized as foreign states intervene to support factions in an ongoing intrastate war.[8] To account for these different parties, we again distinguish between war sites, belligerents, and third countries. For intrastate wars, the geocoding is straightforward. Extrastate wars comprise wars for independence, organized revolts, and clearly circumscribed occupations and are therefore also centered around a well-defined country. Still, to geolocate the actual war site, we proceed on a case-by-case basis, drawing on various sources, including Sarkees and Wayman (2010), Dixon and Sarkees (2016), and Clodfelter (2017).

We measure the intensity of other wars, just as with interstate wars, by the number of casualties relative to the local population.[9] We show the war-intensity measure for other wars in the bottom panel of Figure 2. Comparing interstate wars in the top panel and other wars in the bottom panel, we observe that the sample contains significantly more other wars, most of which exhibit much lower intensity levels. Notable exceptions include highly destructive colonial and secessionist wars such as the Second Spanish-Cuban War (1895–1898) where Spain deployed 180,000 troops to suppress the Cuban rebellion, the Western Saharan War (1975–1983) where Morocco deployed over 100,000 troops in a

[8] According to COW, intrastate wars are internationalized when an outside state intervenes. If the intervening state takes over "the bulk of the fighting", the war transforms into an interstate war.

[9] For these other wars, we obtain data on the number of battle fatalities from COW, follow their convention by imposing a wounded-to-death ratio of 3:1, and assume that casualties are proportionally distributed across them.

territory of fewer than 100,000 inhabitants, and the Tigrean and Eritrean War (1982–1991) where Ethiopia deployed up to 430,000 troops. Recall, that just like in the case of interstate wars, we measure casualties relative to the pre-war population of the war site.[10] Note that other wars exhibit less clustering than interstate wars, though there is a noticeable trend toward increasingly intense wars in the latter half of the 20th century. In our estimation below we pool data from interstate and other wars, but we verify that the results also hold within each subsample.

2.2 Descriptive statistics

Below, we document how economic outcomes and, more broadly, societies change in the context of war—not only in the war site but also in belligerent and third countries. Our analysis is based on an unbalanced panel of 60 countries, which together accounted for 95% of global GDP in 1960, with coverage remaining between 89% and 92% throughout the period from 2000 to 2023. We provide details on the data sources in Appendix A. The outcome variables are typically adjusted by the original sources from historical to current territorial definitions (as defined by the ISO state system); see, for instance, the methodological discussion in Maddison (1995). The sources report output in per capita terms. For our analysis, we compute an aggregate output measure to account for changes in the population during wars. To do this, we rely on population data for the territories that define countries today (Bolt and Van Zanden, 2014; HYDE et al., 2025).[11] We source bilateral trade data from Fouquin and Hugot (2016) and impute missing values using a gravity model which has a pseudo-R^2 of 0.95; see Appendix B for more details.

Table 1 presents summary statistics in relation to macroeconomic variables for interstate wars in Panel A, other wars in Panel B, and the combined sample—which constitutes our baseline—in Panel C. The total sample comprises 694 wars, that is, country-year observations marking the onset of a war in a given war site. About one-third of these are interstate wars. The top panel of Figure 3 offers a systematic comparison of interstate and other wars, displaying histograms of the log10-transformation of our casualties to local population measure, $Site_{j,t}$, along

[10] The intensity of these wars also reflects long durations, with most lasting 7–10 years, allowing casualties to accumulate relative to the local pre-war population base. On the other hand, our measure adequately classifies severe civil wars such as the Thousand Days' War (1899–1902) which was the deadliest and most destructive civil war in the history of Colombia.

[11] Although Bolt and Van Zanden (2014) mostly refer to 1998 boundaries, these have only changed to a small extent since (Schvitz et al., 2022).

Table 1: Summary statistics for war sites

Wars	Casualties / population (in %)		Length		Macro time series for...		
Total	Mean	Median	Mean	Median	Sites	Belligerents	Third
Panel A: Interstate wars							
225	3.50	0.26	2.5	2	84	178	3,648
Panel B: Other wars							
469	0.74	0.13	3.3	2	141	164	6,788
Panel C: Combined sample							
694	1.65	0.15	3.0	2	220	319	7,126

Note: Length denotes number of years that the wars span. Macro time series refers to availability of data on both local pre-war population and consumer prices in years of war onsets across sites, belligerents, and third countries. For coverage of other variables, see Table A.1 in the Appendix. As countries may become sites, belligerents, or third countries to multiple distinct wars simultaneously, numbers in macro time series of panel A and B do not necessarily add up to those outlined in Panel C.

the horizontal axis.[12] While other wars occur more frequently than interstate wars, the distribution for interstate wars is broader and more skewed to the right.[13] In terms of duration, both types of wars have the same median of spanning two years, but the mean is considerably longer for other wars, indicating a higher frequency of protracted lower-intensity conflicts.

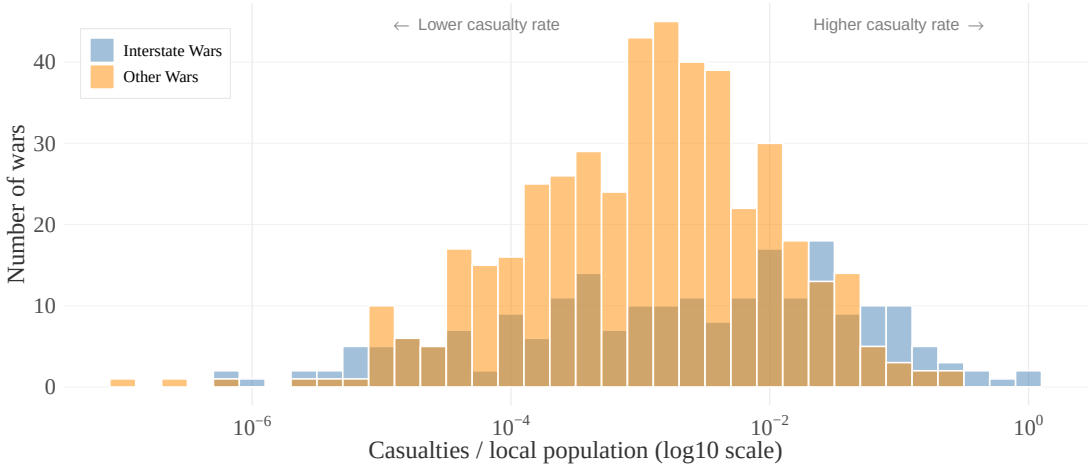
Due to limited data availability for outcome variables, there are only 84 country-year observations for the start of interstate wars where data are available for the war site, along with 178 observations for belligerent countries and 3,648 for third countries. The numbers for other wars are higher and the full sample features 220 country-year observations for war sites, along with 319 corresponding observations for belligerents and 7,126 for third countries. We detail the data coverage across variables in Appendix Table A.1; data sources and definitions are provided in Online Appendix Table O-A.3. In our sample, war sites are broadly distributed across the globe, see Figure A.1 in the Appendix. A few countries, including the U.S. and Sweden, stand out for never having been a war site in our sample. In the case of the U.S., this reflects our choice to exclude casualties in

[12]This visualization uses a logarithmic scale to compress extreme values and reveal the bulk of the distribution: by *pulling in* the long tail, most conflicts are clustered at lower intensity, while still preserving the few extreme outliers. For an early application of the log10-transformation in visualizing historical warfare, see Richardson (1960, Ch. IV). Furthermore, the Kolmogorov-Smirnov test rejects the null hypothesis that interstate and other wars are drawn from the same casualty distribution ($D = 0.212$, $p < 0.001$), suggesting that these conflict types are different.

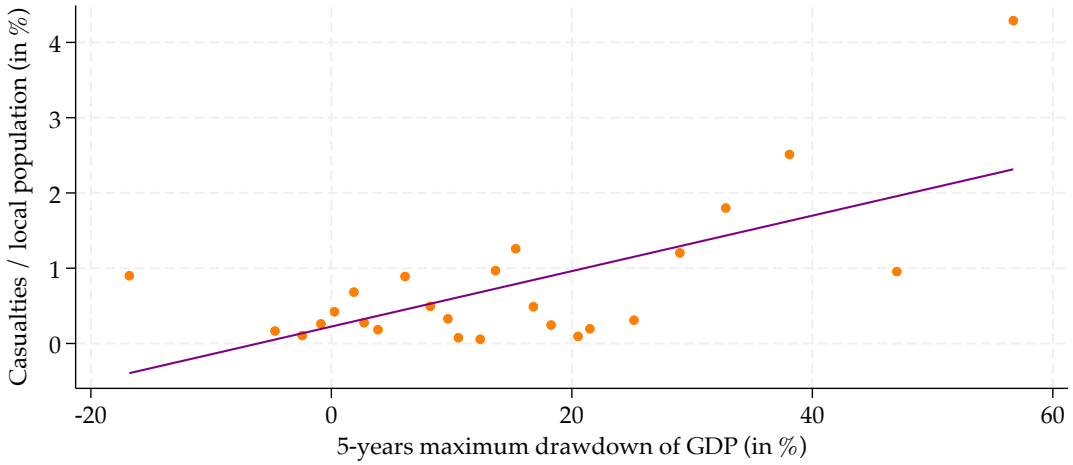
[13]In our analyses below we provide several robustness tests showing that our results are neither driven by the tails in the distribution of war intensity nor by single events.

Figure 3: War sites and economic activity

(a) Distributions of war casualties relative to local population



(b) Correlation with GDP



Note: Top panel shows histograms of log10-transformed $Site_{j,t}$. Bottom panel shows binned scatter plot relating $Site_{j,t}$ to maximum drawdown of GDP after removal of country-specific linear trend in war sites 5 years after war onset.

remote island territories when geolocating war sites.

Finally, we relate our measure for the intensity of war to the maximum loss in output in the 5-year period after the start of the war. Panel (b) of Figure 3 shows a binned scatter plot for all war sites in our sample that juxtaposes the war intensity, measured on the vertical axis, and the maximum loss in output on the horizontal axis. As becomes apparent from the figure, both measures appear to be strongly correlated in a linear fashion: As casualties relative to the local population—our default measure of a war's intensity—increase, so does the decline of output in the war site.

