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Who wins wars?

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ABSTRACT

WHO WINS WARS?

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Economic resources are often seen as decisive for the outcomes of military conflicts. This paper asks whether "deeper pockets" help win wars. We construct a fine-grained dataset covering more than 700 interstate disputes and rely on exogenous resource price shocks to estimate the causal effect of windfall gains on winning chances in interstate conflicts. We find a statistically significant and quantitatively large impact of windfall gains on winning odds and show that a key channel of transmission is a surge in military spending, after an exogenous increase in government revenues.

Keywords: Interstate disputes, International wars, Commodity prices, Conflict outcomes, Military expenditures

JEL classification: D74; F51; H56; N40; Q02

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Nervos belli, pecuniam infinitam.
The sinews of war [are] unlimited money.
— Cicero, 43 B.C.

1 Introduction

The importance of financial and economic resources for warfare has occupied political leaders and military strategists throughout centuries. In ancient Greece, the Athenians were forced to melt down the golden statue of Athena to finance the war against Sparta. An important factor in Rome's rise to superpower status was its *economic might*, largely based on slavery and exploitation of "grain chamber" provinces like Egypt (Duncan-Jones, 1982; Mitchell, 2014; Garnsey and Saller, 2015). In the modern era, Napoleon's expedition to Egypt failed when he ran out of funds to pay his soldiers after the ships carrying the Maltese treasure sank. Half a century later, in the American Civil War, the economic superiority of the North was decisive for the victory over the more agrarian South in what is widely seen as the first war of the industrial age. The two World Wars of the 20th century are routinely viewed as large-scale economic mobilization efforts, with the eventual victories of the Allied powers attributed, in part, to their greater economic resources.[1] Recently, Gorodnichenko, Korhonen and Ribakova (2024) and Garicano, Rohner and di Mauro (2022) argued that Russia's war against Ukraine is enabled by ongoing revenues from commodity exports.

Synthesizing the course of modern history in his book *The Rise and Fall of Great Powers*, Kennedy (2010) famously argued that shifts in global power typically mirror changes in economic resources, emphasizing that economic capacity is a central determinant of military power and success in war. Looking beyond great power rivalries, there is anecdotal evidence of interstate disputes over recent decades where economic shocks are suspected to have been of paramount importance in determining victory versus stalemate or defeat. Such telling case studies include the Chadian-Libyan wars, where oil exports mattered heavily, the war between Sierra Leone and Nigeria, as well as the armed conflict opposing Uganda and Tanzania, in which cases oil and coffee price shocks substantially drove war outcomes. Below in Section 2, we will discuss these anecdotal case studies in more depth.

While these examples and associated debates are useful, systematic quantita-

[1] During World War I, the leading economies directed between 30% and 60% of their national incomes toward the war effort, and up to 70% in World War II (Broadberry and Harrison, 2020).

tive evidence that focuses on the causal impact of economic shocks on military prospects of warring parties is missing. This is the gap in the literature that our paper aims to close.

We study more than 700 militarized interstate disputes from 1977 (our starting year, driven by data availability) to 2013 (the final year, also due to data availability). We also construct a time-varying measure of relative commodity revenues at the dyadic level, exploiting that (exogenous) world market price changes affect countries differently depending on their trade basket. This allows us to exploit shocks to financial resources for conflict parties while filtering out time-invariant country pair characteristics (through dyad fixed effects) and global shocks (through time effects).

We find that a 10 percentage point windfall gain for one conflict party relative to its GDP increases its probability of winning instead of ending up in a draw by 3.2 percentage points. Addressing the concern that wars in commodity-exporting countries can affect world market prices, we show that our results hold when removing product-country combinations with potential market power. The same is true for a range of plausible definitions of conflict and different estimation methods. We also perform various sensitivity tests, remove potential outliers, and carry out placebo exercises. We further study the mechanisms through which price changes affect conflict outcomes and show that windfall gains translate into greater government revenues and greater military spending. We wrap up the analysis by investigating heterogeneous effects.

The effects that windfalls have on conflict outcomes appear particularly large once we consider that only a fraction of windfalls from commodity price changes feed into fiscal and military resources – simply, because these revenue streams are usually dispersed among various actors and the government is only one of them. In times of conflict, only about 50% of windfalls end up in government revenues and only 10% in military expenditures. Thus, an exogenous increase in government revenues by 10 percentage points of GDP leads to an increase in the probability of winning instead of ending up in a draw by some 6.4 percentage points. Assuming that money only affects the chances of victory via military expenditures, these numbers imply that a 10 percentage point increase in military expenditures relative to GDP elevates the winning chances by some 32 percentage points.

Several strands of the literature are relevant to our contribution. First of all, the current paper is part of the literature on the economics of conflict (for recent surveys, see Anderton and Brauer (2021); Rohner and Thoenig (2021);

Rohner (2024*a*)), and in particular part of the work in economics and political science on militarized interstate disputes. The recent (empirical) literature on international conflict has covered among others the role of geography and population characteristics (Toset, Gleditsch and Hegre, 2000; Caselli, Morelli and Rohner, 2015; Spolaore and Wacziarg, 2016; Federle et al., 2024), trade and interdependence (Barbieri, 1996; Martin, Mayer and Thoenig, 2008; Copeland, 2014), regime characteristics (Maoz and Russett, 1993; Conconi, Sahuguet and Zanardi, 2014; Krainin and Ramsay, 2022), alliances and structural features of the international system (Leeds, 2003; Jackson and Nei, 2015) or uncertainty and media communication (Booth and Wheeler, 2008; DellaVigna et al., 2014) for the onset of hostilities. Our focus is very different, both in terms of the main explanatory factors (economic shocks) and in terms of the dependent variables (fighting outcomes rather than onsets).

When it comes to the existing literature on the determinants of victory versus defeat, several qualitative contributions – often related to the *realist* school of thought – have argued that economic factors matter for the projection of power in interstate conflicts (Wohlforth, 1993; Mearsheimer, 2001; Nye, 2011). There is also an established literature in International Relations that has focused on measuring the power of nations, e.g., related to CINC (Composite Indicator of National Capability) scores (Singer, 1988) or other power scores, featuring typically economic output as one component of power. This has given rise to a series of studies that examine whether national capabilities and military spending are associated with military success (Wayman, Singer and Goertz, 1983; Beckley, 2018).

While this seminal work provides very valuable insights on a crucial question, it faces a key shortcoming: Many of the measures employed endogenously depend on either choices or potential confounders. If military spending or CINC scores (which contain military personnel as a component) are outright choice variables, even using, say, the Gross Domestic Product (GDP) would be endogenously related to potential confounders. For example, incompetent governance would both affect GDP and military winning chances, and aggressive autocratic regimes may have tendencies to invest all means in military might, which may affect GDP and boost military clout. In both cases, any positive or negative correlations between national capacities and dispute outcomes could hence be spuriously driven by confounders or reflect reverse causation. Hence, while the correlations between national capacities and conflict outcomes are extremely useful, they may in many cases not be interpreted causally due to these intricate statistical challenges.

As mentioned above, our current contribution proposes a novel approach. We start from the premise that a part of the windfall gains may be used for investing in military capacities, which in turn may increase winning chances. In Appendix A we show that in a slightly extended workhorse contest model, windfall gains in a given country unambiguously increase its fighting strength and winning chances. In particular, we find that beyond the direct "higher military spending" effect, there are also indirect motivational impacts of higher appropriable windfalls for both players, but that in equilibrium, these indirect effects are dominated by the direct effect.

Empirically, to draw on exogenous variation in national military power, we exploit exogenous shocks in world market prices that lead to increases or reductions in rents that are taxable for military goals. The underlying logic is that favorable fluctuations of commodity returns affect government revenues (Caselli and Michaels, 2013), feeding into the ability to sustain military expenditures, which in turn may fuel the chances of victory. In contrast a deterioration of commodity returns may forestall defeat. If commodity price shocks have been exploited in recent work on civil war incidence (Dube and Vargas, 2013; Bazzi and Blattman, 2014; Berman et al., 2017; McGuirk and Burke, 2020; Gehring, Langlotz and Kienberger, 2023), they have to the best of our knowledge never been utilised for studying success prospects in international conflicts.

Also the literature on state capacity is relevant.[2] In particular, Gennaioli and Voth (2015) argue that military innovation in Medieval times has led to a higher importance of fiscal revenues for battlefield success, which has fostered the building of state capacity. Their contribution features stylized facts such as a positive correlation between fiscal revenues and combat success for the late Medieval period. Given that fiscal revenues is an endogenous choice variable in their theoretical framework, their analysis focuses on correlational evidence. We build on this seminal pioneering work and study the complementary question of how exogenous shifts in resource rents have a causal impact on winning prospects for a large number of modern time conflicts.[3]

In summary, our contribution is, to the best of our knowledge, the first that is able to exploit exogenous changes in resource revenues to estimate the causal

[2]Recent contributions linking (the absence of) state capacity to the risk of conflict onsets include Besley and Persson (2011) and Rohner (2024*b*).

[3]There exists also a theory literature, building dynamic two-country models that link military spending and geopolitical risks to resource extraction, trade and government bond markets (Acemoglu et al., 2012; Garfinkel, Syropoulos and Zylkin, 2022; Pflueger and Yared, 2024).

effect on the chances of countries to emerge successful from militarized interstate disputes.

The remainder of the paper is organized as follows: Section 2 illustrates the motivation and scope of the question at hand with anecdotal case study evidence for a series of examples. Next, in Sections 3 and 4 we introduce the data used and causal identification strategy, respectively, while Section 5 is devoted to the main regression analysis. Next, Section 6 investigates major channels of transmission, while Section 7 concludes. A series of supplementary information and results are relegated to the Appendix.

