Does Money Win Wars?

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Abstract

The impact of economic resources on the outcomes of military conflicts is heatedly debated—yet causal empirical evidence is lacking. To address this gap, the current 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 financial windfalls on winning chances in interstate conflicts. We find a statistically significant and quantitatively large impact of windfalls on winning odds and show that a key channel of transmission is a surge in military spending.

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

1 Introduction

Current debates within NATO center on how military spending translates into improved security outcomes, and how Ukraine's battlefield prospects depend on financial support. This question is by no means new: 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. 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. The two World Wars of the 20th century are viewed as large-scale economic mobilization efforts, with the eventual victories of the Allied powers often attributed, in part, to their greater economic resources.[1] Turning to the present war against Ukraine, Gorodnichenko, Korhonen and Ribakova (2024) and Garicano, Rohner and di Mauro (2022) argued that Russia's war efforts are 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 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 quantitative 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

[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).

paper aims to close.

We study more than 700 militarized interstate disputes from 1977 to 2013 and 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 the structure of their trade. 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 1 percentage point windfall for one conflict party relative to its GDP increases its probability of winning instead of ending up in a draw by 0.4 percentage points.[2] 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, thereby focusing on observations where conflict parties are clearly price takers. 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. Commodity price changes can both benefit and harm countries.

Our proposed channel is fiscal. Commodity windfalls increase government revenues, and a portion of these extra resources is funneled into defense. An instrumental-variable strategy shows that, on average, roughly one-third of the windfall generated during a conflict ends up as government revenue. In the second stage, we find that every 1-percentage-point rise in government revenue raises the probability of winning—rather than settling for a draw—by about 1.1 percentage points. A falsification test shows that this effect is absent in states with low pre-conflict state capacity, further corroborating the idea that commodity windfalls modify war outcomes only if the state is equipped with the necessary institutions to channel them into fiscal revenues. This effect is particularly sizable because, even in wartime, "only" around 20 percent of excess government revenues are converted into defense spending. Assuming windfalls influence outcomes solely through defense spending, a back-of-the-envelope calculation implies that a 1-percentage-point increase in military expenditure relative to GDP lifts the likelihood of victory over stalemate by roughly 5.5 percentage points.

We contribute to several strands of literature. First of all, the current paper is

[2]Throughout this paper, we use the term windfalls for both positive and negative effects of price changes. We find symmetric effects irrespective of whether price changes manifest as exogenous gains or losses.

part of the literature on the economics of conflict (for recent surveys, see Anderton and Brauer (2021), Rohner and Thoenig (2021), and 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 in International Relations—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 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 tend 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 utilized for studying success prospects in international conflicts.

The literature on state capacity and conflict is also relevant for this paper.[3] 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 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 conflicts.[4]

In summary, our contribution is, to the best of our knowledge, the first that is

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

[4]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).

able to exploit exogenous changes in resource revenues to estimate the causal 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 presents an instrumental variable approach, showing that windfalls affect conflict outcomes via changes in government revenues. Section 7 inspects in more detail how windfalls affect government revenues and military spending, while Section 8 concludes. A series of supplementary information and results is relegated to the Appendix.

2 A case study: the Chad-Libya wars

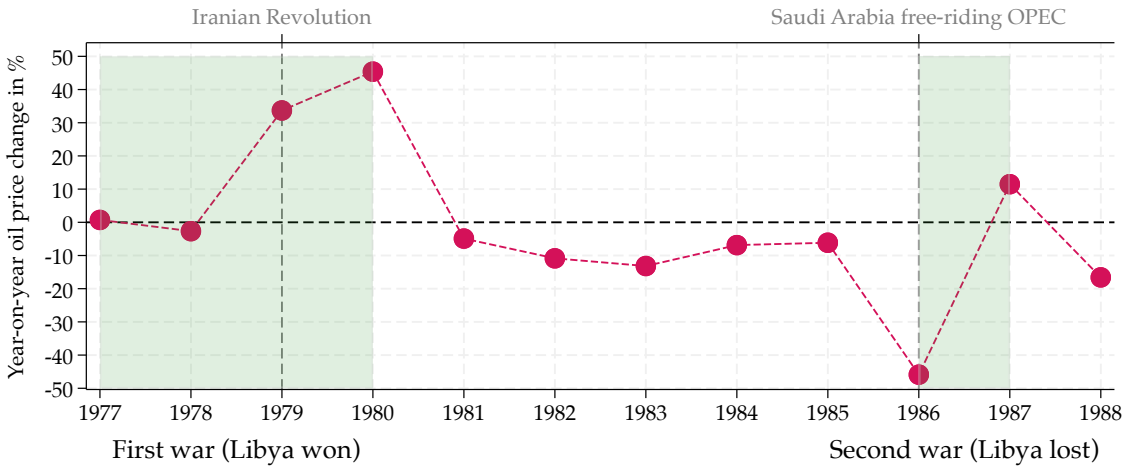
To illustrate the type of mechanism we have in mind, we start by discussing the evolution of battlefield fortunes in the Chadian-Libyan wars against the background of changes in the world price of oil. Militarized tensions between Libya and Chad erupted in the 1970s, around a sovereignty dispute about the Aouzou Strip, located in the extreme north of Chad, bordering Libya.[5] 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

[5]Our discussion of the Chadian-Libyan wars draws among others on Allan (1983); Gross (2019), on the Encyclopedia Britannica entry on the "Aouzou Strip" and 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 Chadian-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.

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". In this period 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.[6]

Obviously, the anecdotal evidence for the Chadian-Libyan conflict amounts to suggestive correlations at best. Still, we believe that this illustrative example

[6]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).

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.