3 Economic dynamics after the outbreak of war

In this section, we document the economic developments following the outbreak of war, contrasting dynamics in the war site, in belligerent countries, and in third countries. Zooming in on third countries, we further examine how these developments vary with a country's exposure to the war site. While our primary focus is on economic outcomes, we also present results for a range of additional indicators, including institutional outcomes like electoral fairness and judicial independence. We begin by introducing our empirical framework and then present results across a variety of variables and specifications. Throughout this section, our goal is to establish the economic patterns that emerge following war onset, without (yet) making any causal claims.

3.1 Empirical framework

We estimate a variant of local projections, following Jordà (2005), that accommodates our specific interest, namely to measure the dynamics of economic (and other) variables in response to wars depending on the intensity of war and the exposure of other countries to the war site:

$$y_{i,t+h} - y_{i,t-1} = \zeta_h' \mathbf{X}_{i,t} + \sum_{c \in \{S,B,T\}} \phi_{c,h}(\gamma_{i,c,t}' \cdot \mathbf{Sites_t}) + \sum_{c \in \{B,T\}} \psi_{c,h}(\epsilon_{i,c,t-1}' \cdot \mathbf{Sites_t}) + u_{i,t+h}\,. \quad (3.1)$$

Here $y_{i,t}$ is an outcome variable such as real GDP, with $i \in N$ indexing countries. Matrix $\mathbf{X}_{i,t}$ denotes the control variables comprising four lags of the outcome variable in first differences to capture short-term trends, lags of the other regressors, country fixed effects, and a term that assumes a value of one if country i shares a common border with the war site.[14] ζ_h is a vector of parameters.

The vector $\mathbf{Sites_t}$ features $Site_{j,t}$ as its elements which, as explained in the previous section, are non-zero—measuring the intensity of the respective war—whenever a war starts on the soil of country j in year t, and zero otherwise. The term $\gamma_{i,c,t}' \cdot \mathbf{Sites_t}$, in turn, captures the relation of country i to the war sites in year t. It is assigned, relative to each war site j, to one of the three categories S (site), B (belligerent), or T (third country) based on the information detailed in

[14] It does not include time fixed effects because our goal is to capture the full economic dynamics after the start of the war, rather than only the dynamics in specific country groups relative to the average.

the previous section. Thus, the loading on the site vector, $\gamma_{i,c,t}$, is country- and category-specific in each year: If country i is a war site in t, vector $\gamma_{i,S,t}$ features a one in row i, and zeros elsewhere. If country i is a belligerent, there are ones in all rows of $\gamma_{i,B,t}$ relating to countries that become a site in a war that country i also participates in without being itself a war site; other rows have zeros. Finally, if country i is not a party to a war (i.e., it is a third country) that starts in year t, $\gamma_{i,T,t}$ features ones in all rows relating to countries that become a war site, and zeros elsewhere. Note that these categories are not exclusive. For instance, a country may be a war site or a belligerent in one war and at the same time a third party to other wars. Specification (3.1) accommodates this possibility.

Parameter $\phi_{c,h}$ captures the dynamics of the outcome variable from the start of the war through year h for each category of countries. The outcome variable is specified in differences relative to the pre-war level to account for the possibility that it changes permanently in the wake of the war (Stock and Watson, 2018; Ben Zeev, Ramey and Zubairy, 2023). In this way, because of country-fixed effects, we also accommodate linear country-specific trends in the outcome variable.

When it comes to the war-related dynamics in belligerent and third countries, the estimates of parameter $\phi_{c,h}$ relate to an aggregate war-site measure, i.e., they capture the behavior of the outcome variable in response to some *overall* intensity of war across different sites in a given year t. However, economic developments during a war may depend on a country's specific exposure to a given war site. For instance, being exposed to a war may have greater repercussions when it occurs in a large country, in a nearby country, or in a country with which there is a close economic relationship. To account for this possibility, we introduce the exposure vector, $\epsilon_{i,c,t-1}$. It measures the strength of the connection between country i and war site j, and hence allows the exposure of i to war in j to be scaled up or down as a function of specific variables we introduce below. These exposure variables are dated $t-1$ so as not to be affected by the start of the war itself.[15] The parameters $\psi_{c,h}$ capture how—after the start of a war—the dynamics in both belligerents and third countries depend on their exposure to the war site. To capture war-related spillover risks in addition to mere economic exposure, the control matrix $\mathbf{X}_{i,t}$ in the specification above also includes a variable that measures the intensity of all wars in countries that share a common border. Finally, $u_{i,t+h}$ denotes the error term.

[15] In cases where multiple different wars take place in the same country, other countries may be a third (non-participating) country to one war while at the same time being a belligerent to the other war. In these cases, the loadings γ and ϵ are scaled proportionally to the intensity of each war.

3.2 Results

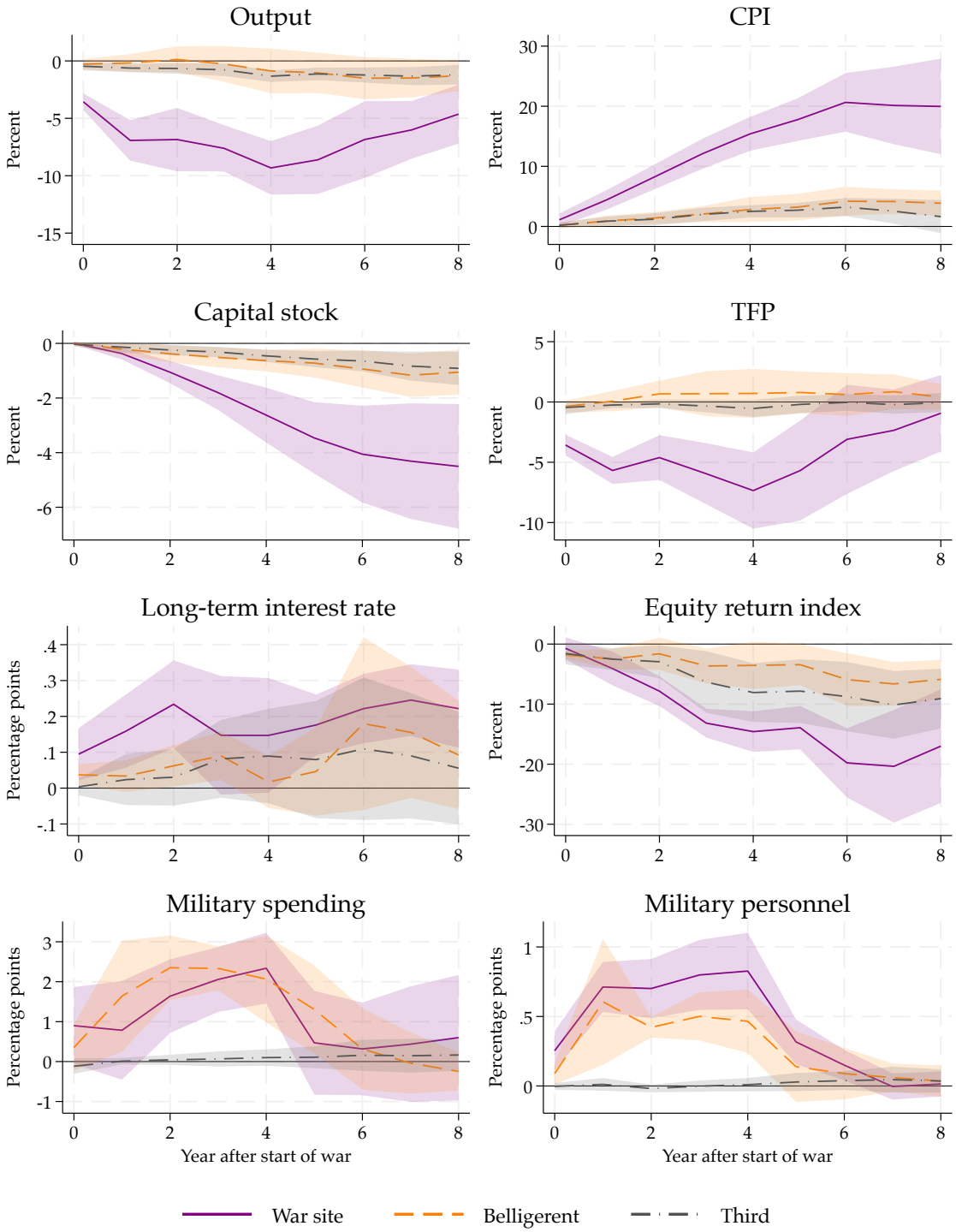
Figure 4 presents our estimates of specification (3.1) for the full sample that includes all wars—contrasting macroeconomic dynamics following the onset of war across war sites, belligerent countries, and third countries. Here and in what follows, we show results assuming an *intensity of war* of 2 percent, measured by the number of casualties relative to the war site's pre-war population (sample average: 2 percent). In each panel, the horizontal axis measures the time in years since the start of the war ($h = 0, \ldots 8$). The vertical axis measures the deviation of a variable from its pre-war level, either in percent or in percentage points. The solid (purple) line represents the outcome for the war site, the dashed (yellow) line for belligerent countries, and the dash-dotted (black) line for third countries. Throughout, shaded areas indicate 90% confidence intervals, computed using standard errors that are robust to heteroskedasticity as well as serial and cross-sectional correlation (Driscoll and Kraay, 1998).

To set the stage, we let the *size* of the war site—measured by its share of world population—be the sole determinant of potential spillovers to other countries, abstracting from any heterogeneity in the exposure to the war site within the belligerents and third countries. Formally, we specify the entries of the exposure vector as the share of a war site's population in the world population; that is, for each country i we set entry j of vector $\epsilon_{i,c,t-1} = Pop_{j,t-1}/Pop_{t-1}^{Global}$. In Figure 4, we show results assuming that the size of the war site amounts to 3 percent of the world population (sample average: 3 percent). Note that, while the *intensity of war* (linearly) scales both the dynamics within the war site and the spillovers to other countries, the *size of the war site* does so only for the latter.