2 A case study: the Chad-Libya wars

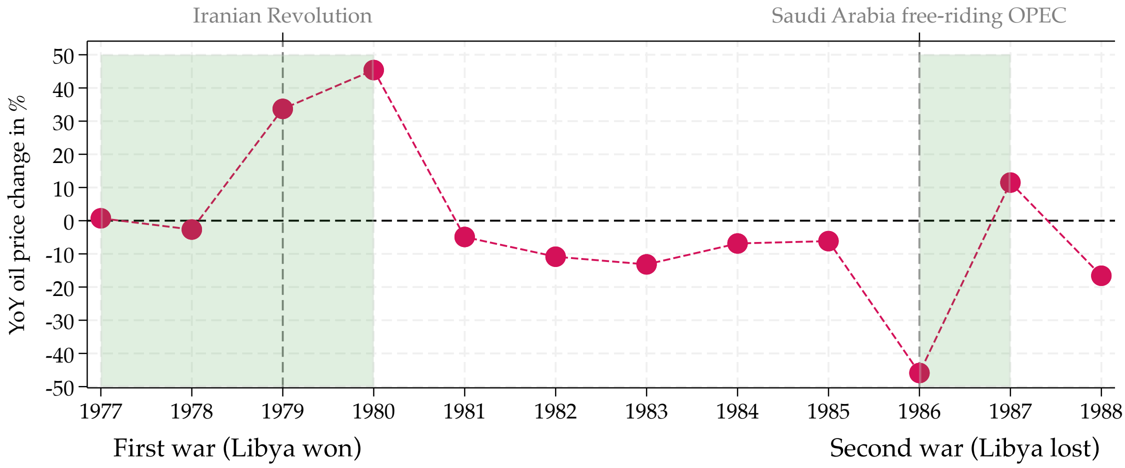
To illustrate the type of mechanism we have in mind, we start by discussing the evolution of battlefield fortunes in the Chadian-Libyan wars in the light of world oil price fluctuations. Militarized tensions between Libya and Chad erupted in the 1970s, around a sovereignty dispute about the Aouzou Strip, located in the extreme north of Chad, bordering Libya.[4] While consisting nearly entirely of the Sahara desert, this narrow land strip also contains rich uranium deposits.

Libya occupied the region in 1973 and annexed it in 1975. For the next dozen years, there were a series of armed conflicts between Chad and Libya over this land strip, where Chad benefited from the support of France and other Western powers, while Libya relied on Soviet arms deliveries. The last phase of the conflict entered history as the so-called "Toyota war", due to the key role played by Chadian Toyota pickup trucks. The two countries decided in 1988 to settle the discord peacefully, and in 1994 the International Court of Justice dismissed Libya's territorial claims, which was followed by the withdrawal of the Libyan troops.

Figure 1 illustrates how the battlefield fortunes of oil export-dependent Libya were associated with world oil price shocks. In the early phase of the conflict (until 1980), oil prices surged and Libya's regional military might was considerable. Especially the year following the 1979 Iranian revolution was characterized by a global oil price shock. As pointed out by Allan (1983), "by 1980, oil revenues were over US $20 bn [...] permitting large allocations to consumption, defence, and international ventures" (1983: 377). In this period

[4]Our discussion of the Chadian-Libyan wars draws among others on Allan (1983); Gross (2019), on the Encyclopedia Britannica entry on "Aozou Strip" and on the "Better Evidence Project" of the George Mason University (https://bep.carterschool.gmu.edu/resolving-the-militarised-territorial-dispute-between-chad-and-libya/).

Figure 1: Oil price changes during Chain-Libyan wars



Note: Figure shows year-on-year oil price changes between 1977 and 1988. Both green shaded areas mark separate wars between Chad and Libya. Grey dashed lines denote Iranian Revolution (1979) and Saudi-Arabia free-riding OPEC (1986) — exogenous events affecting oil prices. Libya, a then-major oil exporting country, won the first but lost the second war.

fall the significant military engagements of Libyan troops and their support for various local rebel groups aiming to overthrow the Chadian government.

In contrast, the tables had turned in the later phases of the conflict. The 1986-1987 period was characterized by Saudi Arabia driving world oil prices down to Cournot level (Griffin and Neilson, 1994), and by the "inability of oil-exporting Libya to pay in cash for its Soviet arms purchases as a result of the dramatic drop in global oil prices at a time when the Soviets needed hard currency" (Ronen, 2014, p.85). In these difficult times for Libya falls its defeat in the decisive Battle of Aouzou and the withdrawal of many Libyan forces from Chad, followed by a ceasefire in September 1987, which paved the way for a peaceful solution.[5]

Obviously, the anecdotal evidence for the Chadian-Libyan conflict amounts to suggestive correlations at best. Still, we believe that this illustrative example is helpful to fix ideas. As a next step, we shall investigate systematically the causal relationship between exogenous commodity price shocks and winning odds, drawing on over 700 incidents of militarized interstate disputes.

[5]The Chadian-Libyan conflict is just one example among many. Other telling cases of a faction's winning odds in conflict being depressed by adverse world market shocks include Nigeria (harmed by the fall in world oil prices following the Asian economic crisis of 1997-1998) or Uganda (suffering from a historic drop in coffee prices in 1978, following an increase in world supply and contraction in global aggregate demand).

3 Data

In this section, we briefly outline the key data and their definitions and sources.

3.1 Unit of observation

The unit of observation is a given militarized dispute for a given country-pair (dyad) and year. Note that each country pair can be involved in several disputes, and hence appear several times in the sample (which allows us to carry out estimations with dyad fixed effects).

We, however, make sure that each dyad-conflict pair is unique, which means that we assign one country as country A and the other country of a given dyad as country B. Specifically, we always put the country with the alphabetically higher three-digit ISO (country) code as country A (for example, the 2001 Invasion of Afghanistan by the U.S. would only appear as the dyad "USA-AFG" but not as its duplicated counterpart "AFG-USA" in our sample.) Which country is country A has no impact on the estimations, as all variables are defined in relative terms to the opponent country.

3.2 Data on militarized interstate disputes

For constructing our main dependent variable we rely on the most widely used dataset on armed disputes between countries.[6] In particular, we identify conflicts and their outcomes using the Dyadic Militarized Interstate Dispute Dataset (MID) version 4.02 of the Correlates of War project (CoW), see Jones, Bremer and Singer (1996); Maoz et al. (2019).[7][8] An MID is characterized as a historical episode of conflict where threat, display, or use of military force is employed by one state against another one.

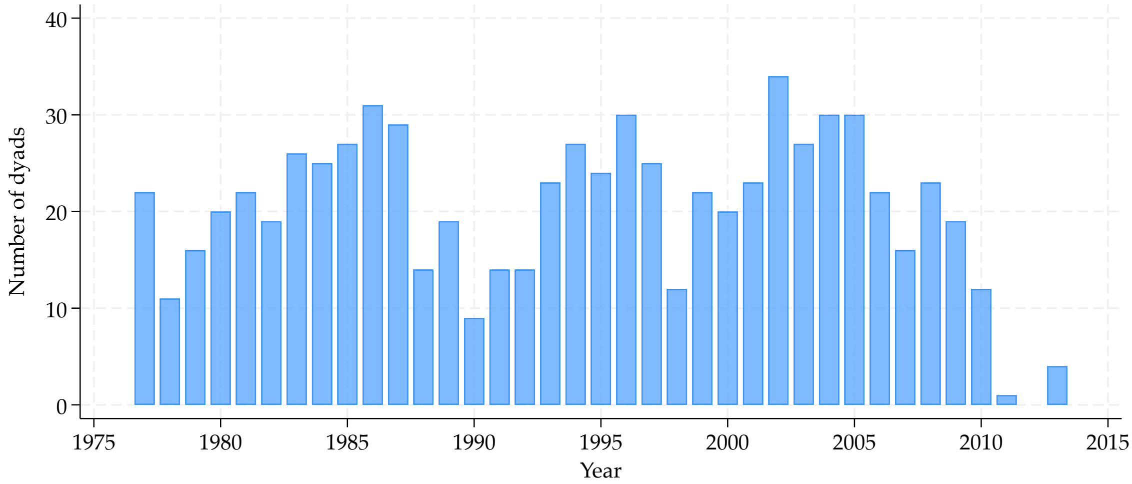
Our sample covers 742 such MID across 135 countries for the period 1977–2013. Note that the sample period is shaped by the joint coverage of our datasets on MID and on commodity trade (described below). The number of distinct conflicts in our sample is 667 (which is less than 742, as some conflicts involve more than two countries).

[6] All data used is described in much more depth in Appendix B.

[7] Note that we are indebted to Zeev Maoz who has shared an updated and corrected dyadic version of this data with us.

[8] The MID data has e.g. been also used in Martin, Mayer and Thoenig (2008); Conconi, Sahuguet and Zanardi (2014); Caselli, Morelli and Rohner (2015), among many others.

Figure 2: Distribution of dyads across time and hostility levels



Note: The Figure shows the number of dyads starting in each year. The low number of conflicts in the 2012–2014 period is due to the exclusion of conflicts without clear outcomes at the time when the dataset was assembled by Maoz et al. (2019).

The MID data distinguishes five intensity levels for each episode of hostilities. The most intense level of hostility (5) is reserved for full-blown ”wars”, which is defined as sustained military combat between state military forces resulting in 1,000 or more combat-related deaths of official military personnel. Note that only 1.4 percent of MID are characterized by the maximum intensity criteria qualifying them as full-fledged wars. Examples of such episodes include for example the wars between Iran-Iraq, Eritrea-Ethiopia, and India-Pakistan, among many others.

The next highest level of dispute intensity (4) is reserved for the ”use of force” and includes blockade, occupation of territory, seizure, attack, clash, declaration of war, or use of chemical, biological, or radioactive (CBR) weapons. This still very violent form of armed interstate conflict is much more frequent, corresponding to 58.4 percent of MID.

The next lower intensity category (3) corresponds to ”display of force”, which includes the show of force, alert, nuclear alert, mobilization, and the fortifying or violation of borders. This category of intensity corresponds to 36.5 percent of MID.

The following category (2) ”threat of force” describes events characterized by a threat to use force, blockade, occupy territory, declare war, use CBR weapons, or join an ongoing war, corresponding to 3.8 percent of the MID.

Figure 2 provides an account of how the included conflicts are distributed over time. Note that the Figure only includes conflicts that are part of our sample, i.e. disputes with a clear-cut victory, yield or tie. The Figure shows, for each year, the number of onsets of dyadic disputes. Overall, conflict onsets seem to be roughly uniformly distributed across time with some slightly less intense spells around the fall of Berlin Wall and at the end of the 1990s before 9/11.[9]

In line with the existing literature (see e.g. Martin, Mayer and Thoenig (2008); Conconi, Sahuguet and Zanardi (2014); Caselli, Morelli and Rohner (2015)), we apply an inclusive definition of militarized interstate disputes, considering not only wars but also lower-intensity conflicts. In particular, in the main analysis we include all MIDs with intensity levels of 2 and above, whereas in robustness checks we show that the results hold entirely once we restrict ourselves to conflicts with at least intensities 3 or 4, respectively (see Appendix Tables C.6 and C.7).

Conveniently, the MID dataset codes war outcomes at a dyadic level into several distinct categories based on extensive narrative accounts. We code ″victory for side A″ and ″yield by side B″ as a win by A (i.e., A achieves a more favorable change of the status quo than the opponent by means of successful military operations or in exchange for stopping a military threat), while analogously ″Victory for side B″ and ″Yield by side A″ correspond to a win by B. We classify a country as the winner in relation to a specific conflict if the outcome indicates either a victory or a yield in its favor and define losers analogously.[10]

The next two categories, "stalemate" and "compromise" are characterized by an outcome that does not favor either side of the conflict or requires both parties to make concessions in such a way that a clear winner cannot be identified. We define a draw for the cases where the MID dataset codes a stalemate or a compromise.