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.[7] 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) and Maoz et al. (2019).[8][9] An MID is characterized as a historical episode of conflict in which military threat, display, or force is employed by one state against another.

Our sample covers 761 such conflicts across 136 countries for the period 1977–2013. Note that the sample period is shaped by the joint coverage of our

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

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

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

datasets on MID and on commodity trade (described below). The number of distinct militarized disputes in our sample is 671 (which is less than 761, as some disputes involve conflicts between more than two countries).

The MID data distinguishes five intensity levels for each bilateral conflict. The most intense level of hostility (5) is reserved for full-blown "wars", which are 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 2.4 percent of conflicts are characterized by the maximum intensity criteria, qualifying them as full-fledged wars. Examples of such episodes include the wars between Iran and Iraq, Eritrea and Ethiopia, as well as India and 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 58.2 percent of MID. An example of such an episode is the conflict between Russia and Georgia between 2007–2010, which is also commonly referred to as a war in public media, albeit not meeting the strict criteria of the Correlates of War project to qualify as such, since it invoked fewer than 1,000 battle-related deaths.

The next lower intensity category (3) corresponds to "display of force", which includes the show of force, alert, nuclear alert, mobilization, and the fortification or violation of borders. This category of intensity corresponds to 35.7 percent of MID.

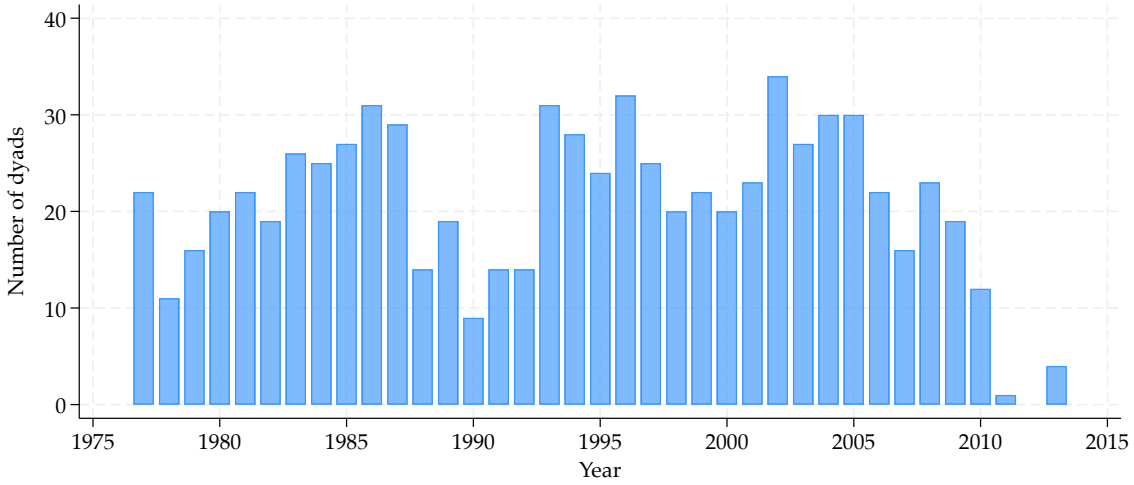
The following category (2) "threat of force" describes events characterized by a threat to use force, blockade, occupy territory, declare war, use CBR weapons, or join an ongoing war, corresponding to 3.7 percent of the MID.

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 the Berlin Wall and at the end of the 1990s before 9/11.[10]

In line with the existing literature (see, e.g., Martin, Mayer and Thoenig (2008), Conconi, Sahuguet and Zanardi (2014), and Caselli, Morelli and Rohner (2015)),

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

Figure 2: Number of dyadic conflicts starting each year



Note: Figure shows number of dyads starting in each year. Low number of conflicts in the 2011–2013 period is due to the exclusion of conflicts that lacked clear outcomes when dataset was assembled by Maoz et al. (2019).

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.8 and C.9).

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.[11]

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

[11]As the expert coding on "yields" is 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.7).

compromise.

The next two categories, "released" and "unclear", refer to situations in which seized material or personnel were 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.[12]

Note that some conflicts in our sample involve large coalitions encompassing a substantial number of countries. For example, the Second Gulf War alone involved 30 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 fewer than ten distinct dyadic conflicts, leading to the exclusion of 124 conflicts. As shown in Appendix Figure C.8, our results are very similar if we broaden or narrow down this restriction.

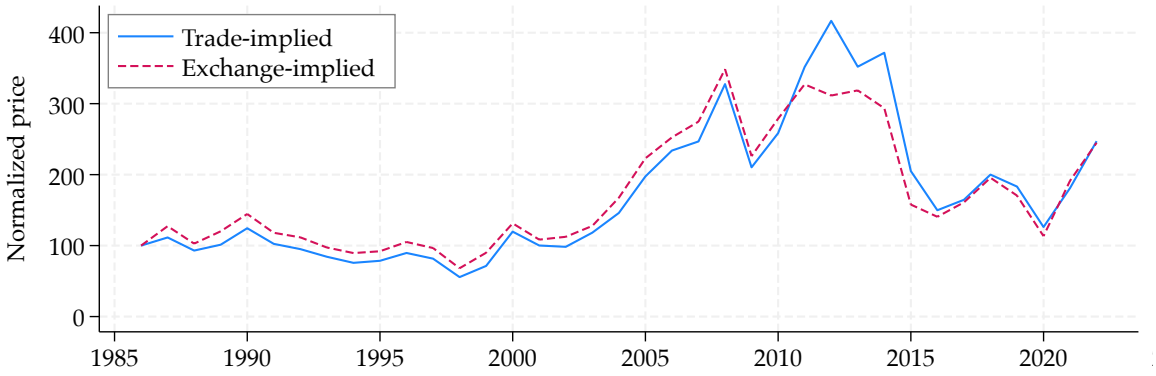
3.3 Data on relative windfall gains

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

[12]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 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 and 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.