Starting with the upper-left panel of Figure 4, we observe strikingly adverse output dynamics in the war-site economy, with a trough about four years after the start of the war when output has declined by nearly 10 percent (relative to the pre-war trend). What's more, the subsequent recovery is slow. In year $h = 8$, output is still lower by about 5 percent, and it takes about 12 years for a complete recovery, as we show in our robustness analysis below. This is also noteworthy in light of the fact that the mean (median) duration of wars spans "only" 3 (2) years. At the same time, we note that GDP does not deviate from its trend in the average belligerent country and only moderately so in the average third country. We revisit this issue below when we allow for heterogeneity in countries' exposure to the war site.

Turning to the top-right panel, we also observe strong inflationary tendencies for the war site: the price level increases steadily and, after six years from the

Figure 4: Macroeconomic dynamics following the start of war



Notes: Intensity of war set to 2 percent (casualties/population); size of war site to 3 percent (war-site population/world population). Solid (purple) line represents war site, dashed (yellow) belligerent, dotted (black) line third country. Vertical axis measures deviation from pre-war level/trend, measured in percent or percentage points of pre-war output (military spending) or of pre-war population (military personnel). Shaded areas indicate 90% confidence intervals, computed using standard errors that are robust to heteroskedasticity as well as serial and cross-sectional correlation. Detailed information on underlying sample provided in Appendix Table A.1.

onset of the war, peaks at some 20 percent above trend. The change in inflation dynamics is not confined to the war site: price levels also tend to rise somewhat faster in third countries and belligerents—consistent with the notion that wars impart a broader inflationary impact.

A salient feature of war, in addition to human casualties, is physical destruction. Against this background, we estimate the response of the capital stock (left panel in the second row). In the war site, it declines by more than 4 percent within eight years from the start of the war. It also declines significantly in the two other groups of countries, though to a much lesser extent. We emphasize, however, that a declining capital stock may not only reflect physical destruction but also reduced investment.[16]

Next, we estimate how TFP evolves after the start of the war. The results (second row, right panel) show that TFP declines immediately and sharply in the war site, consistent with the notion that wars are highly disruptive to the functioning of the economy. This finding also aligns with the notion that a shift of employment to the military sector lowers the productive capacity of the economy, as documented in the classic study of Ramey and Shapiro (1998). That said, we find a TFP decline only in the war-site economy, not for belligerent or third countries. In fact, there is a mild increase of TFP in the belligerent countries, although it is not statistically significant.

Turning to financial variables, wars turn out to be associated with an increase in long-term interest rates in the war-site economy, as we show in the left panel of the third row. The 10-year government bond yield increases by about 0.3 percentage points on average and remains elevated up until 12 years after the start of the war. This path for long-term interest rates—while still moderate in scale—reflects the scarcity of resources during war. In belligerent and third countries we also observe a modest rise in interest rates, though these coefficients are not statistically significant.

Economic disruption and higher interest rates typically weigh on equity markets by reducing the present value of future cash flows. Consistent with this, the right panel of the third row shows that the war site's equity index declines persistently. The cumulative under-performance (relative to trend) reaches 20 percent after six years. The initial response, however, is weak—suggesting that markets do not anticipate fully the adverse developments in the context of war (Ferguson, 2008). Compared to war sites, stock market declines in third countries

[16]See the working paper version of this paper for a model-based interpretation (Federle et al., 2024).

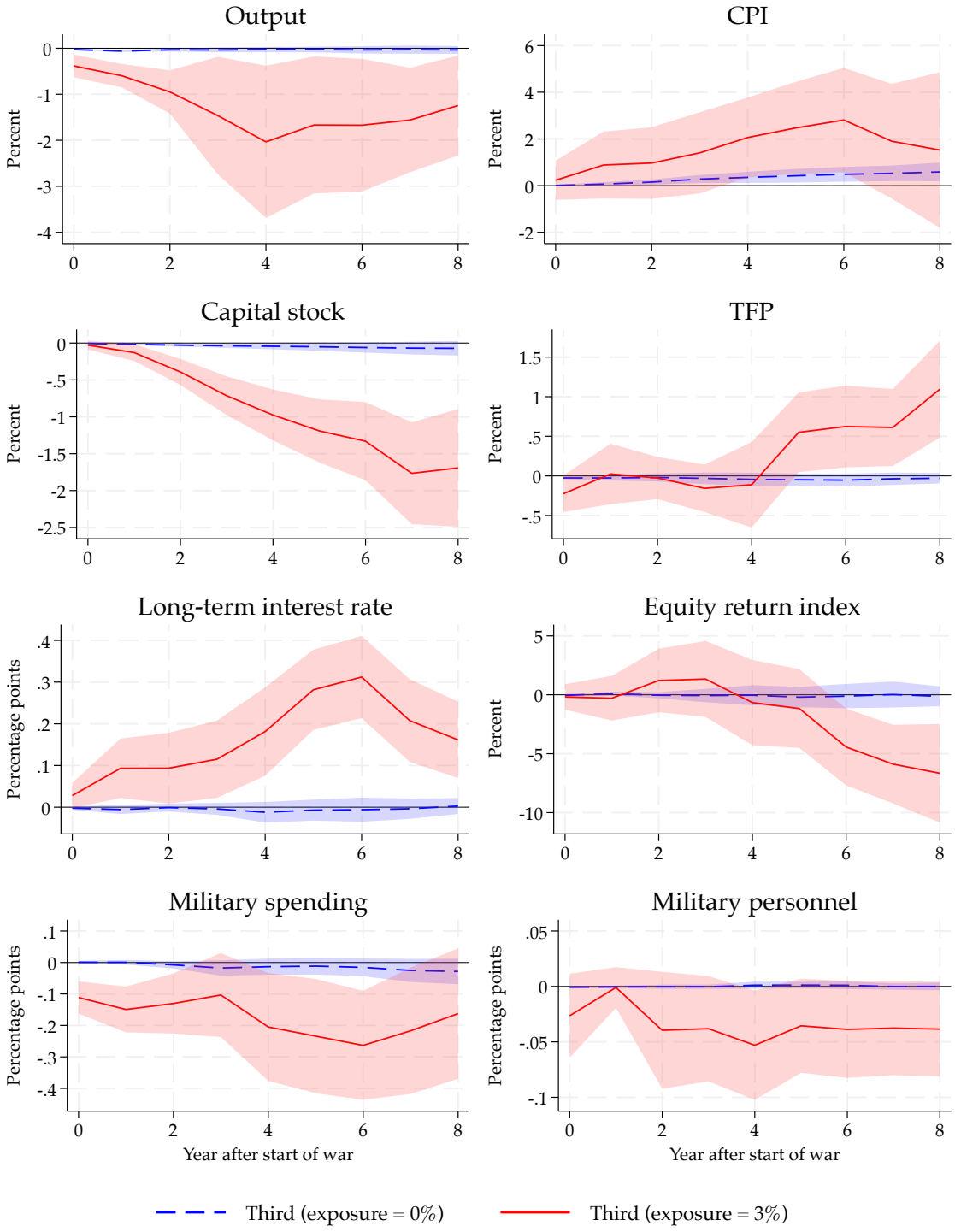
are less than half the size and less statistically significant, while belligerent countries show the most resilient equity market performance.

The apparent resilience of belligerent countries in terms of equity returns and TFP brings to mind recent work showing that government defense spending can stimulate innovation (Antolin-Diaz and Surico, 2025; Ilzetzki, 2024). Indeed, military spending is seen to increase markedly from the start of the war in both war-site and belligerent countries, as shown in the last row (left panel). Interestingly, the increase is of similar size and almost perfectly synchronized between war sites and belligerents. Spending increases initially by about 1 percentage point of GDP and continues to rise up until year four, peaking at 2.5 percentage points of GDP. This pattern is in stark contrast with the flat profile of military spending in third countries, which do not participate in the war. Prima facie, the evidence suggests that belligerent economies derive some of the positive economic side effects of higher military spending—stable to higher output and superior TFP performance—without suffering the obvious downsides from war apparent in the war-site economy.

In the final panel (bottom-right of Figure 4) we consider a related variable, namely the scale of military personnel used from the onset of the war. Again, we find very similar and significant dynamics for the war site and for belligerents: in both, nearly one additional percentage point of the population is recruited by the military over the first four years. The adjustment over time mimics the pattern for military spending. Again, there is no significant change in third countries.

So far, we have documented how macroeconomic dynamics following the start of the war differ across war sites, belligerents, and third countries. For most of the variables considered, notably output and inflation, we find larger (and more adverse) changes for the war site. Only in terms of military spending and personnel do war sites and belligerents look similar—and distinct from third countries—as seems natural in light of their different roles with respect to the war. However plausible these average dynamics appear, they may still mask sizable heterogeneity across countries. To investigate this, we turn to Figure 5. It focuses on third countries (which do not participate in the war) and shows estimates that allow economic dynamics to vary depending on countries' individual exposure to the war site. Specifically, where the exposure vector $\epsilon_{i,c,t-1}$ previously just captured a war site's size relative to the world, it now reflects the strength of pre-war trade connections with individual third countries, as proxied by the imports of country i from the war site in the year prior to the start of the war,

Figure 5: Dynamics in third countries by exposure to war site



Notes: Intensity of war set to 2 percent (casualties/population); trade exposure (imports from war site(s)/GDP) 3 percent (solid red line) v zero (dashed blue line). Vertical axis measures deviation from pre-war level/trend, measured in percent or percentage points of pre-war output (military spending) or of pre-war population (military personnel). Shaded areas indicate 90% confidence intervals, computed using standard errors that are robust with respect to heteroskedasticity as well as serial and cross-sectional correlation. Detailed information on underlying sample provided in Appendix Table A.1.