The next two categories, "released" and "unclear", refer to situations in which seized material or personnel was released from captivity or the outcome is unclear due to conflicting historical sources, respectively. We drop all conflicts from the sample falling into the latter two ambiguous categories.[11]

[9] Note that the lower numbers of included conflicts in the last three years of the sample is due to the fact that quite a lot of the conflicts had not clear-cut outcomes (yet) at the time of the data release.

[10] As the expert coding on ″yields″ is also at times not fully clear-cut, we also include a robustness check where we drop all events that are coded as ″yields″, and focus on only observations with a military victory (see Appendix Table C.5).

[11] The MID dataset does not code to what extent or *which* party was successful in releasing the seized material or personnel, suggestive of the difficulties associated with unambiguously

Note that some conflicts in our sample involve large coalitions encompassing a substantial number of countries. For example, the Second Gulf War alone involved more than 55 conflict dyads. In such cases, the effects of windfall gains, especially if only realized for a small subset of countries, become blurry. For this reason, we restrict our baseline to those conflicts that involve less than ten distinct dyadic conflicts, leading to the exclusion of 143 conflicts. As shown in Appendix Figure C.6, our results are very similar if we broaden or narrow down this restriction.

3.3 Data on relative windfall gains

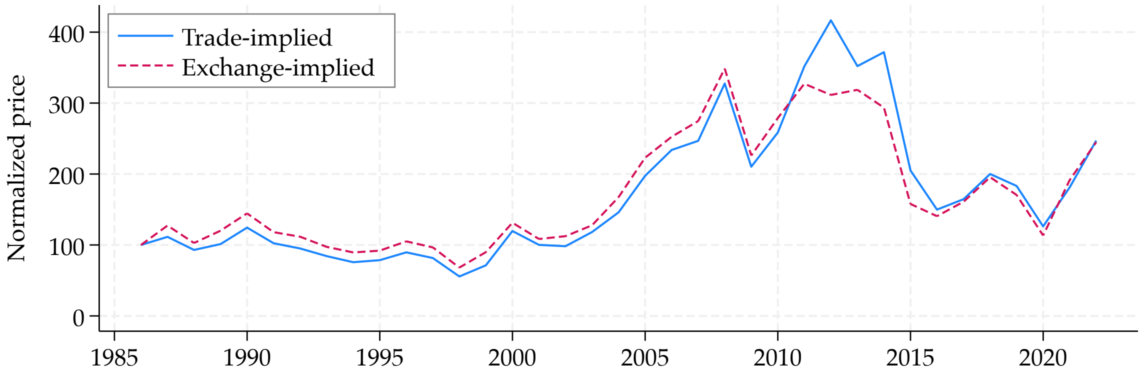
Our key explanatory variable, windfall gains, draws on trade data for all countries in the sample (the exact variable construction will be described in depth below). In particular, the computation of windfall gains requires detailed information on annual product prices, as well as quantities traded across countries. We back out product prices at the 3-digit SITC level using Comtrade (2024). Comtrade comprises, for each importer-exporter-year, the net weights and total free-on-board values of each product traded. The information is reported by either one or both trading partners. We back out the historical price series of each product as the median price paid per unit of net weight in a given year across all reported trades. To account for reporting biases, we only consider products for which we have at least 5 reporting countries in a given year and drop those products that are in the top percentile regarding their price volatility over the whole sample period.

Figure 3 illustrates how our backed-out prices compare against an external pricing source. It shows as an example the normalized oil price for the period 1985–2022. The beginning of the observation period is restricted by the data availability for the red dashed line which depicts the exchange-implied WTI crude oil price as reported by Refinitiv Datastream (2024). The blue solid line shows our trade-implied product price for the SITC code "333", defined as "Petroleum oils, oils from bituminous materials, crude". Strikingly, both price series are almost identical, with the corresponding Pearson correlation exceeding 0.96. This is all the more remarkable, as the trade-implied price series is volume-weighted, whereas the exchange-implied prices are not.

We rely on trade-implied prices for two reasons: (1) The historical availability of data on liquid exchange-traded commodity prices is limited, and (2) many of

attributing a favorable change to a specific party in such cases. Thus, a positive or negative outcome for either party cannot be established based on the information available.

Figure 3: Trade-implied and exchange-implied oil prices



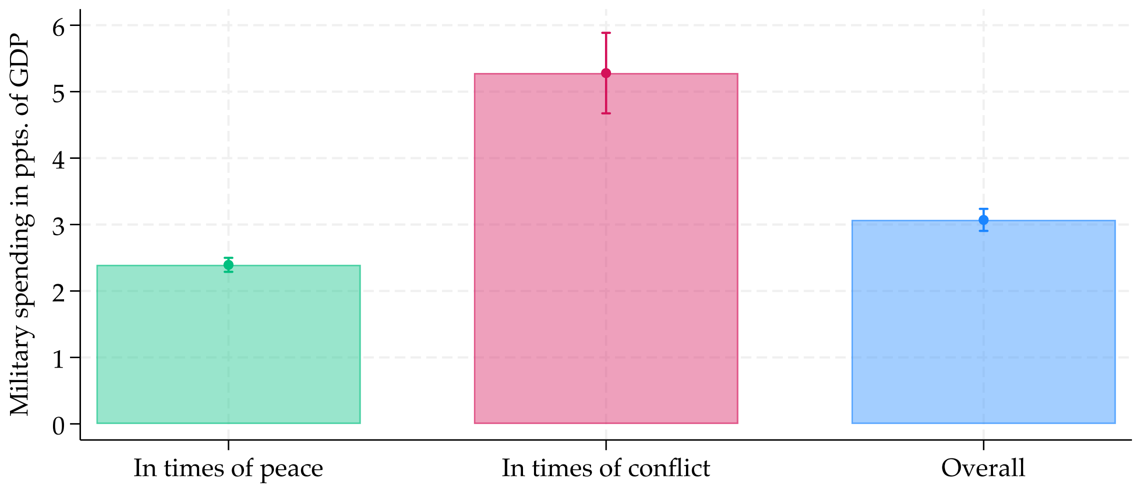
Note: Figure shows and compares oil prices across measurements between 1985–2022. Red dashed line is exchange-implied WTI crude oil price. Blue solid line is trade-implied product price for SITC code "333" ("Petroleum oils, oils from bituminous materials, crude"). Prices normalized to base year 1986.

our products of interest are not standardized to the extent that they can be traded on a centralized exchange. In this regard, we acknowledge that the products within our categories vary to some extent and are not always homogeneous. Ultimately, we value the benefits of the increased sample size associated with using trade-implied prices higher than the benefits of decreasing the noise in our sample by employing exchange-implied prices.

Note that, in principle, Comtrade also comes with free-on-board values of traded goods. However, these data may suffer from reporting biases, particularly so in times of war, as countries may have incentives to disguise their trade and production networks. For this reason, we also consult the Harvard Atlas of Economic Complexity (2024) which adjusts these values for such biases in a two-step procedure: (1) They estimate a country-specific reporting reliability, leveraging the fact that the same trade flows are usually reported twice — by both the importer and exporter country; (2) They adjust reported trade flows based on the reporting reliability of the countries involved.

In summary, our product-price time series used to assess product returns throughout interstate disputes stems from Comtrade and adjusts for reporting biases by using the median price and requiring a minimum of five distinct reporting countries in a given year. In contrast, the total value of net exports of each product-country pair is sourced from the Harvard Atlas of Economic Complexity. Notably, the latter cannot be employed to compute product prices, as it lacks data on traded quantities necessary to construct product prices.

Figure 4: Military spending in times of peace and conflict



Note: The Figure shows averaged military spending relative to GDP in percentage points in times of peace, conflict, and throughout the entire sample.

3.4 Data on military expenditures

As a second major dependent variable, we focus on explaining military expenditures at the country-year level which we source from the World Bank (2024). To keep the expenditure sample consistent with our conflict sample, we only consider the period 1977–2013.

Figure 4 provides a first impression of this data and how it relates to the conflicts in our sample. The figure depicts average military spending relative to GDP on the y-axis. The x-axis distinguishes between two subsamples focusing exclusively on times of peace and conflict, respectively, as well as the entire sample (inclusive of both peace and conflict periods).

Unsurprisingly, in times of peace, as indicated on the very left bar, military spending is lowest with on average about 2 percentage points of GDP. However, this number increases by a factor of 2.5 once we focus on those countries that experience a conflict. Pooling both observations in times of peace and conflict, as shown in the right bar yields an average military spending over GDP ratio of about 3 percentage points.

3.5 Other data

Lastly, we source data on macroeconomic outcomes and controls from various sources: Data on external debt, GDP as well as on U.S. consumer price inflation, used to deflate trade values, which are originally reported in current US dollars, are sourced from the World Bank (2024). Government revenues are drawn from the International Monetary Fund (Mauro et al., 2015). Data on population are sourced from the Maddison project (Bolt and Luiten van Zanden, 2020). Lastly, concordance tables linking country codes are sourced from Federle et al. (2024) and Comtrade (2024).

4 Identification strategy and variable constructions

In this section, we describe our identification strategy and the construction of our windfall gain variable, and present first descriptive statistics.

4.1 Identification strategy

Our goal is to investigate the causal impact of world price shocks on relative winning chances in militarized interstate disputes.

For this purpose, we exploit the fact that the impact of price shocks is different across conflict parties. To illustrate this, in a war between, say, France and the UK, France would benefit relatively more from higher world market prices for wine, as wine is a more important part of the French economy than of the British one. Similarly, in a conflict between Venezuela and Chile, for example, higher global oil prices would favor the former (a net exporter of oil) vis-à-vis the latter (a net importer of oil).

As detailed below, we focus on pre-conflict export-import profiles, and general time-invariant differences between countries and other dyad characteristics get filtered out by dyad fixed effects. To ensure that price shocks are really exogenous to MID, we also provide robustness checks where major producers are excluded. Last but not least, we filter out global shocks using time effects. In what follows, we discuss more formally the variable construction.

4.2 Variable construction

We start by defining a set of conflict-specific variables. Let D be the number of dyadic militarized interstate disputes (MID) that are indexed by $d \in [1, D]$. Each of those conflicts starts in year t_d and lasts for l_d years. Each dyadic dispute, by definition, comprises two opponent countries characterized as $o_d = \{o_d^a, o_d^b\}$. We conveniently refer to the opponents in relation to a certain MID as a or b, to ease the notational burden.