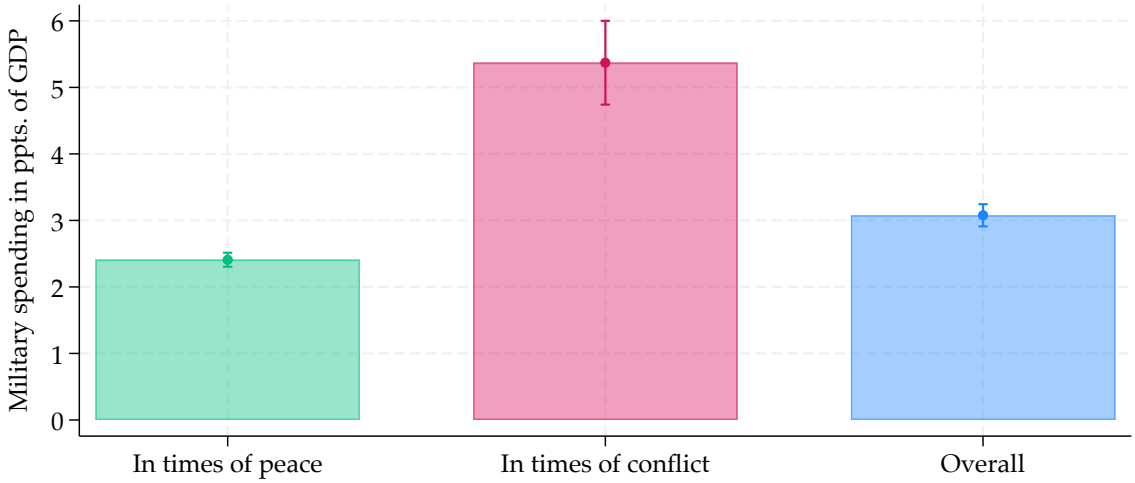
"Petroleum oils, oils from bituminous materials, crude". Strikingly, both price series are almost identical, with the corresponding Pearson correlation exceeding 0.96. This is all the more remarkable, as the trade-implied price series is volume-weighted, whereas the exchange-implied prices are not.

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 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

Figure 4: Military spending in times of peace and conflict



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

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.

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, which is close to the NATO peacetime benchmark. 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

Data on macroeconomic outcomes and controls stem from various sources: Data on external debt, GDP as well as on inflation, used to deflate trade values, are sourced from the World Bank (2024). Government revenues are drawn from the International Monetary Fund (Müller et al., 2025), which we linearly interpolate to fill infrequent gaps in the data. 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 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. We exploit the fact that the impact of global 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 conflicts, we also provide robustness checks where major producers are excluded, such that conflict participants are clearly price takers. 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 conflicts 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 conflict 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 conflict d as

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

where C is the set of all commodities in our sample, $p_{c,t}$ is the global market price of commodity c in period t, and X_{c,t_d-1}^a 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 conflict into the productive capacity of its parties.[13]

The identifying assumption is then restricted to the exogeneity of global changes in commodity prices. Below, we show in robustness checks that our results remain unchanged once we restrict our sample to situations where the countries of a given dyad have only small joint market shares (e.g., below 1 percent) and can be thought of as 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 an equal 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

[13]We note that our baseline is robust to instead computing windfall gains with production levels realized *during* conflicts, see Table C.10. However, as conflicts likely influence production levels, we avoid drawing inferences from these numbers.

countries' pre-conflict GDP, such that

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

Thus, an increase in the windfalls 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.[14]

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:

$$q_{p,t} = \beta\, W_{p,t} + \mu' Controls_{p,t} + \alpha_p + \eta_t + u_{p,t} \tag{4.3}$$

where the outcome variable $q_{p,t} \in \{-1, 0, 1\}$ captures the conflict outcomes (country B winning, draw, or A winning). The main explanatory variable, $W_{p,t}$, denotes the windfall shocks, as detailed above. $Controls_{p,t}$ denotes the set of control variables comprising the log-difference between both parties' pre-conflict population and GDP, respectively. α_p denotes dyad fixed effects filtering out any time-invariant confounders. A further set of time fixed effects, η_t, are included, and filter out aggregate shocks. Note that we shall introduce the various batteries of fixed effects sequentially, which means that the identifying variation will evolve from one regression to another, allowing us to assess the stability of results across various settings. Lastly, $u_{p,t}$ corresponds to the error term. As there is substantial heterogeneity concerning the duration of conflicts, whereas our windfalls are observed annually, we weight the regression to mitigate a potential attenuation bias arising from the varying signal-to-noise ratio of our key explanatory variable.[15] We note, however, that as shown in our robustness exercises below, our results remain unchanged once we turn to an unweighted estimate or subsamples restricted to longer conflicts.

Table 1: Sample of conflicts and windfalls

		Start year		Duration (days)		Windfalls W_d			
	Conflicts	Min	Max	Mean	SD	Mean	SD	Min	Max
Baseline	761	1977	2013	142	358	.009	.166	-1.43	3.213
All disputes	885	1977	2013	140	350	.009	.16	-1.43	3.213

Note: Table presents descriptive statistics for two samples: 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).

4.3 Descriptive summary statistics

Table 1 outlines the summary 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. The baseline subsample contains 761 dyadic conflicts that meet our restriction of at most ten dyads per broader dispute, while the full sample includes 885 conflicts. In both cases, the observation window opens in 1977 and closes in 2013, and is dictated by the joint availability of data on conflicts and commodity trades. On average, the windfall gains are close to zero, but show substantial variation. Conflict episodes are, on average, short-lived: the mean duration in our baseline is 142 days. In our estimations below, we account for this systematically while also providing subsample analyses on conflicts of various durations.