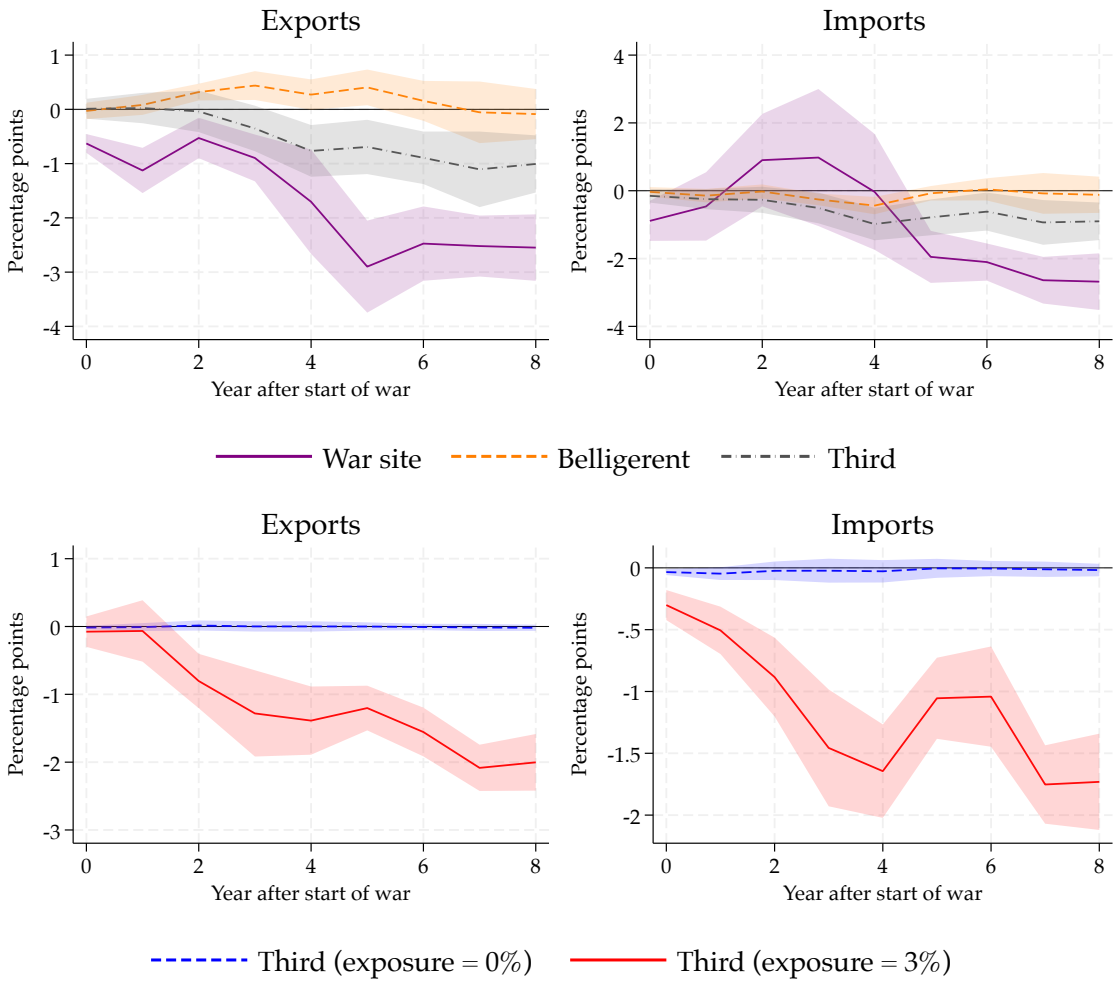
measured relative to the pre-war GDP of country i.

To summarize our results, we show responses for a hypothetical scenario without any pre-existing trade exposure, shown by the dashed (blue) line, and an opposite scenario where trade exposure is high (defined as imports from the war site at 3 percent of the third country's GDP), shown by the solid (red) line. To put this into perspective, in 2023 imports from France accounted, on average, for about 2 percent of Germany's GDP; and imports from the U.S. for 15 percent of Canada's GDP. Importantly, by measuring a country's exposure to the war site on the basis of imports, we indirectly account for the economic size of the war site, too.

The panels of Figure 5 are organized in the same way as in Figure 4 above and reveal a distinct pattern: Third countries with (trade-related) exposure to the war site experience notable changes in macroeconomic dynamics after the start of the war, whereas such changes are largely absent in countries without exposure. Consider output (first row), where we only observe a significant decline in exposed countries. Price levels rise across both types of economies, but the increase is sizeable for exposed economies and negligible for others. This striking divergence carries over to the dynamics of the capital stock, while the change in TFP is generally small (second row). Similarly, interest rates rise, and the return on equity declines in third countries with high exposure to the war site, but not elsewhere (third row). Some differences are apparent even for military spending and personnel, shown in the bottom row. Both series decline somewhat in countries with high trade exposure to the war site and not in other third countries. While this may appear surprising at first glance, recall that our specification controls for a common border with the war site. A decline in military spending in countries with a high trade exposure to the war site may thus simply reflect that fiscal capacity declines due to the recessionary spillovers.

Overall, the pattern in Figure 5 supports the notion that third countries, which do not participate in the war, nonetheless suffer significant adverse spillovers from the war-site economy, provided that there are strong trade linkages. To shed further light on this finding, Figure 6 examines how trade flows change after the start of the war, both in the war site and beyond. The upper panel presents average responses of exports and imports, measured in terms of pre-war GDP, for war sites, belligerents, and third countries. In the war site, both exports and imports ultimately contract by some 3 percentage points of GDP. For exports, the contraction starts early and progresses over time (left panel).

Figure 6: Trade dynamics following the start of war



Notes: Intensity of war set to 2 percent (casualties/population); top panel shows average outcome for war sites (solid purple line), belligerents (dashed yellow line), and third countries (dotted gray line) for size of war site equal to 3 percent (war-site population/world population). Bottom panel shows results for third countries with trade exposure (imports from war site(s)/GDP) set to 3 percent (solid red line) and zero (dashed blue line). Vertical axis measures deviation from pre-war level/trend in percentage points of pre-war GDP. Shaded areas indicate 90% confidence intervals, computed using standard errors that are robust with respect to heteroskedasticity as well as serial and cross-sectional correlation. Detailed information on underlying sample provided in Appendix Table A.1.

Imports show richer dynamics, increasing modestly after an upfront decline but then shrinking again notably from the fourth year after the onset of the war (left panel). On net, the war site's trade balance worsens over most of the period, indicating an increased absorption of resources from the rest of the world. Among belligerent countries, the picture is more nuanced: exports rise modestly, while imports are virtually unchanged. We also note that the initial rise in war sites' imports and in belligerents' exports is not a robust finding: It disappears

when we exclude the World Wars from the sample (in our robustness analysis below), perhaps suggesting that these patterns are driven by military trade in large continental-scale wars. For third countries, wars consistently decrease trade by about 1 percentage point of GDP over time.

The lower panel shows how the dynamics in third countries are differentiated by their trade exposure to the war site. We find that third countries with a high trade exposure experience a notable contraction in both imports and exports. By contrast, countries with no trade exposure remain unaffected. These patterns are consistent with the presence of a *trade channel* by which the economic effects of war spill over to countries with a high economic exposure to the war site: The decline in the war site's productive capacity reduces its ability to export to third countries. This way, the adverse supply shock hitting the war site also gets transmitted to its trading partners: Output falls and prices rise there too, see the working paper version of the paper for a structural account which fleshes out this channel (Federle et al., 2024).

In addition to being economically exposed to the war, countries that are adjacent to the war site also face the *risk* of military spillovers, which, in turn, may negatively affect economic activity. To assess this possibility and to precisely isolate the trade channel of war spillovers, our baseline specification (3.1) includes a variable that measures the intensity of war in adjacent countries. We find, in particular, that sharing a border with a war site significantly affects macroeconomic dynamics beyond what is already captured by the trade exposure variable (see Appendix O-C.13). Neighboring countries see long-term interest rates decrease slightly over time, while equity returns turn negative. This is consistent with the case study by Federle et al. (2025), which reports an increased disaster risk premium in nearby countries following the Russian invasion of Ukraine. Finally, we see some evidence of an increase in military spending and personnel that further corroborates the notion of military spillover risks transmitting to neighboring third countries. As we control for this spillover channel in our baseline specification, we conclude that our earlier trade-related results reflect a distinct trade channel for the transmission of war-related shocks. Supply-side disruptions in the war-site economy likely propagate internationally via a reduction in trade flows with existing trading partners.

3.3 Robustness and further results

In what follows, we extend our empirical work in four ways. First, we show that our main results also hold for various subsamples. Second, we report results for a range of alternative specifications. Third, we investigate specifically whether winning or losing an interstate war is associated with different macroeconomic dynamics. Fourth, we document the dynamics around wars for several additional outcome variables.

The results of our subsample analyses—which turn out to be very similar to those for the baseline—are provided in Appendix O-B. They present, in each instance, the counterparts to Figures 4 to 6. First, recall that the baseline defines war sites in terms of constant (modern-day) borders, consistent with how the outcome variables are defined and coded. However, wars may lead to border changes. To ensure that our results are not driven by such changes, we re-run the estimation after removing all country-year observations from our panel that are followed by at least one war-related border change within eight years—the time horizon we consider after the onset of war.

Second, we consider the role of the two World Wars for our results. We find that our results are robust to dropping these episodes. Third, since it has been argued that the Kellogg-Briand Pact of 1928 increased the costs of war (Hathaway and Shapiro, 2017), we re-estimate our models separately for the periods before and after the Pact but find similar responses, notably for war sites.[17] Fourth, we restrict the sample to the 18 countries included in the *Jordà-Schularick-Taylor Macrohistory Database*. Fifth, we drop war sites with fewer than 1,000 casualties. Sixth, we exclude interstate wars and report results only for other wars (instead, in the next section we focus on interstate wars). Lastly, we estimate our results on subsamples for different continents and find that, while there is heterogeneity in the size of the adjustment dynamics around war, the basic patterns are very similar across continents.

Next, we report results for a range of alternative specifications while delegating, as before, the counterparts of Figures 4 to 6 to Appendix O-C. First, we consider an alternative measure of war intensity. Rather than using casualties as in the baseline, we now measure intensity using the Geopolitical Risk (GPR) index compiled by Caldara and Iacoviello (2022). Specifically, we consider the maximum value of the country-level GPR in the five years following the start of

[17] We are grateful to an anonymous referee for this suggestion.