We next proceed to define the absolute value gains in pre-conflict net exports arising from price changes that opponent a experienced over the course of a MID d as

$$\Delta V_d^a = \underbrace{\sum_{i=0}^{l_d-1} \underbrace{\sum_{c \in C} \overbrace{\left(\frac{p_{c,t_d+i}}{p_{c,t_d-1}} - 1\right)}^{\text{Returns on commodity } c} * \overbrace{X^a_{c,t_d-1}}^{\text{Pre-conflict net exports of } c \text{ by } a}}_{\ldots\text{of all commodities}}}_{\ldots\text{over all years in which the conflict is ongoing}} , \tag{4.1}$$

where C is the set of all commodities in our sample, $p_{c,t}$ is the price level of commodity c in period t as defined in the previous section, and X^a_{c,t_d-1} denotes the pre-conflict net exports of commodity c by opponent a in the conflict. As such, our measure captures the cumulative absolute value changes in pre-conflict net exports due to changing commodity prices over the course of the interstate dispute. By focusing on the value change of pre-conflict instead of contemporaneous net exports, we seek to avoid our windfalls being confounded by possible feedback effects of MID into the productive capacity of its parties.

The identifying assumption is then restricted to the exogeneity of global changes in commodity prices. Below, we will in robustness checks show that our results are very robust to restricting our sample to situations where the countries of a given dyad have only very dismal market shares (e.g. below 1 percent), and clearly are pure price takers.

However, *absolute* value gains may only provide limited insights with respect to conflict outcomes. Consider for example the Vietnam War, where certainly a \$50 billion gain for the U.S. would have had an entirely different impact than a gain for Vietnam. To account for the asymmetric effects of windfalls on the opponents, we define the windfall gains of each opponent pair relative to the countries' pre-conflict GDP such that

$$W_d = \frac{\Delta V_d^a}{GDP^a_{t_d-1}} - \frac{\Delta V_d^b}{GDP^b_{t_d-1}} . \tag{4.2}$$

Thus, an increase in the windfall gains of opponent a (b) over the course of dispute d relative to its own pre-conflict GDP due to commodity price changes would lead to an increase (decrease) of W_d in the same magnitude.

Armed with these elements, and knowing that a given dispute d starts in a given country pair (dyad) p and year t, we are now able to formulate the regression specification:

Table 1: Sample of conflicts and windfalls

	Conflicts	Start year		Duration (days)		Windfalls W_d			
		Min	Max	Mean	SD	Mean	SD	Min	Max
Baseline	742	1977	2013	119	278	.009	.167	-1.43	3.213
All disputes	893	1977	2013	150	383	.009	.159	-1.43	3.213

Note: Table presents descriptive statistics for two samples: The baseline sample is restricted to dyadic disputes within broader conflicts comprising at most ten dyads, and entire sample. Table outlines mean ("Mean"), minimum ("Min"), and maximum ("Max") values as well as standard deviations ("SD") for conflict start years, their duration in days, and for windfall gains, as given by Equation (4.2).

$$q_{pt} = \beta \, W_{pt} + \mu_{pt} + FE_p + FE_t + \varepsilon_{pt} \tag{4.3}$$

where the outcome variable $q_{pt} \in \{-1, 0, 1\}$ captures the conflict outcomes (country B winning, draw, or A winning). The main explanatory variable, W_{pt}, denotes the windfall shocks, as detailed above. μ_{pt} represents a vector of control variables varying at the level of dyad p and year t. FE_p denotes dyad fixed effects filtering out any time-invariant confounders. A further set of time fixed effects, FE_t, are included, and filter out aggregate shocks. Last but not least, ε_{pt} corresponds to the error term.

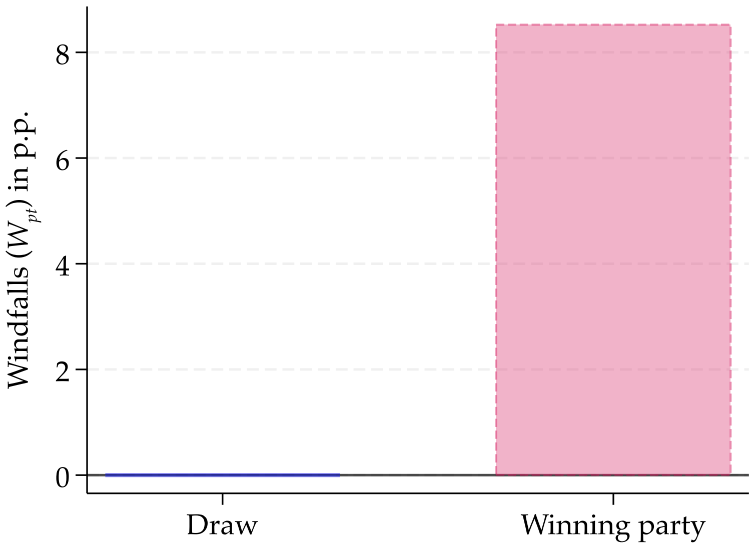
4.3 Descriptive summary statistics

Table 1 outlines some key statistics of our sample. It displays the mean ("Mean"), minimum ("Min"), and maximum ("Max") values, as well as standard deviations ("SD") for conflict start years, their duration in days, and for windfall gains, as given by Equation (4.2) above. On average, the windfall gains are close to zero, but show substantial variation.

For illustration, consider again the case of Libya and Chad, as outlined above. During the first dispute, Chad experienced windfall gains of about 0.22 percentage points of its GDP. At the same time, Libya experienced windfall gains of 143.19 percentage points of its GDP, resulting in relative windfall gains of -1.429.[12] Notably, due to the presence of these limiting cases which exceed the samples' standard deviation severalfold, we conduct extensive robustness tests accounting for outliers.

[12]The negative sign reflects the fact that Chad is country A and Libya is country B in the specific dyad. As mentioned, whether a given country is listed as country A or as country B does neither affect estimations nor alter the interpretation.

Figure 5: Windfalls across winners and losers



Note: The Figure shows mean windfall gains on y-axis for conflicts that ended in draw (left bar) and those which were won by party A (right bar) on x-axis.

5 Main results

To set the stage for our subsequent analysis, we start with a series of stylized facts corroborating the notion of windfall gains being associated with conflict outcomes. After this, we shall present our main results aiming at establishing a causal relationship. Lastly, we present a battery of robustness tests.

Figure 5 depicts some descriptive evidence of our windfall gain variable across different conflict outcomes. The bars indicate the mean value of the relative windfall gain variable across those conflicts that ended in a draw (left bar) and those in which one party won (right bar). The figure indicates that there are on average no windfall gains taking place in situations of conflicts ending in a draw. An altogether other picture emerges, however, once we turn to the red bar on the right, which implies, that the winning party of a conflict, on average, experiences windfall gains which are more than 8 percentage points higher than those of its opponent. Thus, for the winning party, windfall gains are about a half standard deviation higher than for those who end up in a draw.[13]

As striking as these stylized facts are, the direction of causality requires a careful econometric investigation. To carry this out, we draw on the causal

[13]As we look at the difference in windfall gains across parties, these are exactly equal to zero in the case of a draw. The windfalls in case of a victory are significantly larger than zero, as implied by t-test ($p < 0.084$).

identification strategy sketched above, and estimate specification (4.3).

Our unit of observation is a given interstate dispute, which involves a given country pair p and starting in a given year t. The outcome variable q_{pt} captures the conflict outcomes (country A winning, B winning, or a tie), varying at the level of dyad p and starting year t.

The main explanatory variable, W_{pt}, denotes the windfall shocks, as detailed in Equation (4.2). Our coefficient of interest, β, yields an estimate of how a windfall gain of 1 percentage point relative to a country's GDP affects the conflict outcome. μ_{pt} represents a vector of control variables varying at the level of dyad p and year t, including the log difference of GDP and population across the opponents, respectively. Further, as discussed above, we include in the most demanding specifications both dyad fixed effects and conflict start year fixed effects. We cluster the standard errors at the dyad level.[14]

Table 2 displays the results of estimating a set of varying specifications of Equation (4.3). The leftmost column (1) depicts the estimates for the case in which we neither include controls nor absorb any fixed effects. Here, we find that an increase in the relative windfall gains for one party by 10 percentage points increases the chance of falling into the next higher outcome category, i.e., ending up in a draw instead of losing or winning instead of ending up in a draw, by 2.45 percentage points.

The point estimate remains stable and the statistical significance unchanged once we turn to column (2), where we include controls for the countries' relative sizes in terms of both GDP and population. Note that the missing observations in these control variables lead to a drop in the sample size.

In columns (3) and (4) we progressively introduce dyad and year fixed effects. We observe the windfall gain coefficients to slightly increase while remaining statistically significant at the 1 percent level. Given that some country dyads experience a bilateral dispute only once, including dyad fixed effects will drop them from the sample, which accounts for the reduction in the number of observations.

We check the robustness of our findings along a number of different dimensions.

Price takers: First, we address potential concerns that conflicts may themselves impact global supply and hence affect world commodity prices. Concretely, one may think that fighting can disrupt the production of particular goods, and

[14]Note that our results are robust to employing a two-way clustering at the dyad and conflict start year level, to only clustering at the year level, and to computing heteroscedasticity robust standard errors, see Appendix Tables C.8, C.9, and C.10.

Table 2: The effects of windfall gains on conflict outcomes

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.245***	0.253***	0.296***	0.321***
	(0.0805)	(0.0772)	(0.0828)	(0.0847)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.019	0.020	0.082	0.081
N dyads (clusters)	226	212	121	121
N	742	688	597	596

Note: Table shows results of estimating Equation (4.3). Standard errors clustered at dyad level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

scarcity may fuel price spikes. We do not think that this is a major concern in our particular case. In particular, given that major production disruptions mostly happen in full-fledged wars, such biases are unlikely to drive our results (remember, that only 1.4 percent of MID are characterized by the maximum intensity level 5). Further, in principle such price shocks should – if anything – bias the results against our findings, for the following reason: On average, the losing party is more likely to experience violent destruction on its soil which may hamper its ability to sustain commodity exports, thereby driving up the global prices of precisely those goods for which the losing party has significant market power.

Beyond this argument, to address this issue more formally, we employ a set of estimations where we only include goods in the calculation of the windfall gains in each dyad, for which the combined net exports of both conflict parties sum to at most 10%, 5%, 1% or 0.1% of global trade, respectively. In this way, we seek to exclude those observations for which a reverse causality seems theoretically possible. Our results are robust to this demanding sensitivity test, see Appendix Table C.2.