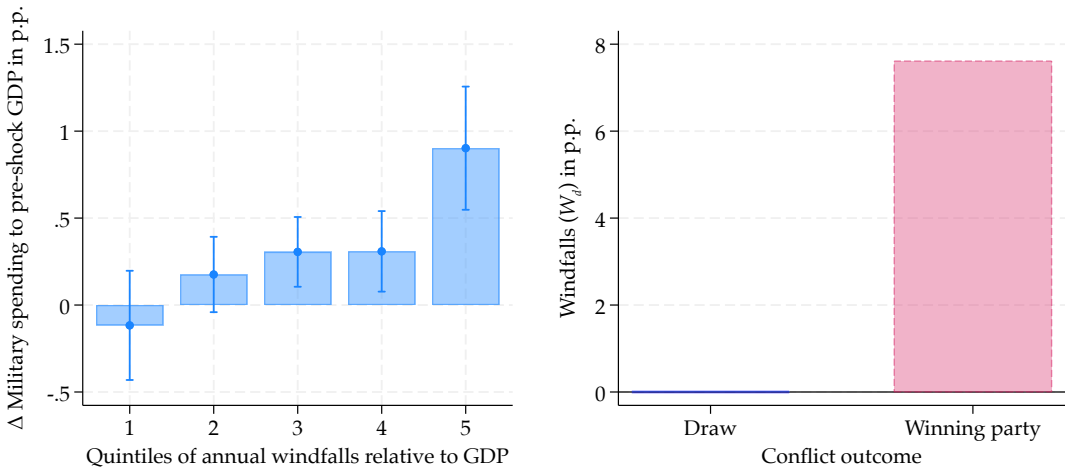
Turning to economic shocks, relative windfall gains cluster around zero. The mean windfall is—by construction—insignificantly different from zero. Windfalls roughly follow a fat-tailed normal distribution, with extremes in some conflicts reaching more than 300 percentage points of pre-conflict GDP.[16] 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

[14]Note that windfalls can be both positive and negative. We do not find statistically significant asymmetries if we allow the coefficients to differ (see Appendix Table C.16).

[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. Thus, 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.

[16]This is particularly the case for protracted conflicts lasting several years, where windfalls may accumulate over time.

Figure 5: Windfalls, military expenditures, and the odds of winning



Note: Left panel shows average three-year change in military expenditure as a share of pre-conflict GDP in percentage points (y-axis) by quintiles of contemporaneous windfalls in percentage points of GDP (x-axis). Right panel shows average relative windfall gains for conflicts that ended in a draw (blue) versus conflicts in which one party prevailed (red).

points of its GDP, resulting in relative windfall gains of -1.429.[17] Notably, due to the presence of these limiting cases, which exceed the samples' standard deviation severalfold, we conduct extensive robustness tests accounting for outliers and dominant market power.

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 starts with some descriptive evidence of our windfall gain variable. The left-hand panel relates the size of a year-on-year windfall of countries in conflict to the subsequent three-year adjustment in military spending, expressed as a share of pre-windfall GDP. A clear monotonic pattern emerges: countries in the bottom quintile of the windfall distribution—where the average shock is -3.7 percentage points of GDP—curtail military spending slightly, whereas those in the top quintile, blessed with an average gain of almost 5 percentage points,

[17]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 not affect estimations nor alter the interpretation.

Table 2: The effects of windfall gains on conflict outcomes

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.351***	0.355***	0.374***	0.411***
	(0.0710)	(0.0632)	(0.0970)	(0.0696)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.122	0.146	0.337	0.482
N dyads (clusters)	238	224	121	121
N	761	707	604	603

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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

expand their budgets by roughly 1 percentage point. The elasticity implied by the slope of the relationship suggests that governments translate unexpected fiscal space into additional military resources.

The right-hand panel turns from inputs to outcomes by comparing average windfalls across conflicts that end in a draw and those that produce a clear winner. 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 symmetry of dyads implies that the average windfalls in conflicts that end up in a draw are zero. 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 half a standard deviation higher than for those who end up in a draw. The windfalls in case of a victory are significantly larger than zero, as implied by a t-test ($p = 0.084$).

Taken together, Figure 5 provides *prima facie* evidence that favorable resource shocks, at least correlationally, feed into higher defense spending and translate into a higher probability of military success. 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 identification strategy sketched above, and estimate specification (4.3).

In Table 2, we now approach causality as we turn to an empirically more rigorous account of the relationship between conflict outcomes and financial windfalls. The table displays the coefficients resulting from estimating Equation (4.3) for a set of increasingly more demanding specifications, including both dyad and conflict start year fixed effects as well as controls comprising log differences in the conflict opponents' pre-conflict population and GDP. 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 1 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 0.35 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 missing observations for some of the control variables lead to a small 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, drops them from the sample, which accounts for the reduction in the number of observations.

We now turn to how a war is settled. We code a discrete settlement outcome, $S_{p,t} \in \{\text{Negotiated, Imposed, None}\}$ according to (Maoz et al., 2019). A *Negotiated* settlement is a mutually accepted termination reached through bargaining or discussion—identified by some agreement (formal or informal) without unconditional surrender and without external imposition. *Imposed* settlements, by contrast, are forced terminations, indicated by unconditional surrender, occupation without withdrawal prior to termination, or evidence that one side was compelled to accept terms. Finally, *None* captures disputes that end without resolving the underlying issues and without any successful negotiated or imposed termination—i.e., no identifiable settlement effort succeeds.[18] To analyze how windfalls relate to these distinct modes of termination, we estimate a multinomial logit model for $\Pr(S_{p,t} = j)$ over the three categories, using $W_{p,t}$ and the baseline controls as covariates.