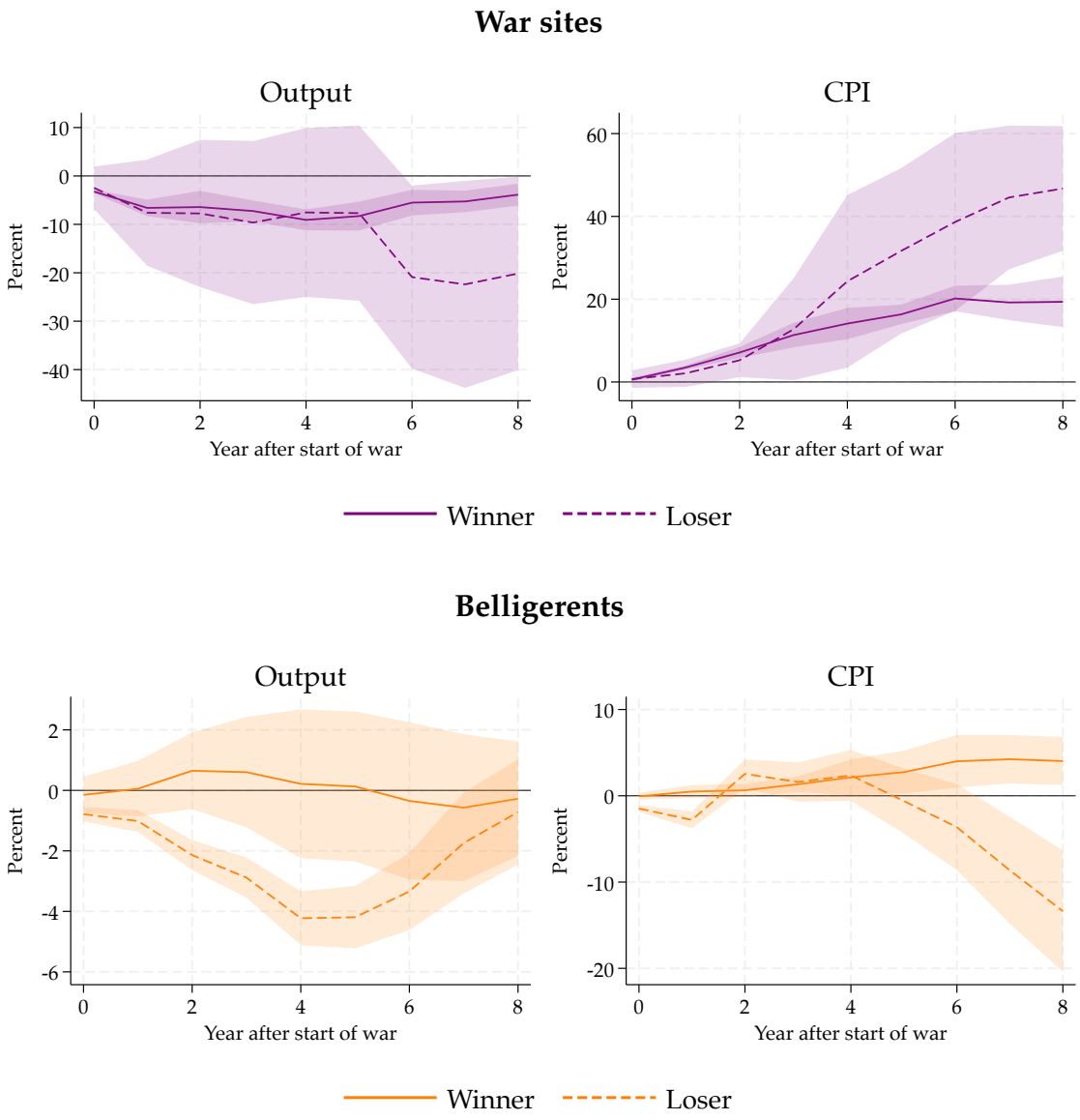
the war. The fact that the results are very similar to the baseline seems particularly noteworthy, as the GPR index is based on newspaper reporting and thus is fundamentally distinct in its construction from our baseline intensity measure based on casualties.

Second, we consider a longer time horizon following the onset of the war. We find that the changes in macroeconomic dynamics documented before tend to converge back to the trend after about 12 years. Third, we document that trade exposure to the war site matters not only for macroeconomic outcomes in third countries but also for the belligerents. Fourth, we redefine the start of the war as the year in which a broader relevant conflict begins—even if there is not yet military action in the war site (as required by our baseline specification). Reassuringly, we find that the observable changes in macroeconomic dynamics simply set in later, as one would expect if the actual onset of the war in the war site is crucial. Fifth, we control for the presence of nationalist governments. As we document in Section 4 below, nationalism ranks among the top reasons why countries go to war and may, at the same time, have a direct effect on the economy. We find, however, that the inclusion of this variable makes no difference to our results. Sixth, trade exposure may influence the opportunity costs of war. However, upon controlling for the countries' openness we find our results remain unchanged relative to the baseline.

Seventh, we winsorize our baseline measure of war intensity—casualties relative to the pre-war population—by capping it at 10% (roughly corresponding to the 95th percentile in our sample). In this way, we verify that our results are not driven by the positive skew of the $Site_{i,t}$-distribution. The same also applies to the remaining alternative specifications: Instead of assuming a constant linear country-specific trend, we employ a Hodrick-Prescott filter (with a smoothing parameter of 100) and a piecewise-linear trend, which allows for different dynamics not only across countries but also between the pre- and post-World War eras. Eighth, we allow for non-linear dynamics, by including squared terms of the regressors of interest. We detect only a moderate extent of non-linearity in the intensity of war and none when analyzing trade exposure. Lastly, we show that macroeconomic dynamics do not systematically differ between shorter wars lasting up to three years and longer wars.

We conclude this section with two additional results. First, we consider a dimension specific to interstate wars: the distinction between winning and losing countries. Thus, we allow the responses to interstate war in both war sites and

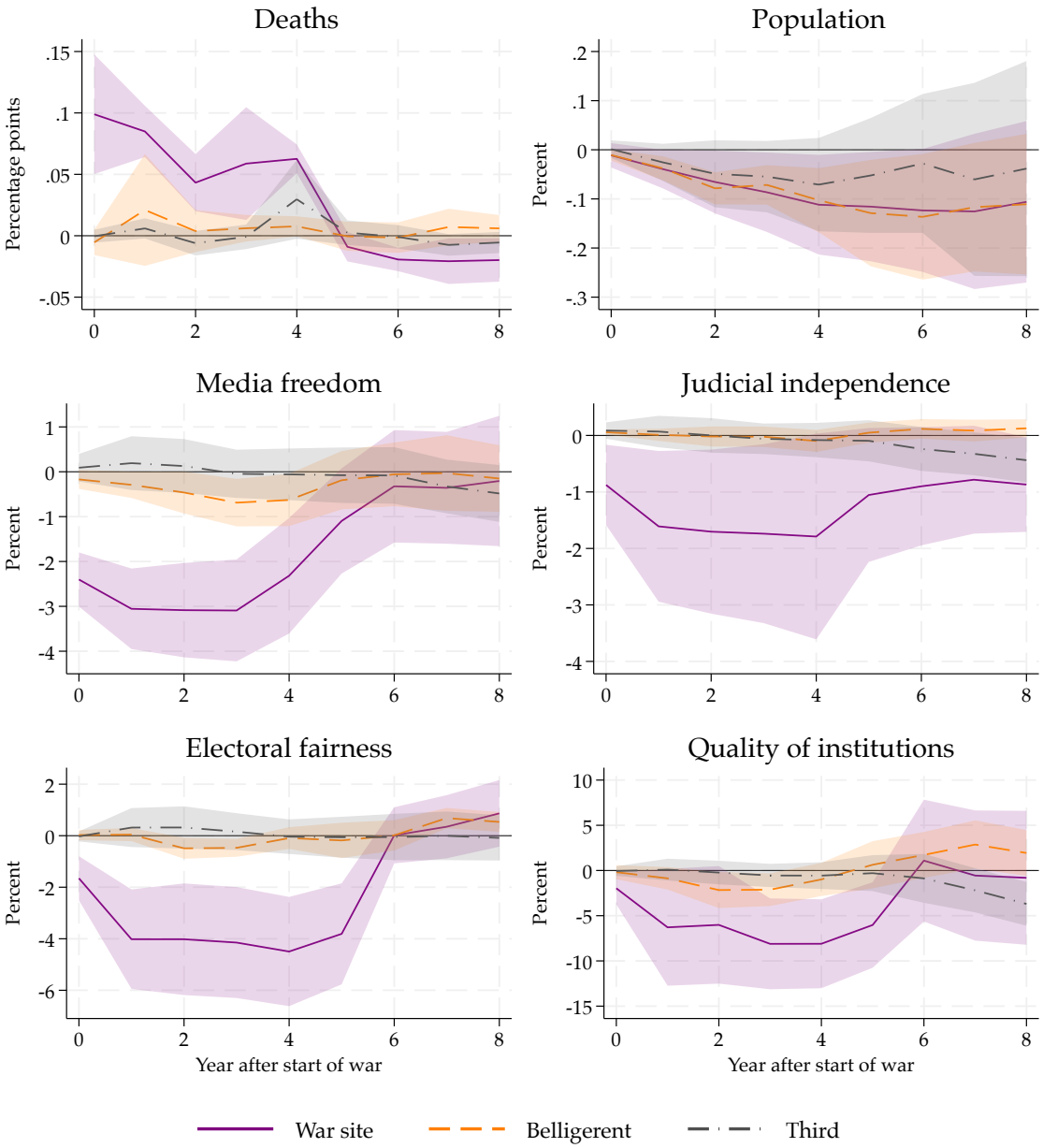
Figure 7: Winners and losers



Notes: Intensity of war set to 2 percent (casualties/population); size of war site set to 3 percent (war-site population/world population). Top panels show average outcome for winning (solid purple line) and losing (dashed purple line) war sites. Bottom panels show average outcome for winning (solid orange line) and losing (dashed orange line) belligerents. Shaded areas indicate 90% confidence intervals, computed using standard errors that are robust with respect to heteroskedasticity as well as serial and cross-sectional correlation. Detailed information on underlying sample provided in Appendix Table A.1.

belligerent countries to differ depending on whether a country emerges as a winner or loser—a classification that COW provides. Instead of distinguishing between war sites, belligerents, and third countries, we now differentiate between winning and losing war sites, winning and losing belligerents, and, as before, third countries. Figure 7 presents the results for output and prices in the war sites

Figure 8: How society changes during war



Notes: Intensity of war set to 2 percent (casualties/population); size of war site set to 3 percent (war-site population/world population). Solid (purple) line represents war site, dashed (yellow) belligerent, dotted (black) line third country. Vertical axis measures deviation from pre-war level/trend, measured in percent or percentage points of pre-war output (military spending) or of pre-war population (military personnel). Shaded areas indicate 90% confidence intervals, computed using standard errors that are robust to heteroskedasticity as well as serial and cross-sectional correlation. Detailed information on underlying sample provided in Appendix Table A.1.