Placebo: Next, we display in Appendix Figure C.1 two placebo tests speaking to potential outliers in our dataset and possibly other idiosyncratic properties of our empirical setup. Both panels in the Figure depict the distribution of our windfall gain coefficient across 1,000 simulations. For the left panel, we shuffled in each simulation the windfall gains across the observations in our baseline sample. In this way, the left panel serves as a robustness test for outliers driving our results: If only a handful of observations were driving the results, then they may yield spurious significant results in the placebo estimations, which reassuringly is not the case. In particular, we only observe our baseline estimate to be reached in 1 out of 1,000 simulations once we randomly shuffle windfall gains.

In contrast, the right-side panel in the Figure is set to examine the dynamics at a more fundamental level: Here, the point of departure is again our baseline sample. However, we now randomize the start years of the disputes for each simulation in the sample. Again, the results suggest our baseline estimate to be very unlikely reached by pure chance.

Outlier analysis: As a next step, we conduct a battery of sensitivity analyses related to dropping observations. We begin by examining how our windfall coefficient changes once we drop specific dyads from our sample, always one at a time. We next do the same for entire countries and conflicts. Reassuringly, this analysis suggests that it is unlikely that our results are driven by specific dyads, countries, or broader conflicts. Throughout all specifications, the coefficient of interest maintains significance and remains quantitatively close to the baseline estimates, as shown in Appendix Figures C.2, C.3, and C.4.

Products: In what follows, we examine whether our measured effect is driven by windfall gains stemming from a single product, such as e.g. crude oil. Much in the spirit of the previous robustness tests, we re-run our empirical analysis but exclude entire products from the calculation of our windfall gain measurement, again one at a time. Across all estimations, the coefficient is positive and significantly different from zero (see Appendix Figure C.5).

Dispute participants thresholds: Recall that in our baseline, we drop conflicts with excessively large coalitions from our sample, as we presume our bilateral windfall measure loses meaning if the number of countries involved in the conflict is too large. For this reason, we restrict our baseline to those conflicts which involve at most ten individual dyadic disputes. We document, however, that our results are highly robust to variations in this threshold and to re-estimating our baseline on the whole sample (see Appendix Figure C.6). Note that when we restrict the sample to conflicts featuring only very few participants, the windfall coefficient remains of stable magnitude, but due to the dropping of observations, the standard errors get mechanically inflated, degrading statistical significance.

Alternative estimators: Next, we examine our findings in the light of alternative estimators. First, we re-estimate our baseline regression using an ordered logit model to account for the interval nature of our outcome variable (see Appendix Table C.3). Second, we account for the varying length of conflicts, and therefore the inherently different signal-to-noise ratio of our annually observed windfall gains measure (see Appendix Table C.4).[15]

Focusing on militarily decisive outcomes: In our baseline analysis we code conflict victory as either (militarily) winning or gaining concessions (i.e. outcomes where one party ”yields”). Given that at times the coding of such concessions may be debatable, we perform in Appendix Table C.5 a sensitivity test, where we drop such ”yield” outcomes, and focus on clear-cut (military) victories only. Reassuringly, our findings are unchanged for this alternative sample.

Restricting the sample to higher intensity conflicts: In Appendix Tables C.6 and C.7, we restrict the sample to conflicts only that experience at least an intensity level of 3 or 4, respectively. One purpose of this robustness test is that more intense conflicts may be subject to less reporting bias or leeway in coding. As shown in Appendix Tables C.6 and C.7, the results are – if anything – even stronger for more intense conflicts.

6 Mechanisms and channels of transmission

6.1 Unit of analysis and definitions

In this section, we examine the mechanisms through which windfall gains affect conflict outcomes. To start with, we move from the dyadic conflict level to another unit of analysis, the country level in a given year. We accordingly also modify our windfall gain variable, in order to explore how windfall gains affect the economy more generally. In this way, we are able to verify that windfall gains indeed significantly increase government revenues and military expenditures. After gaining a big-picture overview, we zoom in on the effect during times of conflict.

As the questions we seek to answer throughout this section are not exclusively related to conflict but also to the general dynamics between windfall gains and

[15]To this end, we assign each conflict a weight given by $\omega_d = \frac{dur_d}{(year_d^{end} - year_d^{start} + 1) * 365}$, where dur_d is the conflict's duration in days. As such, a war that only lasts 10 days is also only assigned a dismal weight in the regression. As windfall gains are only observed at an annual level, such a weighting can account for the differing information content of the gains regarding conflicts of varying lengths.

the economy, we slightly modify our windfall gain measure defined in Equation (4.2) to capture now the year-on-year windfalls due to commodity price changes, such that

$$W_{i,t}^{yoy} = \sum_{c \in C} \underbrace{\left(\frac{p_{c,t}}{p_{c,t-1}} - 1 \right)}_{\text{Return on commodity } c} * \underbrace{X_{i,t-1,c}}_{\text{Prior-year net exports of } c \text{ by } i} . \tag{6.1}$$

In this way, the windfall measure reflects the fact that — in contrast to interstate dispute outcomes — macroeconomic outcomes such as military expenditures are not defined on a dyadic scale.

Here, C is again the set of different commodities in our sample, $p_{c,t}$ is the price of commodity c in period t, and $X_{i,t,c}$ are country i's net exports of commodity c in year t. We then normalize the absolute windfall gains, $W_{i,t}^{yoy}$, by the pre-period GDP of country i in order to arrive at our final shock term used to examine the effects of windfall gains on the economy throughout this section,

$$\varepsilon_{i,t} = \frac{W_{i,t}^{yoy}}{GDP_{i,t-1}} . \tag{6.2}$$

Lastly, we presume that windfalls may have differential effects on the economy depending on whether they are gains or losses. We therefore employ signed versions of the shock terms. More formally, we define a positive windfall shock as

$$\varepsilon_{i,t}^{+} = \begin{cases} \varepsilon_{i,t} & \text{if} \quad \varepsilon_{i,t} > 0 \\ 0 & \text{otherwise} \end{cases} , \tag{6.3}$$

and a negative windfall shock as

$$\varepsilon_{i,t}^{-} = \begin{cases} -\varepsilon_{i,t} & \text{if} \quad \varepsilon_{i,t} < 0 \\ 0 & \text{otherwise} \end{cases} . \tag{6.4}$$

6.2 Windfall gains and the economy

Equipped with these variables, we can estimate the impact of commodity windfall gains on the economy in general. Again, our point of departure is a set of simple ordinary least squares regressions of the following form:

$$x_{i,t} = \alpha_i + \eta_t + \psi^{+} \varepsilon_{i,t}^{+} + \psi^{-} \varepsilon_{i,t}^{-} + u_{i,t} , \tag{6.5}$$

Where α_i and η_t denote country and year fixed-effects, and $x_{i,t}$ is the two-period change of either military expenditures, government revenues, or external debt, relative to the pre-shock GDP.[16] The term ψ^+ (ψ^-) yields an estimate of how these variables respond to a positive (negative) windfall shock. Standard errors are two-way clustered by countries and years.[17]

The top panel of Table 3 depicts descriptive statistics for the key outcome variables. We see that the military expenditures of the countries in our sample average about 3 percentage points of GDP. The numbers for government revenues and external debt equal about 30 and 56 percentage points of GDP, respectively.

The bottom panel of the table depicts the results of estimating Regression (6.5) above. The three columns outline the signed windfall gain coefficients for separate regressions, each differing with respect to their dependent outcome variable, as denoted in the top row of the table. Additionally, for each specification, we test whether the estimated coefficients of a positive windfall shock significantly differ from those of a negative shock, using a set of Wald tests.

Throughout all specifications, we find significant associations between the windfall gains and these macroeconomic outcomes. In particular, we find that a positive 1 percentage point windfall gain relative to the pre-period GDP leads to an increase in military expenditures by about 0.04 percentage points, also relative to the pre-period GDP. For government revenues, this figure is even at 0.32 percentage points. In line with our expectations, we also see a significant reduction in external debt, corroborating the idea of the proceeds partially being used to pay off existing debt.

Somewhat surprisingly, we do not find significant effects of negative windfall gains which may result from the positive skew inherent to the windfall gain distribution.[18] As such, we have more and a lot of higher windfall gains in the positive range than we have in the negative one. This could mean that more identifying variation eases finding significant results for the positive windfall gains, compared to the negative ones. The fact that exogenous surges in government revenues (from windfalls or inter-governmental grants) trigger more public spending changes than other constellations (income changes, negative shocks)

[16]Note that we look at the two-period change, as fiscal years do not match calendar years for quite a few countries in our sample. Therefore, realized windfall gains in a given year may only show up in national accounts of the subsequent year.

[17]Note that our results are robust to clustering only at the year or country level as well as to computing heteroscedacity robust standard errors, see Appendix Tables C.11-C.17.

[18]While asset returns have a natural lower bound, as their underlying value cannot drop by more than 100%, they do not have an upper bound, rendering the positive skew in return distributions a common mechanical feature in finance applications.

Table 3: Unconditional effects of windfall gains

	$\frac{\text{Military expenses}}{\text{GDP}}$	$\frac{\text{Government revenue}}{\text{GDP}}$	$\frac{\text{External debt}}{\text{GDP}}$
Panel A: Descriptive statistics			
Mean	0.031	0.297	0.557
Median	0.017	0.293	0.430
SD	0.070	0.137	0.510
Panel B: Regression results			
Windfall (pos.)	0.0433*	0.320***	-0.123**
	(0.0229)	(0.0900)	(0.0565)
Windfall (neg.)	-0.000989	0.0397	-0.00243
	(0.00153)	(0.0425)	(0.00874)
Difference	0.044*	0.280***	-0.121**
F-Statistic	3.913	11.968	5.125
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.135	0.140	0.230
N	4,329	1,690	3,511

Note: Standard errors two-way clustered at country year level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

is also in line with the so-called ”flypaper effect” from the public economics literature (Courant, Gramlich and Rubinfeld (1978)).