Table 3 reports average marginal effects. A 10 percentage point windfall increases the probability of a negotiated settlement by about 1.7 percentage

[18] In a subset of cases, termination is coded as unclear/ambiguous in the underlying sources; we drop these observations from the settlement analysis.

Table 3: The effects of windfall gains on how conflict is settled

	Settlement		
	None	Negotiated	Imposed
Windfalls (W_{pt})	-0.225***	0.165***	0.060***
	(0.070)	(0.054)	(0.023)
Pseudo R^2		0.062	
N dyads (clusters)		223	
N		699	

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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

points, and increases the probability of an imposed settlement by about 0.6 percentage points, while reducing the probability of no settlement. This pattern is consistent with wars being initiated under heterogeneous beliefs about the likelihood of victory: windfalls realized during the conflict update participants' expectations relative to pre-war beliefs, making termination more likely—either through bargaining (negotiation) or through a decisive shift in bargaining power (imposition).

In summary, we think of the results in this section as evidence of financial windfalls having a significant positive effect on military success probabilities in conflict. Before we proceed with examining the underlying mechanism, we now proceed to check the robustness of our findings along several 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 scarcity may fuel price spikes. We do not think that this is a major concern in our particular case. Given that major production disruptions mostly happen in full-fledged wars, such biases are unlikely to drive our results (remember that only 2.4 percent of conflicts 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%, or 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 and, in line with our reasoning of the direction of a potential bias arising from market dominance, rather increase the coefficient—if insignificantly so (see Appendix Table C.3).

Who starts war: In principle, countries may start a war based on their expectations regarding future commodity prices. Reassuringly, we do not find evidence for this possibility, as the relative windfalls are insignificantly related to who has started the war. In line with this evidence, we find that our baseline coefficients remain stable and significant upon including a control variable capturing who started the conflict (see Appendix Table C.4).

Placebo tests: 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. The first placebo test randomly shuffles windfall gains across the observations in our baseline sample. In this way, the placebo 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 2.8% out of 1,000 simulations once we randomly shuffle windfall gains. In contrast, the second placebo test 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 is unlikely to be arrived at by pure chance, with our coefficient only reached in 4.4% out of 1,000 simulations.

Outlier analysis: We also conduct a battery of Jackknife tests dropping certain observations by groups. 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.5. Related tests show that our results are robust to leaving out all conflicts starting in a given year, suggesting that our findings are not driven by specific episodes (see Figure C.4 in the Appendix).

Products: In what follows, we examine whether our measured effect is driven by windfall gains stemming from specific products. Much in the spirit of the previous robustness tests, we re-run our empirical analysis but exclude entire product groups at the 1-digit SITC level from the calculation of our windfall gain measurement, again one at a time. Notably, this includes tests excluding all products relating to petroleum oils and related materials as well as both natural and manufactured gas simultaneously. Across all estimations, the coefficient remains positive and significantly different from zero (see Appendix Figure C.6).

Conflict duration: The conflicts in our sample vary in many aspects, duration being one of them. In principle, the windfalls in our sample—which are observed at an annual level—should only have a smaller impact on conflicts with a duration of less than a year. Our baseline specification accommodates this by weighing conflict observations based on their duration relative to the years they span. Nevertheless, we systematically assess the role of conflict duration and find that, in line with our expectations, coefficients tend to rise somewhat as we restrict the sample towards longer war episodes (see Appendix Figure C.7).

Multilateralized conflicts: Recall that in our baseline, we drop conflicts with 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 that involve at most ten individual dyadic disputes. We document, however, that our results are robust to variations in this threshold (see Appendix Figure C.8).

Alternative estimators: We also 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.5). Second, in our baseline, we estimate a weighted regression that

accounts for the varying duration of wars. We show that our results are robust to estimating an unweighted regression instead (see Appendix Table C.6).

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.7 a sensitivity test, where we drop such "yield" outcomes, and focus on clear-cut (military) victories only. Reassuringly, our findings remain similar for this alternative sample.

Standard errors: Note that in our baseline, we cluster standard errors at the dyad level. However, our results are robust to employing a two-way clustering at the dyad and conflict start year level as well as to clustering only at the conflict start year level, at the conflict level, and to computing heteroscedasticity robust standard errors (see Appendix Tables C.12, C.13, C.14, and C.15).

Asymmetric effects: Our measure of windfalls can be positive or negative. We test whether the effects of both types of windfalls are symmetric and do not find statistically significant differences between the coefficients, although the magnitudes of positive windfalls are more precisely estimated (see Appendix Table C.16).

Restricting the sample to higher intensity conflicts: In Appendix Tables C.8 and C.9, we restrict the sample to conflicts 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. The results are—if anything—even stronger for more intense conflicts.

Variations of the windfall measure: To assess how our results depend on choices in the construction of our windfall measure, we employ a set of additional specifications to check its limitations. First, we compute an alternative measure where we do not relate the absolute windfall gains to the opponents' pre-conflict GDP but instead to their pre-conflict population. We arrive at similar findings with per capita windfalls having a strong positive and statistically significant effect on winning conflicts across all specifications (see Table C.11). Another concern is that by relating price changes to pre-conflict net export quantities, our windfall measure captures a counterfactual revenue stream that is not necessarily

realized when war affects the country's productive capacities. To accommodate this argument, we run an alternative specification where we relate price changes to actual net export quantities realized during wars and show that the measure also positively correlates with war outcomes and yields significant coefficients, see depicted in Table C.10. We, however, stress that this measure is most likely confounded and only provides very limited insights, given that productive capacities arguably may be affected by conflicts themselves in a non-trivial way.