(top panels) and in belligerent countries (bottom panels). Across both groups, losing countries experience a sharper decline in output and a stronger increase in prices. Still, winning countries also face declining output and rising prices,

provided they are a war site.

Finally, we broaden the perspective of our empirical exercise. So far, we have focused on how economic variables change after the start of a war, but change is unlikely confined to the economy. We corroborate this notion by applying our empirical specification to a set of outcome variables that capture other key aspects of society. The results are summarized in Figure 8, distinguishing, as before, between war sites, belligerents, and third countries. The first row shows results for demographic developments. Mortality, measured as deaths relative to the pre-war population, increases significantly in the war site, but not in other countries. In contrast, the population declines both in the war site and in belligerent countries, but not significantly in third countries. These patterns align well with prior expectations about the destructive nature of war.

In the remaining panels of the figure, we extend our analysis to a set of socio-demographic and political indicators for which sufficient data are available. Throughout, the patterns we observe are consistent with our analysis above. By far the largest changes take place in the war site: Media freedom and judicial independence decline, as do electoral fairness and, more broadly, the quality of institutions. None of these findings come as a significant surprise. Remarkably, however, in each instance, these changes are limited to the war site, with one noteworthy exception: Media freedom also declines in belligerent countries, though to a more limited extent.

4 Causality

A robust finding of our analysis in the previous section is that economic dynamics change systematically in the context of war. In particular, we observe significant shifts in output, inflation, and other key variables over a 2–5 year horizon after the start of the war, or what could be called business cycle frequency. The patterns in the data are consistent with the notion that wars are highly disruptive to economic activity, especially in the war site but also in third countries with significant exposure to the war-site economy. We have so far refrained from making any causal claims because short-run economic conditions may, in principle, influence the occurrence of war, rather than just be influenced by it. Such two-way causality indeed strikes us as likely in case of civil wars, more so than for interstate wars.

To assess whether our findings can be interpreted as war *causing* economic changes, we therefore now restrict our sample to *interstate wars* and pursue three lines of analysis. First, we verify—based on historical accounts—that interstate

wars are not typically caused by business-cycle conditions, as reflected in output growth and inflation. We then exclude instances where such a cause cannot be ruled out. Second, for the restricted sample, we show that neither GDP growth nor inflation improves the prediction of whether a country becomes a war site or not. Finally, we discuss potential biases and outline the assumptions under which our results for output and inflation permit a causal interpretation.

4.1 The *casus belli* of interstate wars: a narrative classification

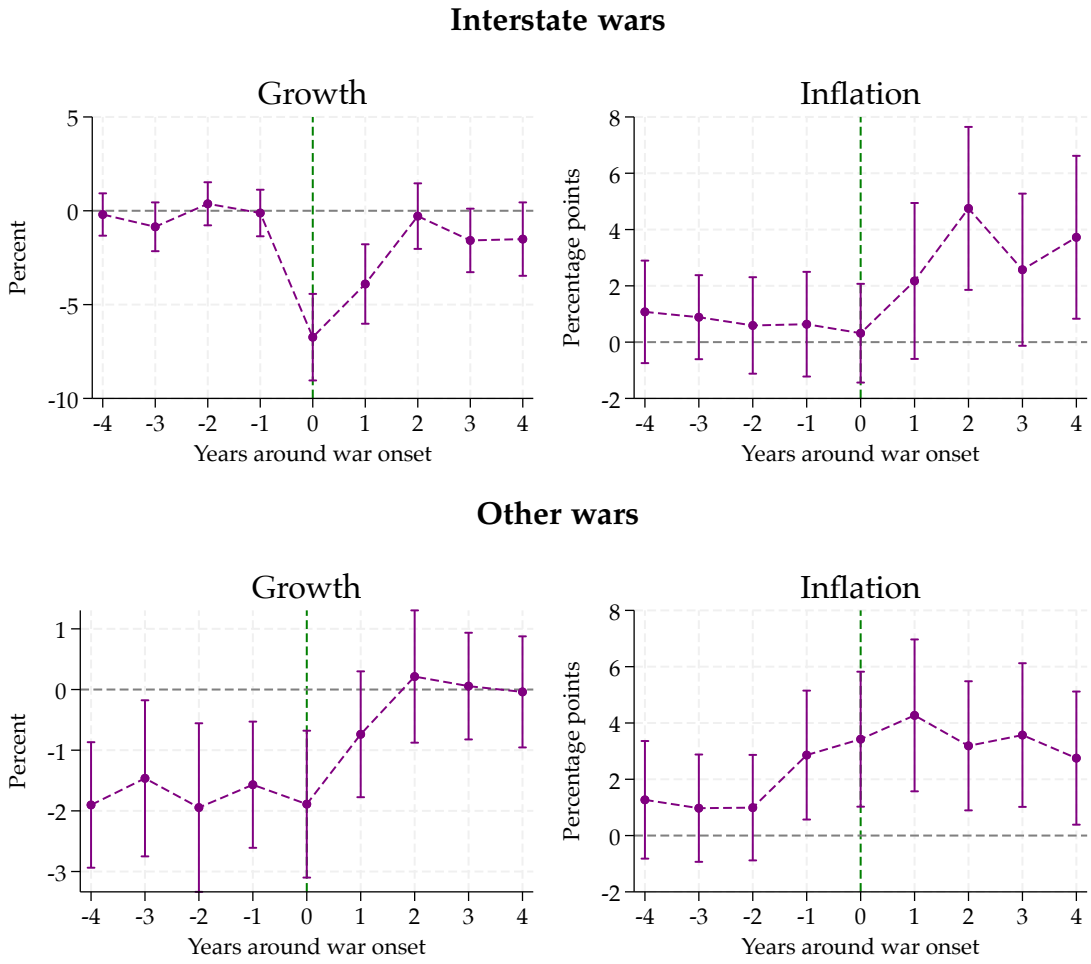
Our baseline does not distinguish between interstate wars and other types of conflict—namely, intrastate and extrastate wars. However, anecdotal evidence suggests that intrastate wars are sometimes intertwined with domestic economic crises. For example, a civil war may be triggered by a sharp deterioration in economic conditions, such as surging inflation, declining real wages, or sudden tax hikes.[18] More systematically, Miguel, Satyanath and Sergenti (2004) show that negative growth shocks raise the probability of civil conflict.

Figure 9 provides supporting evidence based on our sample, zooming in on average growth and inflation in war sites within an eight-year window centered around the start of the war. For interstate wars (top), average growth, shown net of country and time fixed effects in the left panel, declines sharply at the onset of war and remains depressed in the subsequent years, but looks steady before the war begins. In contrast, for other wars (bottom), a noticeable growth weakness is already evident prior to the outbreak of conflict. Similarly, average inflation, also measured net of country and time fixed effects and shown in the right panels, rises markedly in interstate wars only once the war starts, with no significant increase beforehand. For other wars, inflation starts to rise earlier. This divergence in pre-trends across types of war reinforces the concern that other wars are endogenous to the state of the economy in ways that interstate wars do not appear to be.

Hence, in what follows, we restrict our sample to interstate wars and investigate to what extent such wars are exogenous to the business cycle. On the one hand, there is some specific evidence whereby U.S. presidents have been more likely to deploy military force in times of "economic misery" (Ostrom and

[18] Sarkees and Wayman (2010) illustrate this with the Egypt-Palestine Anti-Conscription Revolt of 1934 (intrastate #521), which was driven not only by forced military service but also by new taxes imposed on Syrians under Egyptian administration; and the indigenous uprising during the 1898 Hut Tax War (extrastate war #413) in Sierra Leone, where colonial authorities levied a fee on each hut, ostensibly to fund British *protection*.

Figure 9: Economic performance in war sites around start of war



Note: Sample period 1870–2023. Average output growth and inflation in the war site around the start of war (in the war site), measured in percentage points (net of country and year fixed effects). Whiskers indicate 90% confidence intervals. Event windows for other variables are provided in Appendix O-D.

Job, 1986) and during recessions—provided they were up for reelection (Hess and Orphanides, 1995), notably in the post-WW2 period.[19] On the other hand, standard treatments in political science discuss the causes of war in terms of power struggle or power transition, not in terms of business cycle conditions (for instance, Organski and Kugler, 1980; Lebow, 2010). Robust evidence in support of a *diversionary theory of war* appears to be lacking (Leeds and Davis, 1997; Meernik and Waterman, 1996).[20] There is also a large literature on the

[19]For the purposes of our exercise, we may disregard this evidence because the U.S. never became a war site in our sample, a point also stressed by Goldin (1980).

[20]A recent exception is Jung (2024), who finds that slowing economic growth raises the likelihood of initiating military conflicts, although this pattern is confined to militarized interstate disputes short of outright war. As an example, he cites repeated saber-rattling by North Korea's leader. We revisit this evidence and find that the growth-slowdown effect disappears entirely

Table 2: Reasons for going to war

Nationalism	Creation of own sovereign state, wars for independence, imperialism	57
Power Transition or Security Dilemma	A rising power challenges a dominant one. Classic examples of the security dilemma in action are situations, where measures taken by one country to increase its security lead others to feel less secure and to take countermeasures, resulting in increased tensions that can lead to war.	37
Religion or Ideology	Deep-rooted disagreements over religious beliefs or ideologies (e.g., communism)	35
Economic, Long-Run	States might go to war to gain control over trade routes, markets, or valuable resources; economic rivalry and protectionism	35
Border Clashes	Unclear borders or intensifying border clashes	28
Revenge/Retribution	Wars can be initiated in response to perceived wrongs or to regain lost honor, even if there's no tangible gain to be had	18
Domestic Politics	Leaders may use foreign war to distract from domestic political issues or to rally their population around a common cause	13
Economic, Short-Run	Wars are fought because the economy is in a severe recession	6

Note: Some wars have multiple causes, which is why sum of war reasons in table exceeds total number of wars in our sample. Reasons were identified using various sources; see Appendix O-E.

fiscal transmission mechanism which typically assumes that military spending is exogenous to the business cycle (see, for instance, Barro, 1981; Barro and Redlick, 2011; Ramey, 2011; Miyamoto, Nguyen and Sheremirov, 2019).