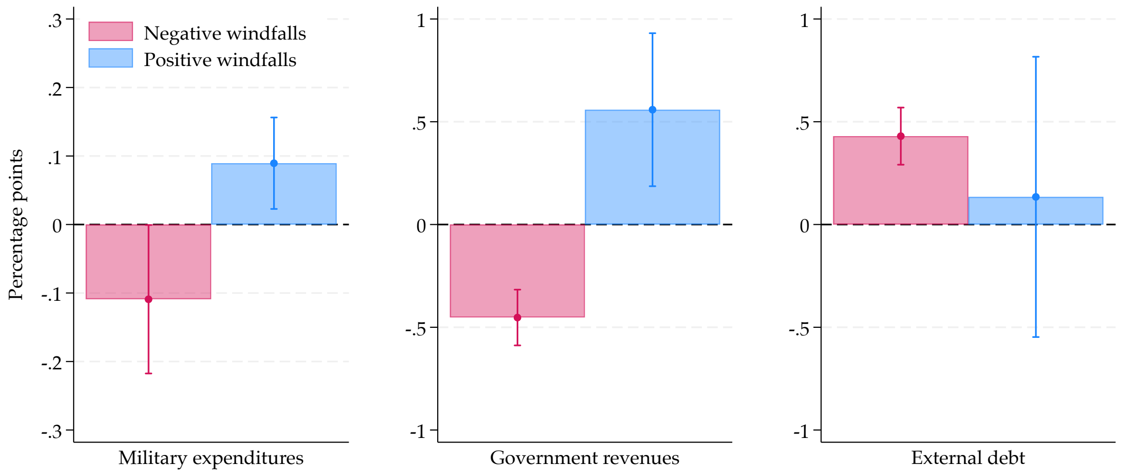
This being said, we see, however, that throughout all variables the differences between positive and negative windfall gains are statistically significant and point towards the expected direction. In summary, we think of Table 3 as evidence of commodity windfall gains increasing government revenues. Thereby, they contribute to the state’s ability to sustain military expenditures.

6.3 Windfall gains in times of conflict

We now go one step further and condition the estimation of the effects on the presence of conflict. To this end, we estimate the ordinary least squares regression

$$\begin{aligned} x_{i,t} = \alpha_i + \eta_t + \beta conflict_{i,t} + \psi_c^+ conflict_{i,t}\varepsilon_{i,t}^+ + \psi_c^- conflict_{i,t}\varepsilon_{i,t}^- \\ +\psi_p^+(1 - conflict_{i,t})\varepsilon_{i,t}^+ + \psi_p^-(1 - conflict_{i,t})\varepsilon_{i,t}^- + u_{i,t+h}\,, \end{aligned} \tag{6.6}$$

Figure 6: Impact of windfall changes of 1 percentage point of GDP during conflict



Note: The Figure shows responses of military expenditures, government revenues, and external debt relative to pre-shock GDP to 1 percentage point windfall shocks in times of conflict. Red bars indicate responses to negative windfalls, blue bars indicate responses to positive windfalls. Vertical lines signify 90% confidence bands. More detailed results provided in Appendix Table C.14.

where $conflict_{i,t}$ is a dummy variable indicating whether country i was in a conflict in year t. In this way, Equation (6.6) enables us to capture the heterogeneous effects of windfall gains in times of conflict ($\psi_c^{\cdot}$).

Figure 6 depicts the estimated coefficients for the effects in times of conflict. The figure shows, as before, for military expenditures, government revenues, and external debt, the response to a one percentage point windfall shock relative to the pre-shock GDP, simply now restricting the response to times of conflict. It turns out that in times of conflict, a positive windfall shock in the height of 1 percentage point of pre-shock GDP, leads to an increase in government revenues of more than 0.5 percentage points — also relative to the pre-shock GDP. Analogously, a negative shock of similar magnitude leads to a similar change of government revenues in the opposite direction.

Intuitively and very much in line with our proposed channel through which windfall gains influence conflict outcomes, we find that military expenditures in times of conflict change significantly in response to both positive and negative windfall gains, and much more so in times of conflict than in times of peace. In fact, in times of conflict, the sensitivity of military expenditures to windfall gains is more than four times higher than in times of peace.[19] Furthermore, we

[19] The corresponding results for the effects of windfalls in times of peace are presented in Table C.14.

see that in times of conflict external debt significantly increases in response to a negative windfall shock.

7 Conclusion

The outcomes of military conflicts between competing neighboring states or superpowers have shaped the history of the world. It is not sure if our children would learn Latin in school if Hannibal and his Carthaginian army had not lost against Rome. Also, if the Allied Forces had not prevailed in World War II, we may be living (or rather vegetating) under the yoke of a totalitarian regime, with the world's dominant *lingua franca* maybe being German instead of English. And if the Soviets had won the Cold War, children growing up in Central and Eastern Europe may not be able to choose where to live, what to work, and what political views to hold.

In all of these cases, historical accounts stress the paramount importance of "deep pockets". And while the informal argument of "deep pockets matter for conflict outcomes" has been made in specific contexts of particular conflicts, and a series of papers have studied correlations between economic and military factors, to the best of our knowledge there does not exist so far any causal estimates of the impact of exogenous financial shocks on military winning odds. The purpose of the current paper has exactly been to address this key shortcoming in the literature. To tackle this gap, we have assembled a fine-grained dataset covering relative dyadic resource windfall gains (triggered by global price shocks) and militarized interstate disputes from 1977 to 2013.

We have detected a statistically significant and quantitatively large impact of windfall gains on winning odds. In particular, we find that quantitatively a 10 percentage point windfall gain increases the probability of ending up in a draw instead of losing or winning instead of ending up in a draw by about 3.2 percentage points. A key mechanism and channel of transmission is a surge in military spending in the aftermath of positive windfall gains.

Further research on this important topic is very much needed. In particular, an important open question is to what extent channels of transmission work differently in democratic regimes and in autocracies. Maybe the latter channel greater proportions of windfall gains to the military and maybe there are differences in how an additional dollar or ruble translates into military capacities.

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Online Appendix to
Who wins wars?

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A Formal model

To formalize the theoretical predictions, we shall start from a very simple standard contest model with endogenously selected fighting efforts (see e.g. Konrad (2009)). This framework is enriched by distinguishing different levels of operational performance induced by differences in access to funding (following a similar approach as Bonadio et al. (2024)).

In particular, there are two conflict parties, countries $i = A, B$, who fight over some "prize" (R) that typically includes the control of the resources belonging to the two countries, which are of value $R_A + R_B$, as well as other rents ψ (that could be ideological rents, geoeconomic gaines or any other stakes of the dispute). In sum, $R \equiv \rho(R_A + R_B) + \psi$. For simplicity we assume without loss of generality that the winner gains control over the entire "prize", and that a share ρ of resource rents can be appropriated by the victorious regime.

The winning probability for A, p_A, is given by equation (A.1) (and B is assumed to win with likelihood $(1 - p_A)$). The results would be qualitatively unchanged if we were to also include a "tie" as additional outcome (yet this would come at the cost of a greater algebraic complexity).

$$p_A \equiv \frac{\phi_A f_A}{\phi_A f_A + \phi_B f_B}. \tag{A.1}$$

The choice variables f_A and f_B correspond to the total fighting efforts of the two factions, capturing the sizes of armies fielded. The fighting technology parameter $\phi_A \equiv \lambda_A R_A$ captures the notion that a share λ_A of resources R_A is used to strengthening the fighting technology (and it is analogous for ϕ_B).[20] This is very similar to the assumption that remittances support fighting strength in Bonadio et al. (2024).

There is, as usual in contest models, a time constraint, $1 - f_i$, and $f_i w_i$ captures the opportunity cost for a given country i of fielding soliders who otherwise could carry out productive work at salaries w_i.

Putting together these building blocks, we obtain the following payoff functions:

$$V_A \equiv R\frac{\phi_A f_A}{\phi_A f_A + \phi_B f_B} + (1 - f_A)w_A, \tag{A.2}$$

[20] It is assumed that the share ρ of resource rents appropriated and the rents share λ used for strengthening the fighting technology sum up to less than one, i.e. $\lambda_A + \rho < 1$, $\lambda_B + \rho < 1$.

$$V_B \equiv R\frac{\phi_B f_B}{\phi_A f_A + \phi_B f_B} + (1 - f_B)w_B. \tag{A.3}$$

When computing the first order conditions of V_A with respect to f_A and of V_B with respect to f_B, we obtain a system of two equations and two unknowns (f_A, f_B). Solving the best replies to the opponent's best reply, we obtain the following unique stable Nash Equilibrium:

$$f_A = \frac{\phi_A \phi_B c_B}{(\phi_A c_B + \phi_B c_A)^2} R, \tag{A.4}$$

$$f_B = \frac{\phi_A \phi_B c_A}{(\phi_A c_B + \phi_B c_A)^2} R. \tag{A.5}$$

This leads to the following equilibrium winning odds:

$$p_A \equiv \frac{\phi_A c_B}{\phi_A c_B + \phi_B c_A}. \tag{A.6}$$

Note that the winning chances are independent of the total "prize" R. Hence, when a given country, say, A has a positive windfall shock, ΔR_A, this leads in equation (A.6) only to an increase in ϕ_A.

Proposition 1: *An positive windfall shock in country i, ΔR_i, increases p_i, while a positive windfall shock in opponent country j, ΔR_j, decreases p_i.*

Proof: We have $\partial p_A / \partial \phi_A > 0$ and $\partial p_A / \partial \phi_B < 0$. **QED**

Hence, put in plain English, if a given country benefits from a positive windfall shock, our setting predicts an unambiguous increase in its winning odds.

B Additional data description

Table B.1: Variable descriptions

Variable	Sources	Notes
Dyadic militarized interstate disputes	Jones, Bremer and Singer (1996); Maoz et al. (2019)	Deviating from original sources, we use an updated and corrected dyadic militarized interstate dispute dataset which Zeev Maoz shared with us in April 2024.
Bilateral trade between countries	Harvard Atlas of Economic Complexity (2024)	Accessed on April 19, 2024
Commodity returns	Comtrade (2024)	Backed out as median price paid across bilateral trade flows in a given year. Price defined as free-on-board value per net weight of product. Bulk data download between April 20, 2024 and May 28, 2024.
Military Expenditures	World Bank (2024)	Accessed on March 28, 2024.
External debt	World Bank (2024)	Accessed on March 7, 2024.
GDP	World Bank (2024)	Accessed on June 28, 2024.
U.S. CPI inflation	World Bank (2024)	Accessed on March 28, 2024.
Government Revenues	Mauro et al. (2015)	Accessed on March 28, 2024.
Population	Bolt and Luiten van Zanden (2020)	Accessed on March 28, 2024.

Note: Table outlines sources and construction of variables for our sample.

C Robustness tests and further evidence

In what follows, we include a series of Tables and Figures with robustness results, as discussed in the main text.