6 Mechanisms and channels of transmission

6.1 Instrumental variable approach

To complement our main results and further investigate the mechanism through which resource windfalls may affect conflict outcomes, we implement an instrumental variable strategy. The core idea is to pin down the exogenous variation in government revenue changes that stems from global commodity price shocks. Specifically, we instrument the relative change in government revenues between conflict parties with their respective commodity windfall gains. This enables us to estimate the causal effect of fiscal resources on the probability of victory in a militarized interstate dispute.

For each conflict dyad, we calculate the change in government revenues for each party throughout the conflict, normalized by pre-conflict GDP. That is,

$$\Delta Rev_d = \sum_{i=0}^{l_d-1} \left(\frac{Rev^a_{t_d+i} - Rev^a_{t_d-1}}{GDP^a_{t_d-1}} - \frac{Rev^b_{t_d+i} - Rev^b_{t_d-1}}{GDP^b_{t_d-1}} \right), \tag{6.1}$$

where again l_d is the duration of the conflict in years d, t_d is the conflict start year, $GovRev^a_{t_d+i}$ ($GovRev^b_{t_d+i}$) is the government revenue of party a (b) in the ith year of the conflict, and $GDP^a_{t_d-1}$ ($GDP^b_{t_d-1}$) is the pre-conflict GDP of party a (b).[19]

Thus, our government revenue measure indicates by how much government revenues relative to pre-conflict GDP—aggregated over the entire conflict period—have changed more in conflict party a than in party b. This definition conceptually matches our definition of windfall gains above, which captures—also in terms of the pre-conflict GDP of each party—by how much the cumulative windfalls of party a throughout the conflict exceed those of party b.

[19]We winsorize the government revenue deltas at the 0.5% and 99.5% values throughout the paper as we do for all macroeconomic outcomes, to filter out outliers.

As each dyadic conflict d is uniquely identified by a given opponent pair p and the conflict start year t, our first stage relating government revenues to windfalls is given by the following equation:

$$\Delta Rev_{p,t} = \beta W_{p,t} + \mu' Controls_{p,t} + \alpha_p + \eta_t + u_{p,t}\,, \tag{6.2}$$

where $\Delta Rev_{p,t}$ is the government revenue delta across both parties accumulated during a conflict which takes place between opponent-pair p and starts year t, as defined by Equation (6.1) above and $W_{p,t}$ is the relative windfall measure from our baseline as defined by Equation 4.2. $Controls_{p,t}$ again comprise the log differences between the opponents' pre-conflict population and GDP, α_p denotes opponent-pair fixed effects, η_t conflict start year fixed effect, and $u_{p,t}$ denotes the error term. As before, standard errors are clustered at the opponent-pair level.

The second stage of the instrumental variable setup looks as follows:

$$q_{p,t} = \beta \widehat{\Delta \text{Rev}}_{p,t} + \mu' Controls_{p,t} + \alpha_p + \eta_t + u_{p,t}\,, \tag{6.3}$$

where $q_{p,t}$ again captures the conflict outcome with a value of -1, 0, and 1 signifying that party a either lost, ended up in a draw, or won, respectively. The variable $\widehat{\Delta \text{Rev}}_{p,t}$ denotes the estimated government revenues from the first stage, and the rest of the variables are defined analogously to the first stage.

Table 4 presents the results of the two-stage least squares estimation. Panel A reports the first-stage coefficients. We find that relative windfall gains are strongly and significantly associated with increases in relative government revenues across conflict parties. The coefficients range from 0.31 to 0.38 depending on the specification, indicating that roughly 30–40% of windfall gains (as a share of GDP) are converted into fiscal resources. These estimates are consistent across model variants, including those restricted to higher-intensity disputes. The strength and precision of these results highlight that windfalls translate into fiscal capacity in a meaningful and measurable way.

Panel B presents the second-stage estimates, which link instrumented government revenue changes to conflict outcomes. We find that a one percentage point increase in relative government revenue raises the probability of achieving a more favorable conflict outcome (i.e., moving from loss to draw or from draw to win) by roughly 1.1 percentage points. This effect is statistically significant across all model specifications. The magnitude of the effect suggests that fiscal resources play a substantial role in determining battlefield success.

Panel C reports standard diagnostics for instrument relevance. The Cragg-

Table 4: Two-Stage Least Squares

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage	Government revenue delta ($\Delta Rev_{p,t}$)					
Windfalls ($W_{p,t}$)	0.31***	0.32***	0.33***	0.36***	0.36***	0.38***
	(0.09)	(0.09)	(0.07)	(0.05)	(0.05)	(0.06)
Panel B: Second stage	Conflict outcome ($q_{p,t}$)					
Gov. rev. ($\widehat{\Delta \text{Rev}}_{p,t}$)	1.14**	1.13***	1.11***	1.13***	1.14***	1.12***
	(0.45)	(0.43)	(0.39)	(0.27)	(0.27)	(0.27)
Panel C: IV diagnostics						
Cragg-Donald F-Stat	135.3	135.4	117.7	140.6	136.0	72.5
Kleibergen-Paap F-Stat	13.0	13.2	23.6	53.9	54.1	46.1
Controls		✓	✓	✓	✓	✓
Dyad fixed effects			✓	✓	✓	✓
Year fixed effects				✓	✓	✓
Conflict intensity	> 1	> 1	> 1	> 1	> 2	> 3
Observations	665	634	538	537	520	295

Note: Panel A reports first-stage regressions of relative change in government revenues ($\Delta Rev_{p,t}$) on windfall gains ($W_{p,t}$). Panel B shows second-stage estimates of the effect of instrumented revenue changes on conflict outcomes ($q_{p,t}$). Regressions duration-weighted to account for varying war length. Standard errors clustered at the dyad level are reported in parentheses. Controls comprise log differences of the opponent's GDP and population, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Donald and Kleibergen-Paap F-statistics exceed conventional critical values significantly, alleviating concerns about weak instruments. Across all specifications, the Kleibergen-Paap statistics range from 13 to 54, far above the rule-of-thumb threshold of 10.