Against this background, we attempt a systematic, if brief, narrative classification of the reasons for why countries go to war. Specifically, we classify the apparent *casus belli* for all interstate wars in our sample, based on the following procedure. First, for every conflict, we consult narrative excerpts from Gibler (2018) alongside the detailed entries in Martel (2012, 2018). Second, we complement these with various additional sources, most prominently Morillo, Black and Lococo (2009) and Parker (2020). All of these sources—and the specific excerpts underpinning each classification—are documented in Appendix O-E. Countries go to war for a variety of reasons and we do not restrict them to be mutually exclusive. As we try to determine the reasons for going to war, our reading of the historical records results in an average of three main reasons per war. If our sources suggest different reasons, all of them are included in our classification. In this way, we seek to be conservative in assessing whether a war may (also)

once we limit the analysis to actual interstate wars.

have been motivated by short-term economic considerations. Table 2 lists the results of our classification based on eight distinct categories. In the right-most column, we report the number of wars which fall into each category.

Nationalism and power transitions rank among the primary reasons for going to war. While we also find that countries have pursued economic objectives in certain wars, these objectives typically relate to long-term goals, such as securing control over trade routes or gaining natural resources. Such long-term goals should be largely orthogonal to the business cycle, as has been similarly argued in the influential study on the effects of tax shocks by Romer and Romer (2010). In our sample, we identify only six wars in which short-term economic factors appear to have played a critical role. The 1982 Falklands War is a classical example: after failing to enact necessary economic reforms, Argentina's junta seized the *ice-cold bunch of rocks* (Reagan) of the Falklands—strategically trivial but politically potent—garnering a brief surge in popular support (Levy and Vakili, 2014). Other examples include the Boxer Rebellion of 1900 and the Italian-Turkish War of 1911. In the first case, religion and nationalism were key aspects, but so were adverse economic conditions. In the second case, nationalism, or more specifically colonialism, was key. However, dire economic conditions in Italy, as reflected in mass emigration in the decade before the war, were arguably also conducive. Similar considerations apply to the Second Sino-Japanese War of 1931-1933, the Conquest of Ethiopia of 1935-1936, and the Football War of 1969. Accordingly, we exclude these six wars from our analysis. The restricted final sample of interstate wars features macroeconomic time-series observations for 83 war sites, compared to 84 in the baseline, see Table 1 above.[21]

4.2 Predictive regressions and Granger causality

We now complement our narrative evidence with a formal assessment of whether business cycle conditions, as summarized by output growth and inflation, help to predict the onset of war. In the subsample distilled from our narrative analysis, this should not be the case. At the same time, we would expect other outcome variables considered above to have predictive power for the likelihood of becoming a war site, notably military spending and personnel, but also indicators of societal quality, given arguments developed by Garfinkel (1994).

[21] Recall that the composition of the sample is also influenced by the lack of macroeconomic time series for some of the wars in our data set. Even when this is the case and we lack data for certain war sites, available data for third countries exposed to the war would, nonetheless, remain part of our econometric analysis.

In what follows, we thus examine whether the variables of primary business cycle interest—GDP growth and inflation—help predict a country's probability of becoming a war site. For this purpose, we binarize the variable $Site_{j,t}$ and estimate a logistic regression that includes macroeconomic indicators, political-institutional controls, and other relevant variables as covariates. Specifically, following classic studies in both economics (Martin, Mayer and Thoenig, 2008) and political science (Bremer, 1992), we include lagged values (up to four years) of GDP growth, inflation, military spending relative to GDP, and trade openness (imports plus exports as a share of GDP) alongside additional geopolitical variables: the number of land borders, major power status, and the V-Dem liberal democracy index. We also include the number of years of peace in polynomial form to capture potential nonlinear duration effects (Carter and Signorino, 2010). However, we pursue a monadic approach rather than the dyadic approach more common in the literature, since we are interested in predicting that country j turns into a war site (and not that war breaks out between country j and country i).[22] We estimate a generalized linear model with a logistic link function and cluster standard errors by country to correct for within-country correlation of errors.

We delegate details to Appendix O-F and present results compactly in Table 3, summarizing whether GDP growth and inflation have statistically significant predictive power for whether a country becomes a war site. Across specifications, we consistently find that this is not the case for interstate wars. In other words, there is no evidence suggesting that weaker (or stronger) economic performance significantly increases the risk of becoming a war site. By contrast, a "control" exercise for *other wars* reveals a few cases of statistical significance. Our findings also align with the previous literature on the determinants of militarized interstate conflicts. Multilateral openness affects the risk of war (Martin, Mayer and Thoenig, 2008; Lee and Pyun, 2016). Geopolitical factors also turn out to be significant predictors, in line with typical findings from conflict research: (1) a dynamic relationship exists between military expenditure and war-site status, reflecting short-term escalation and longer-term deterrence; (2) states with more neighboring countries exhibit a slightly lower risk of becoming a war site (though not in a statistically significant way); (3) liberal democratic governance reduces the risk of war; (4) major power status also reduces the risk; and (5) the risk of war onset exhibits a significant non-linear relationship with peace spell duration

[22] As a result and because we only consider interstate wars, our data set in this section is much narrower in scope. It is also limited by data availability of covariates.

Table 3: Significance of predictors Across selected logit models

	Probability of being war site							
	Interstate wars				Other wars			
	(21)	(22)	(29)	(30)	(21)	(22)	(29)	(30)
GDP growth (any lag)	❍	❍	❍	❍	❍	●	❍	❍
Inflation (any lag)	❍	❍	❍	❍	❍	●	●	●
Openness (any lag)			●	●			❍	❍
Military exp. (any lag)			●	●			❍	●
Borders (t-1)			❍	❍			❍	❍
Democracy (t-1)			❍	●			●	●
Major power (t-1)			❍	❍			●	❍
Peace years (any term)	●	●	●	❍	●	●	●	❍
Country fixed effects		✓		✓		✓		✓
Observations	7030	4386	4811	2309	6883	3825	4656	1937
War sites	77	77	45	45	120	120	63	63
Pseudo-R^2	0.03	-0.03	0.06	0.14	0.15	0.12	0.12	0.01
Log-Likelihood	-402.4	-355.7	-217.7	-147.6	-503.0	-428.3	-271.9	-232.8

Note: Logit model estimates for binary war-site indicator variable with standard errors clustered at the country level. ❍ and ● refer to variables included in regressions; ● denotes that the most significant lag has $p < 0.05$. The column numbers refer to the columns of the Tables in Appendix O-F, which report the full regression results and further specifications.

(see, e.g. Bremer, 1992; Buhaug, 2005).

As a final step, we test formally whether past economic growth and/or inflation *Granger-causes* a country becoming a war site. Given that our dependent variable is binary, we deviate from a standard VAR framework and instead employ logistic regression models in a dynamic panel framework. Following the idea of Dumitrescu and Hurlin (2012), we compare a *restricted model*—containing only lags of the dependent variable (onset of war) and a polynomial of years of peace—against *unrestricted models* that additionally include lagged GDP growth, lagged inflation, or both. A likelihood-ratio (LR) test then allows us to asses whether past economic performance *Granger-causes* the onset of war in the war site. The *LR* statistics and corresponding *p*-values (with four degrees of freedom for individual economic variables and eight for the joint inclusion) indicate no significant predictive improvement from incorporating up to four lags of GDP growth ($LR = 2.75$, $p = 0.601$), inflation ($LR = 0.36$, $p = 0.986$), or both simultaneously ($LR = 2.99$, $p = 0.935$). Thus, we cannot reject the null hypothesis that the coefficients on the lags of GDP and/or inflation are jointly zero. In other words, there is no evidence of Granger causality in the interstate sample. The opposite is true when we apply the analysis to other wars. In this case, the

likelihood-ratio tests reveal that past GDP growth does Granger-cause the onset of war ($LR = 11.98$, $p = 0.018$). The joint inclusion of both lagged GDP growth and inflation also yields significant predictive power ($LR = 17.14$, $p = 0.029$), whereas inflation alone remains non-significant ($LR = 6.04$, $p = 0.196$). This suggests that past economic performance helps to predict wars that are not interstate conflicts.

4.3 Results for restricted sample and potential biases

Next, we re-estimate our baseline specification from Section 3.1 on the restricted sample, which includes only interstate wars and excludes cases in which our narrative analysis identified short-run economic developments as a potential cause of the war. For this restricted sample, our predictive regressions and Granger causality tests in the previous subsection have suggested that prior economic conditions—captured by GDP growth and inflation—do not affect the likelihood of becoming a war site. Re-running our baseline regressions on this sample produces estimates that are remarkably similar to the results shown in Figures 4 to 6 above. In fact, the similarity is so close that, to economize on space, we present these restricted sample results in Appendix O-C.15.

Given these arguments and results, a causal interpretation—whereby war *causes* the business cycle dynamics we have documented—appears justified. However, this interpretation is still subject to a number of caveats. First, whether a country becomes a war site might depend not on its past growth, but on expectations about its future growth potential, insofar as this also influences its military capabilities. For instance, deteriorating economic prospects could conceivably lead a country to initiate conflict, especially if it fears a decline in its relative military strength over time.[23]

One potential mitigant for this concern is that the business cycle is only weakly correlated with a country's military strength. To show this, we regress military strength—measured by the log of the Composite Index of National

[23]One plausible example is 1914 Germany, where political-military elites were deeply concerned about losing "the financial arms race" with Britain, France, and Russia due to the fiscal constraints imposed on the Second Reich finances by the federal constitution (see Ferguson, 1998, Chapter 5). Similarly, in 1939, inflationary pressures in Germany were mounting and material shortages worsening such that "Germany had reached a point at which it had very little to gain from a continuation of the peacetime arms race" (Tooze, 2006, Chapter 9). Likewise, it has been argued that Japan's decision to attack Pearl Harbor in 1941 was partly driven by U.S. sanctions, which restricted Japan's oil supply and led to the belief that it needed to act quickly to secure oil and other resources in Southeast Asia (Miller, 2023). We are grateful to an anonymous referee for highlighting this issue and providing these instructive examples.