Table C.2: Excluding products where parties have market power

	Conflict outcome (q_{pt})			
	Restricting net export share to be...			
	$< 10\%$	$< 5\%$	$< 1\%$	$< 0.1\%$
Windfalls (W_{pt})	0.452***	0.392**	0.305*	0.498*
	(0.158)	(0.158)	(0.170)	(0.271)
Controls	✓	✓	✓	✓
Dyad fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Adj. R^2	0.073	0.057	0.047	0.050
N dyads (clusters)	121	121	121	121
N	596	596	596	596

Note: Table shows results of estimating Regression (4.3). Products where the combined net exports of both conflict parties relative to global exports exceed 10%, 5%, 1%, or 0.1% are excluded. Standard errors clustered at dyad level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.3: Ordered logit

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	1.020**	1.020***	1.048**	1.062***
	(0.00823)	(0.00784)	(0.0195)	(0.0202)
Controls		✓	✓	✓
Dyad dummies			✓	✓
Year dummies				✓
Pseudo R^2	0.020	0.024	0.382	0.476
N dyads (clusters)	226	212	121	121
N	742	688	597	596

Note: The table shows results of estimating Regression (4.3) in an ordered logit model. Coefficients scaled to represent a 1% windfall. Standard errors clustered at dyad level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.4: Weighing observations by conflict lengths

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.356***	0.356***	0.382***	0.397***
	(0.0739)	(0.0733)	(0.0997)	(0.0691)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.158	0.180	0.332	0.493
N dyads (clusters)	226	212	121	121
N	742	688	597	596

Note: The table shows results of estimating Regression (4.3). Each observation is weighted with the length of the conflict. Standard errors clustered at dyad level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.5: Exclude dyads with outcome "yield"

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.292***	0.300***	0.356***	0.367***
	(0.0714)	(0.0688)	(0.0798)	(0.0822)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.061	0.078	0.178	0.198
N dyads (clusters)	225	211	118	118
N	710	656	563	562

Note: The table shows results of estimating Regression (4.3). Each observation is weighted with the length of the conflict. Standard errors clustered at dyad level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.6: Restricting to minimum hostility level: 3

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.247***	0.255***	0.295***	0.319***
	(0.0805)	(0.0766)	(0.0837)	(0.0862)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.020	0.022	0.079	0.083
N dyads (clusters)	218	204	118	118
N	714	660	574	573

Note: The table shows results of estimating Regression (4.3). Standard errors clustered at dyad level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.7: Restricting to minimum hostility level: 4

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.327***	0.324***	0.362***	0.392***
	(0.0802)	(0.0778)	(0.102)	(0.0946)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.055	0.058	0.122	0.128
N dyads (clusters)	162	150	76	75
N	442	405	331	328

Note: The table shows results of estimating Regression (4.3). Standard errors clustered at dyad level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.8: Windfall gains and conflict outcomes – Standard errors clustered at start year level)

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.245***	0.253***	0.296***	0.321***
	(0.0799)	(0.0820)	(0.0817)	(0.0790)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.019	0.020	0.082	0.081
N dyads (clusters)	36	36	36	35
N	742	688	597	596

Note: The table shows results of estimating Regression (4.3). Standard errors clustered at conflict start year level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.9: Windfall gains and conflict outcomes – Standard errors clustered at dyad and start year level)

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.245***	0.253***	0.296***	0.321***
	(0.0844)	(0.0861)	(0.0853)	(0.0849)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.019	0.020	0.082	0.081
N dyads (clusters)			36	35
N	742	688	597	596

Note: The table shows results of estimating Regression (4.3). Standard errors two-way clustered at dyad and conflict start year level and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.10: Windfall gains and conflict outcomes – Robust standard errors

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.245***	0.253***	0.296***	0.321***
	(0.0756)	(0.0724)	(0.0780)	(0.0794)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.019	0.020	0.082	0.084
N dyads (clusters)				
N	742	688	597	596

Note: The table shows results of estimating Regression (4.3). Standard errors are robust to heteroscedasticity and reported in round brackets. Controls comprise log differences of the opponent's GDP and population, respectively. Sample: Baseline *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.11: Unconditional effects of windfall gains – Standard errors clustered at country level

	$\frac{\text{Military expenses}}{\text{GDP}}$	$\frac{\text{Government revenue}}{\text{GDP}}$	$\frac{\text{External debt}}{\text{GDP}}$
Panel A: Descriptive statistics			
Mean	0.031	0.297	0.557
Median	0.017	0.293	0.430
SD	0.070	0.137	0.510
Panel B: Regression results			
Windfall (pos.)	0.0433**	0.320***	-0.123**
	(0.0195)	(0.103)	(0.0497)
Windfall (neg.)	-0.000989	0.0397	-0.00243
	(0.00139)	(0.0386)	(0.0134)
Difference	0.044**	0.280***	-0.121**
F-Statistic	5.442	13.349	6.800
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.135	0.140	0.230
N	4,329	1,690	3,511

Note: Standard errors clustered at country level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.12: Unconditional effects of windfall gains – Standard errors clustered at year level

	$\frac{\text{Military expenses}}{\text{GDP}}$	$\frac{\text{Government revenue}}{\text{GDP}}$	$\frac{\text{External debt}}{\text{GDP}}$
Panel A: Descriptive statistics			
Mean	0.031	0.297	0.557
Median	0.017	0.293	0.430
SD	0.070	0.137	0.510
Panel B: Regression results			
Windfall (pos.)	0.0433**	0.320***	-0.123
	(0.0210)	(0.0898)	(0.0900)
Windfall (neg.)	-0.000989	0.0397	-0.00243
	(0.00167)	(0.0599)	(0.0170)
Difference	0.044**	0.280**	-0.121
F-Statistic	4.691	7.030	1.882
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.135	0.140	0.230
N	4,329	1,690	3,511

Note: Standard errors clustered at year level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.13: Unconditional effects of windfall gains – Robust standard errors

	$\frac{\text{Military expenses}}{\text{GDP}}$	$\frac{\text{Government revenue}}{\text{GDP}}$	$\frac{\text{External debt}}{\text{GDP}}$
Panel A: Descriptive statistics			
Mean	0.031	0.297	0.557
Median	0.017	0.293	0.430
SD	0.070	0.137	0.510
Panel B: Regression results			
Windfall (pos.)	0.0433**	0.320***	-0.123
	(0.0172)	(0.104)	(0.0854)
Windfall (neg.)	-0.000989	0.0397	-0.00243
	(0.00152)	(0.0569)	(0.0197)
Difference	0.044***	0.280***	-0.121
F-Statistic	7.157	7.519	2.091
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.135	0.141	0.230
N	4,329	1,690	3,511

Note: Standard errors robust to heteroscedasticity and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.14: Spending impact of windfall changes in times of conflict and peace

	Military expenses	Gov. revenue	Ext. debt
Windfall (+), conflict	0.0894**	0.559**	0.134
	(0.0395)	(0.220)	(0.403)
Windfall (-), conflict	-0.109*	-0.453***	0.430***
	(0.0641)	(0.0801)	(0.0822)
Windfall (+), peace	0.0165*	0.208**	-0.103*
	(0.00865)	(0.0838)	(0.0592)
Windfall (-), peace	-0.00105*	0.0680**	-0.0107
	(0.000543)	(0.0314)	(0.0118)
War	0.000549	0.000235	0.00869
	(0.00137)	(0.00317)	(0.00800)
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.145	0.156	0.234
N	4,329	1,690	3,511

Note: The table shows results of estimating equation (6.6). Standard errors two-way clustered at country year level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.15: Spending impact of windfall changes in times of conflict and peace – Standard errors clustered at country level

	Military expenses	Gov. revenue	Ext. debt
Windfall (+), conflict	0.0894**	0.559***	0.134
	(0.0448)	(0.190)	(0.354)
Windfall (-), conflict	-0.109*	-0.453***	0.430***
	(0.0624)	(0.0672)	(0.0624)
Windfall (+), peace	0.0165*	0.208**	-0.103*
	(0.00924)	(0.0939)	(0.0528)
Windfall (-), peace	-0.00105	0.0680**	-0.0107
	(0.000841)	(0.0296)	(0.0150)
War	0.000549	0.000235	0.00869
	(0.00135)	(0.00282)	(0.00864)
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.145	0.156	0.234
N	4,329	1,690	3,511

Note: The table shows results of estimating equation (6.6). Standard errors clustered at country level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.16: Spending impact of windfall changes in times of conflict and peace – Standard errors clustered at year level

	Military expenses	Gov. revenue	Ext. debt
Windfall (+), conflict	0.0894***	0.559**	0.134
	(0.0300)	(0.242)	(0.355)
Windfall (-), conflict	-0.109	-0.453***	0.430***
	(0.0666)	(0.100)	(0.154)
Windfall (+), peace	0.0165*	0.208**	-0.103
	(0.00842)	(0.0978)	(0.0925)
Windfall (-), peace	-0.00105	0.0680**	-0.0107
	(0.000791)	(0.0325)	(0.0264)
War	0.000549	0.000235	0.00869*
	(0.000899)	(0.00283)	(0.00456)
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.145	0.156	0.234
N	4,329	1,690	3,511

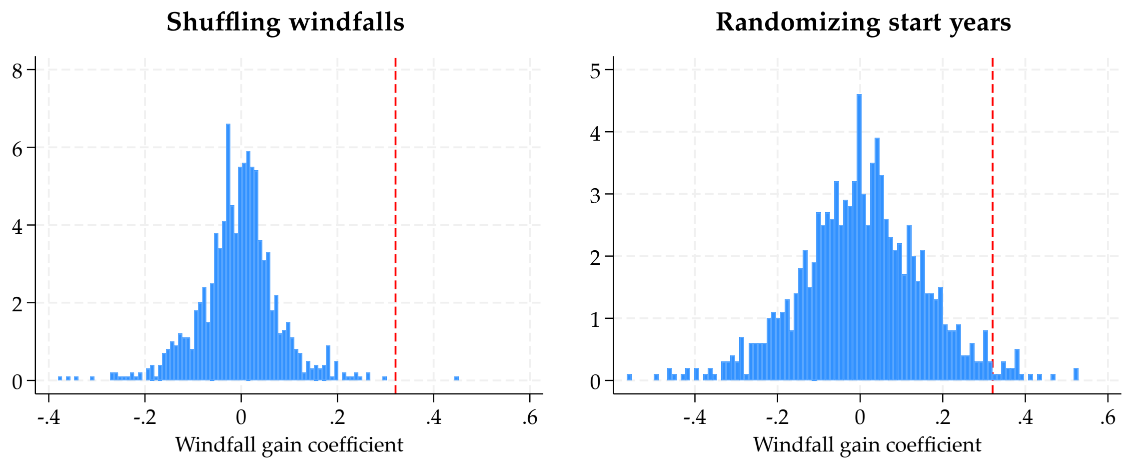
Note: The table shows results of estimating equation (6.6). Standard errors clustered at year level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.17: Spending impact of windfall changes in times of conflict and peace – Robust standard errors