Together, the estimates in Table 4 provide evidence for the fiscal transmission mechanism. Windfall gains improve the odds of military success not merely through latent economic power, but through tangible, measurable increases in state revenues that can be directed toward the war effort. As with any instrumental variable strategy, however, the validity of these estimates hinges on the exclusion restriction—namely, that windfalls affect conflict outcomes only via fiscal resources. While this assumption is inherently untestable, in the next section, we provide supportive evidence suggesting that our measured effect is absent in states with low fiscal capacity.

6.2 The role of fiscal capacity

Windfall gains should only translate into usable wartime resources and conflict outcomes if the state manages to funnel them into government revenues. Strong fiscal institutions—broad tax coverage, effective auditing, and functioning treasuries—are thus a natural prerequisite for such a conversion (see also Acemoglu, Johnson and Robinson, 2005). The absence of such institutions should shrink the effects of windfalls, allowing us to have an explicit investigation of the exclusion restriction imposed by the instrumental variable approach we pursue above. For this purpose, we partition our windfall instrument into two components based on fiscal capacity: one capturing windfalls accruing to high-capacity states and one for low-capacity states. We then assess their differential effects within our instrumental variables framework.

For this purpose, we define the dummy variable $F^a_{t_d-1}$ (analogously $F^b_{t_d-1}$) that equals 1 when government revenue-to-GDP for conflict party a (b) in the year preceding the conflict is above the in-sample median (equal to about 21 percentage points). A value of 0, therefore, signals relatively *low* fiscal capacity.

We interact these indicators with our windfall terms to produce two mutually exclusive components of the relative windfall measure,

$$W^{F+}_d = F^a_{t_d-1}\frac{\Delta V^a_d}{GDP^a_{t_d-1}} - F^b_{t_d-1}\frac{\Delta V^b_d}{GDP^b_{t_d-1}}, \tag{6.4}$$

and

$$W^{F-}_d = (1-F^a_{t_d-1})\frac{\Delta V^a_d}{GDP^a_{t_d-1}} - (1-F^b_{t_d-1})\frac{\Delta V^b_d}{GDP^b_{t_d-1}}. \tag{6.5}$$

The term W^{F+}_d isolates relative windfalls received by high-capacity contenders, while W^{F-}_d captures those received by low-capacity states.[20] We expect windfalls to translate into greater military advantage only when the recipient has the institutional capacity to channel private rents into public revenues.

Equipped with these variables, we re-estimate both the first stage of our IV specification and the baseline war outcome regression to test whether fiscal capacity indeed moderates the effects of windfalls on state revenues and conflict outcomes. As an additional control, we include the difference in the opponents' pre-conflict tax capacity, $\Delta F_d = F^a_{t_d-1} - F^b_{t_d-1}$, such that our estimates of windfalls across the two fiscal capacity regimes are not confounded by the potential

[20] In defining $F^a_{t_d-1}$ and $F^b_{t_d-1}$, we extrapolate missing values in government revenues relative to GDP, assuming growth is proportional to world average growth.

Table 5: Windfalls and fiscal capacity

	Government revenues (1)	War outcome (2)
Windfalls, high fiscal capacity ($W_{p,t}^{F+}$)	0.391***	0.418***
	(0.0380)	(0.0780)
Windfalls, low fiscal capacity ($W_{p,t}^{F-}$)	-0.141	0.115
	(0.542)	(0.355)
Controls	✓	✓
Dyad fixed effects	✓	✓
Year fixed effects	✓	✓
Adj. R^2	0.630	0.534
N	537	537

Note: Table shows results of relating government revenues and war outcomes to windfall gains split by fiscal capacity. $W_{p,t}^{F+}$ denotes relative windfalls accruing to high-capacity states, $W_{p,t}^{F-}$ to low-capacity states. Regressions duration-weighted to account for varying war length. Standard errors clustered at the dyad level are reported in parentheses. Controls comprise log differences of the opponent's GDP and population, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

baseline effect that fiscal capacity may have on conflict outcomes. Table 5 presents the results. Column (1) examines how windfalls translate into changes in government revenues. We find a strong and statistically significant effect when windfalls accrue to high-capacity states: a 1 percentage point windfall (as a share of pre-conflict GDP) raises government revenue by approximately 0.39 percentage points. In contrast, we detect no significant fiscal response for low-capacity governments, suggesting that states with low fiscal capacity are unable to funnel windfalls into government revenues.

Column (2) turns to conflict outcomes and yields a parallel result. Windfalls improve the likelihood of military success only when they accrue to high-capacity states. The effect is larger than for our baseline and statistically significant. For low-capacity states, by contrast, we again observe no significant effect. Together, these findings lend support to the revenue channel as a key mechanism: fiscal capacity determines whether exogenous resource shocks translate into usable wartime inputs and ultimately influence battlefield outcomes.

The fiscal capacity split serves both as an investigation of the exclusion restriction and a broader validation of the proposed mechanism. By showing that windfalls only affect conflict outcomes when states possess the institutional ability to convert them into public resources, the exercise reinforces the interpretation of

our instrument and highlights the central role of revenue-generating institutions in mediating the link between resource shocks and wartime performance.