Capability—on the log of real GDP and on GDP growth, respectively, while controlling for both country and year fixed effects. We find that military strength is significantly correlated with output, but not with output growth.[24]

Still, this insight might be lost on aggressor countries. Hence, our estimates for the war-site economy (typically the country being attacked) may *understate* the full (negative) economic impact of war if a country is targeted precisely because of its strong perceived growth prospects.[25] Mutatis mutandis, our estimates for belligerents (typically the aggressors) might *overstate* any negative output effects if their decision to initiate a war were partly motivated by deteriorating growth prospects. In both cases, while we can confidently rule out a selection bias based on past economic performance for our restricted sample, the unobservable nature of future growth expectations implies that our estimates should be interpreted as bounds on the short- to medium-run absolute economic impact of war.

Endogeneity bias could also affect our results—and thus limit causal interpretations of the descriptive evidence presented in Section 2.2—if some of the apparent reasons for going to war (see Table 2) also had a direct positive or negative effect on near-term economic performance. Nationalism is a plausible contender in this regard, which is why we included explicit controls for nationalist government in the robustness checks in Section 3.3. Encouragingly, this modification does not affect our results in any meaningful way.

5 Conclusion

Using data from 60 countries since 1870, we analyze the macroeconomic dynamics occurring after the outbreak of wars. We observe adverse outcomes—lower output and higher inflation—not only in countries directly exposed to war on their own soil but also among other belligerents and even in third countries, especially those with strong trade ties to the war site. Is war the cause of these changes? Our baseline analysis stops short of causal claims, acknowledging that short-run economic conditions may influence the onset of war rather than just be its consequence. Such two-way causality seems particularly plausible in the case of civil wars. However, for a subset of interstate wars, we find evidence that war is indeed the likely driver. What's more, the economic dynamics in this subset

[24]We report these results in Table O-A.4. In our robustness analysis above, we also explicitly control for military strength and find that the results remain essentially unchanged relative to our baseline specification.

[25]Indeed, we show that the initiator of a conflict is much less likely to become a war site; see Table O-A.5 in the Appendix.

resemble those in the broader data set, which also includes intra- and extrastate wars.

From an economic perspective, war acts as an adverse supply shock for the war-site economy: its destructive nature hampers productive capacity, disrupts output, and drives up prices. These effects spill over through trade. For countries neighboring the war site, there is an added risk of military spillovers, reflected in weak equity returns.

We close with a caveat: While we look beyond macroeconomic aggregates to also capture certain societal aspects of war, our analysis cannot possibly capture the full implications of war for human welfare. Still, our results already make clear that *The Price of War*—in economic, political, and socio-demographic terms—is paid by many countries, well beyond the actual war site.

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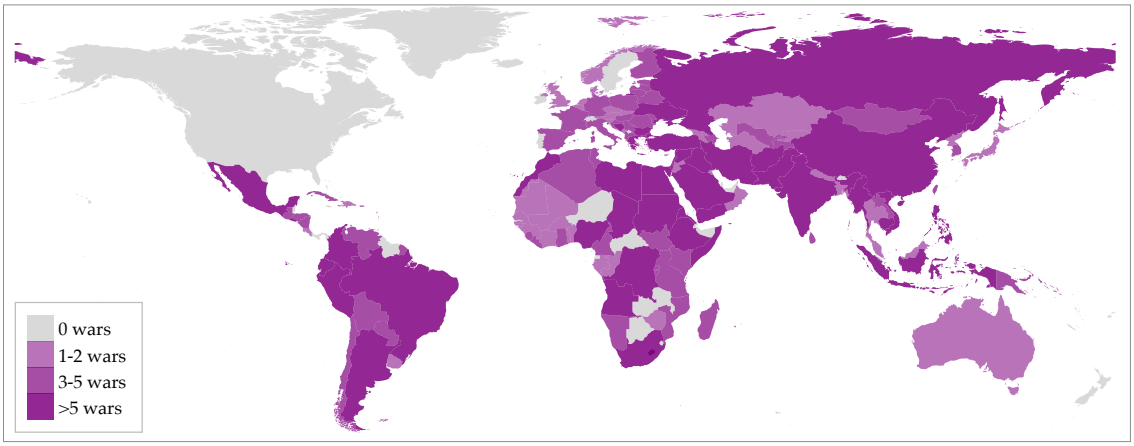
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A Data sources

Figure A.1: Geographical distribution of war sites



Note: Colors indicate number of wars that took place on a country's soil for the period 1870–2023.

We obtain time-series data for output, inflation, and equity returns from the *Macrohistory Database*, which covers 18 advanced countries starting in 1870 (Jordà, Schularick and Taylor, 2017). This database, in turn, is constructed from a number of sources, including Bolt and Van Zanden (2014). These sources typically make adjustments for changing borders so that the data refer to current borders; see, for instance, Maddison (1995). We complement the *Macrohistory Database* with time series for additional countries from various sources (World Bank, 2022; Funke, Schularick and Trebesch, 2023) which, in turn, build on Ursùa and Barro (2010) and Inklaar et al. (2018). These sources provide us with data for GDP in per capita terms. For our analysis, we compute an aggregate output measure to account for changes in the population during wars. In so doing, we rely on population data for the territories that define countries today (Bolt and Van Zanden, 2014; HYDE et al., 2025).[26] The same sources provide us with a measure of consumer price inflation. We also obtain data on total factor productivity, labor, and capital stocks from the *Long-Term Productivity Database* (Bergeaud, Cette and Lecat, 2016). The COW project provides data on military expenditure, employment in the military, a composite index of military strength, and, for interstate wars, coding of winners and losers (Singer, Bremer and Stuckey, 1972; Singer, 1988; Sarkees and Wayman, 2010).[27] We exclude

[26]Although Bolt and Van Zanden (2014) mostly refer to 1998 boundaries, these have only changed to a small extent since (Schvitz et al., 2022). In cases where these sources still lack population data for the pre-war years of interstate wars, we complemented data manually with entries from Fouquin and Hugot (2016), Gapminder (2024), and Fink-Jensen (2015).

[27]COW documents all data for historically existing states. In the few cases where states have

Table A.1: Sample size across variables

	Macro time series for…				
Variable	Total	Sites	Belligerents		Third
Figures 4 and 5: Macroeconomic outcomes					
GDP	7,655	206	321		6,998
CPI	7,796	220	319		7,126
Capital stock	3,059	40	192		2,830
TFP	3,059	40	192		2,830
Long-term interest rate	4,135	57	228		3,670
Equity return index	2,258	23	177		2,075
Military spending	6,258	181	318		5,902
Military personnel	6,343	206	314		5,970
Figure 6: Trade outcomes					
Imports	7,002	188	268		6,362
Exports	7,030	190	269		6,388
Deaths	3,366	24	138		3,046
Population	9,240	265	329		8,462
Media freedom	7,829	238	328		7,304
Judicial independence	7,843	238	328		7,318
Electoral fairness	7,798	237	328		7,279
Institutions	6,958	209	322		6,495
Figure 7: Winners and losers					
		Winners	Losers	Winners	Losers
GDP	7,655	27	27	117	30	3,573
CPI	7,796	28	28	116	30	3,648

Note: Table counts non-missing observations for each variable in entire sample ("Total") and in years of war shocks.

instances where countries have switched sides over the course of the war (from winners to losers or vice versa). We source bilateral trade data from Fouquin and Hugot (2016). For data on media freedom, judicial independence, electoral fairness, and political institutions we draw on Funke, Schularick and Trebesch (2023), Coppedge et al. (2022) and Coppedge et al. (2025). Since our empirical specification requires trade data to be balanced over time, we impute missing values using a standard gravity model featuring importer-exporter and time fixed effects. The model has a pseudo-R^2 of 95% and is explained in more detail in Appendix B. Table O-A.3 in the Online Appendix offers a further overview of the times-series data, our sources, and basic transformations of the data.

dissolved into multiple other states, we manually convert country codes to the largest successor. Country codes are standardized from UCDP (Gleditsch and Ward) and the IMF to ISO3 using the *countrycode* (Arel-Bundock, Enevoldsen and Yetman, 2018) and *peacesciencer* (Miller, 2022) R packages.

Table B.1: Gravity estimation

	(1)	(2)	(3)	(4)	(5)
Constant	19.15***	19.54***	21.80***	22.27***	22.93***
	(0.00931)	(0.00929)	(0.00855)	(0.00782)	(0.00330)
Year FE		✓			✓
Importer FE			✓		
Exporter FE			✓		
Importer × Exporter FE				✓	✓
Pseudo R^2	0.00	0.10	0.67	0.78	0.95
N	2,676,600	2,676,189	2,676,259	2,401,260	2,400,898

Note: Table shows results of estimating eq. (B.1). Standard errors are denoted in parentheses.

B Imputation of trade data

To construct a balanced panel of bilateral trade flows from 1870 to the present, we combine historical data from CEPII's TradHist (Fouquin and Hugot, 2016) dataset with IMF's Direction of Trade Statistics data. The CEPII data are converted from British pounds to U.S. dollars using historical exchange rates contained in TradHist, while IMF data from 2015 onward are already in U.S. dollars. All values are deflated to constant 2015 U.S. dollars using a long-run U.S. consumer price index series.

Despite this combined coverage, many trade flows remain missing. We estimate a gravity model to impute these missing trade values on a balanced panel including all possible country pairs and years:

$$\text{Imports}_{i \to j,t} = \exp(\alpha_{ij} + \delta_t) + \varepsilon_{ijt} \tag{B.1}$$

Here, $\text{Imports}_{i \to j,t}$ denotes the value of imports received by country j from country i in year t (constant 2015 U.S. dollars). The term α_{ij} represents a country-pair fixed effect capturing persistent trade relationships, while δ_t captures global trade trends. We estimate the model using *Poisson Pseudo-Maximum Likelihood*, which is robust to zero or missing trade flows. Table B.1 shows estimation results for progressively more demanding specifications. Column (5) features both year and importer-exporter fixed effects and has a pseudo-R^2 of 95%. We use these predictions to fill missing observations in our trade exposure calculations.