	Military expenses	Gov. revenue	Ext. debt
Windfall (+), conflict	0.0894**	0.559***	0.134
	(0.0370)	(0.214)	(0.296)
Windfall (-), conflict	-0.109*	-0.453***	0.430***
	(0.0648)	(0.0933)	(0.143)
Windfall (+), peace	0.0165*	0.208*	-0.103
	(0.00904)	(0.107)	(0.0882)
Windfall (-), peace	-0.00105	0.0680**	-0.0107
	(0.00103)	(0.0317)	(0.0279)
War	0.000549	0.000235	0.00869
	(0.000907)	(0.00246)	(0.00588)
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.145	0.157	0.234
N	4,329	1,690	3,511

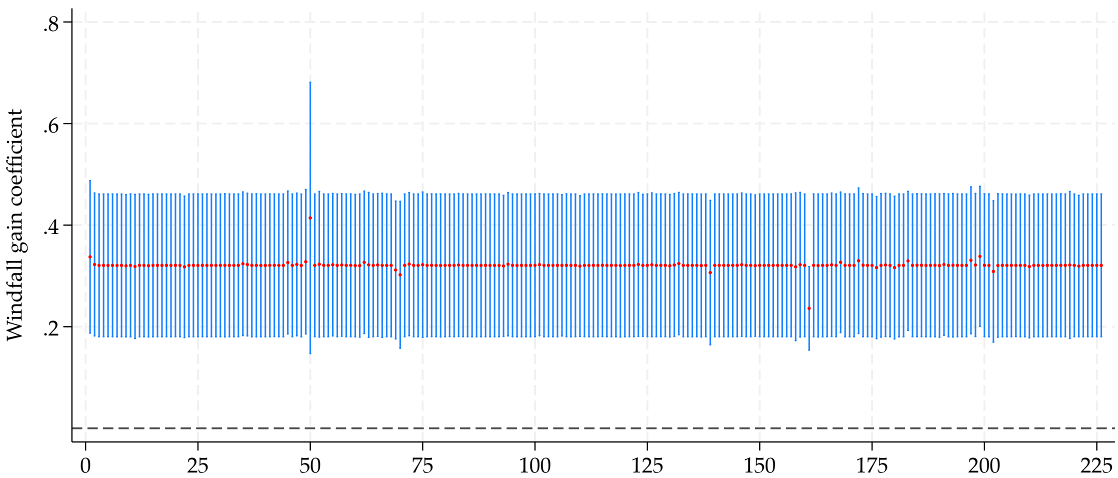
Note: The table shows results of estimating equation (6.6). Standard errors are robust to heteroscedasticity and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure C.1: Placebo tests



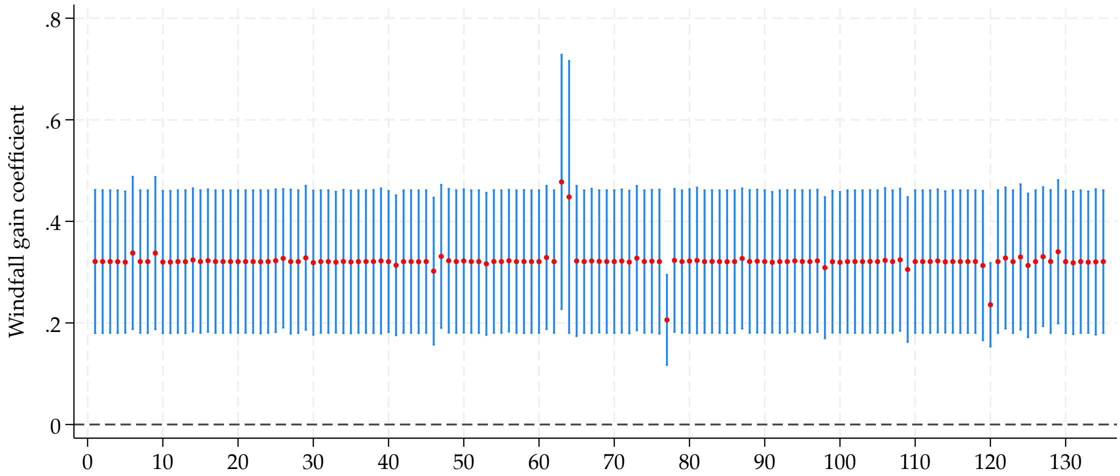
Note: Figure shows distributions of windfall coefficients across two different placebo tests across 1,000 simulations. Share of observations is denoted in percent on y-axis, estimated windfall gain coefficient is shown on x-axis. Left panel shows simulations where windfall gains are randomly shuffled across conflicts. Right panel shows simulations where conflict start years are randomly assigned. Vertical red dashed line marks the coefficient yielded in our baseline estimation in Table 2, Column 4. Baseline coefficient is reached 1 time (0.1%) in left panel and 20 times (2%) in right panel.

Figure C.2: Sensitivity analysis: Dropping dyads



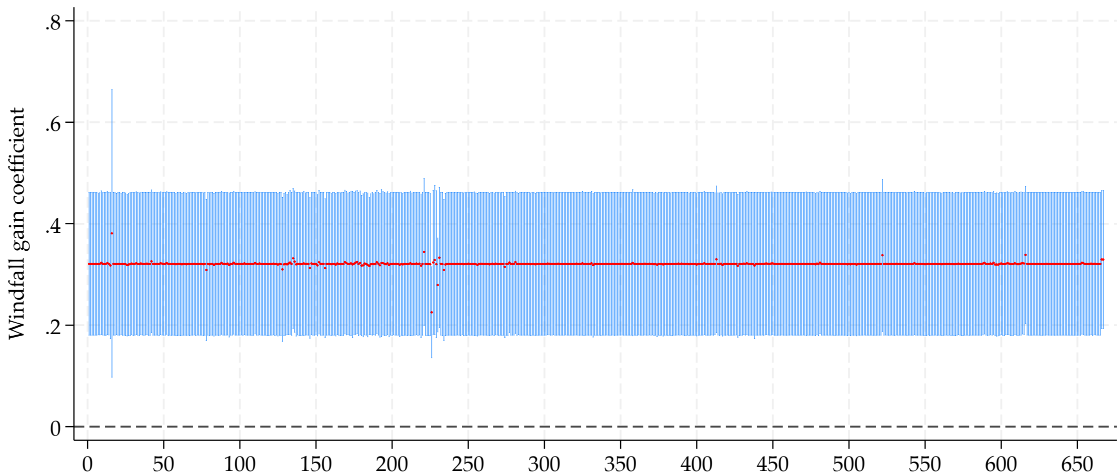
Note: Figure shows how windfall coefficient behaves when dropping entire dyads, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.3: Sensitivity analysis: Dropping entire countries



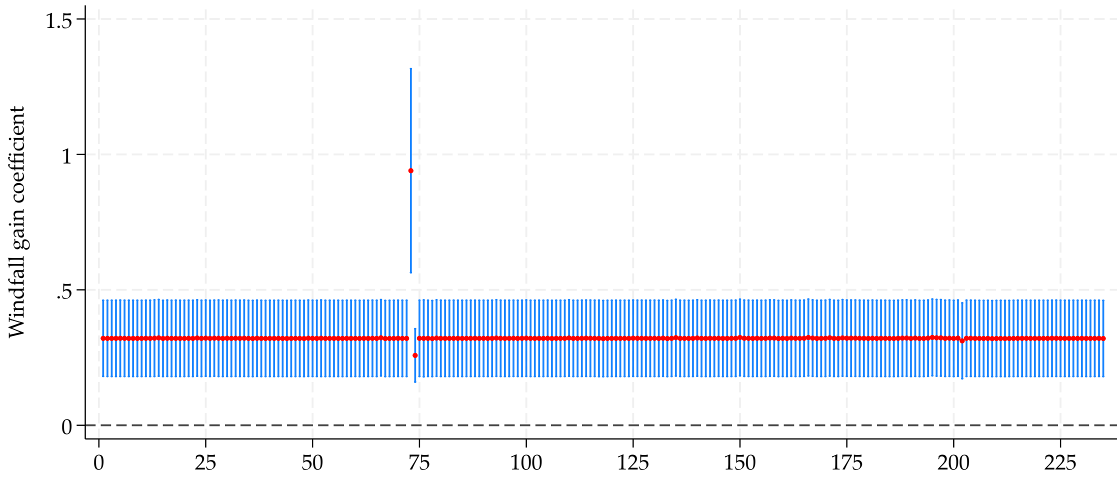
Note: Figure shows how windfall coefficient behaves when dropping entire countries, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.4: Sensitivity analysis: Dropping entire conflicts



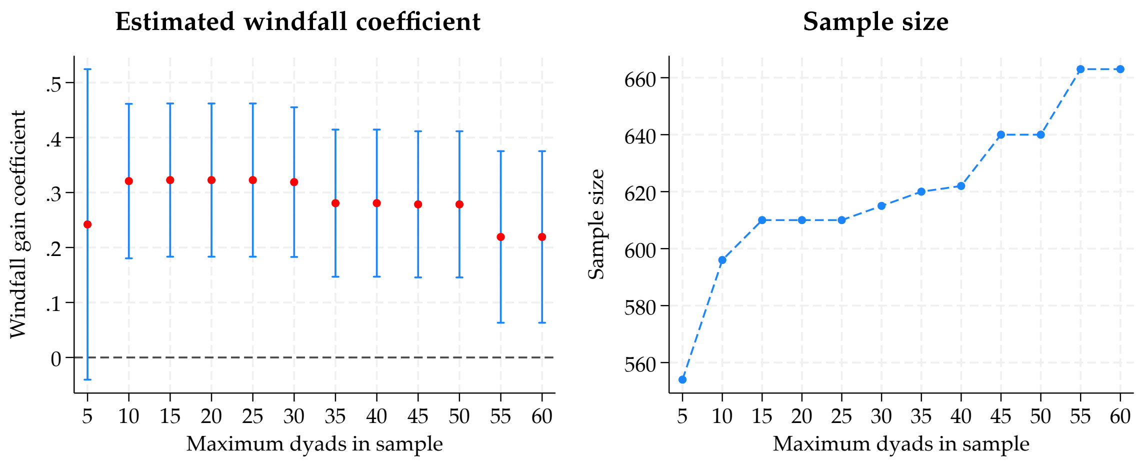
Note: Figure shows how windfall coefficient behaves when dropping entire conflicts, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.5: Sensitivity analysis: Dropping products



Note: Figure shows how windfall coefficient behaves when dropping excluding products, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.6: Coefficients depending on conflict size



Note: Figure shows how sample and results depend on including conflicts with large coalitions. For both panels the x-axis depicts the restriction of the sample depending on the maximum coalition size. Left panel shows estimated windfall coefficient on y-axis. Vertical lines mark 90% confidence bounds. Right panel shows sample size.