7 Windfalls, revenues, and military spending

7.1 Unit of analysis and definitions

Having established the importance of windfall gains and losses for war outcomes, we now study the transmission of commodity windfalls into conflict outcomes at a more granular level. Data availability forces us to leave the dyadic framework as key variables of interest, such as data on military expenditures, are often not available for both conflict parties simultaneously. We therefore move to a country-year design with a year-on-year windfall measure and slightly modify the windfall measurement as follows to capture year-on-year windfalls:

$$W_{i,t}^{yoy} = \sum_{c \in C} \underbrace{\left(\frac{p_{c,t}}{p_{c,t-1}} - 1 \right)}_{\text{Return on commodity } c} * \underbrace{X_{i,t-1,c}}_{\text{Prior-year net exports of } c \text{ by } i} . \tag{7.1}$$

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 global market price of commodity c in year 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,

$$\varepsilon_{i,t} = \frac{W_{i,t}^{yoy}}{GDP_{i,t-1}} . \tag{7.2}$$

7.2 How windfalls affect revenues and military spending

Equipped with these variables, we proceed to estimate the impact of commodity windfall gains on government revenues, military spending, and external debt. Again, our point of departure is a set of simple ordinary least squares regressions of the following form:

$$x_{i,t} = \beta \varepsilon_{i,t} + \alpha_i + \eta_t + \mu' Controls_{i,t} + u_{i,t} , \tag{7.3}$$

Where $x_{i,t}$ is the two-period change of either military expenditures, government revenues, or external debt, relative to the pre-shock GDP.[21]. On the right-hand side, $\varepsilon_{i,t}$ denotes the year-on-year relative windfalls of country i in year t, the terms α_i and η_t denote country and year fixed-effects, and $u_{i,t}$ is the error term. The set of controls, $Controls_{i,t}$ comprises four lags of the explanatory variables as well as the outcome variable in first differences to account for potential short-term trends. Standard errors are two-way clustered by countries and years.[22]

Lastly, we test whether windfalls have differential effects on the economy depending on whether they are gains or losses. For this purpose, we employ signed versions of the shock terms. More formally, we define a positive windfall shock as

$$\varepsilon_{i,t}^{+} = \begin{cases} \varepsilon_{i,t} & \text{if} \quad \varepsilon_{i,t} > 0 \\ 0 & \text{otherwise} \end{cases}, \tag{7.4}$$

and a negative windfall shock as

$$\varepsilon_{i,t}^{-} = \begin{cases} -\varepsilon_{i,t} & \text{if} \quad \varepsilon_{i,t} < 0 \\ 0 & \text{otherwise} \end{cases}. \tag{7.5}$$

Equipped with these variables, we can estimate a signed version of Equation (7.3), which allows the response of macroeconomic outcomes to vary across positive and negative windfalls:

$$x_{i,t} = \psi^{+}\varepsilon_{i,t}^{+} + \psi^{-}\varepsilon_{i,t}^{-} + \alpha_i + \eta_t + \mu' Controls_{i,t} + u_{i,t}\,, \tag{7.6}$$

All variables are defined analogously to Equation (7.3) above. The newly introduced term ψ^{+} (ψ^{-}), however, yields an estimate of how these variables respond to a positive (negative) windfall shock.

Panel A at the top of Table 6 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.[23] The numbers for government revenues and external debt equal about 30 and 56 percentage points of GDP,

[21]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. Throughout this section, we winsorize all outcome variables at the 0.5% and 99.5% level.

[22]Note that our results are also robust to clustering solely at the year or country level as well as to computing heteroscedacity robust standard errors, see Appendix Tables C.17-C.19.

[23]We drop Liberia and the Marshall Islands from our sample as they exhibit severe outliers in the windfalls, inflating the standard deviation of windfalls in the entire sample by a factor of four, if included.

Table 6: 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	0.270	0.543
Median	0.018	0.238	0.427
SD	0.070	0.143	0.444
Panel B: Unsigned regression results			
Windfall	0.0366***	0.162***	0.0413
	(0.0110)	(0.0422)	(0.0764)
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.273	0.148	0.270
N	3,519	4,328	3,012
Panel C: Signed regression results			
Windfall (pos.)	0.0295**	0.164***	-0.0176
	(0.0140)	(0.0546)	(0.122)
Windfall (neg.)	-0.0361**	-0.201***	-0.130
	(0.0150)	(0.0673)	(0.178)
Difference	0.066***	0.365***	0.112
F-Statistic	9.627	17.092	0.428
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.273	0.152	0.271
N	3,519	4,328	3,012

Note: Table shows descriptive statistics on military spending, government revenues, and external debt—all relative to GDP in Panel A. Responses of these variables to unsigned windfall shocks, as yielded by estimating Equation (7.3) outlined in Panel B. Panel C shows coefficients of signed windfall shocks as yielded by estimating Equation (7.6). Standard errors two-way clustered at country and year level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

respectively.

Panel B in Table 6 shows the results of estimating Regression (7.3) above. The left columns shows that in response to a 1 percentage point windfall relative to GDP, military spending increases by some 0.03 percentage points relative to

pre-windfall GDP. The second column shows that government revenues increase by some 0.16 percentage points relative to pre-windfall GDP in response to a 1 percentage point windfall. Both effects are significant at the 1% level. The effect of increased government revenues is ex ante ambiguous: On the one hand, it may alleviate fiscal pressures, leading to lower debt levels. On the other hand, they may increase debt capacity and change the financing conditions of debt in favor of the beneficiary country, ultimately incentivizing higher debt levels. In our analysis, we do not find a significant effect on debt.

Panel C at the bottom of the table depicts the results of estimating Regression (7.6) above. The three columns now 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.

Again, we find significant associations between the windfall gains and military spending as well as government revenues. For both variables, we arrive at symmetric responses, suggesting similar effects (in absolute terms) of both positive and negative windfalls on the macroeconomic outcomes.

We also observe that for both military spending and government revenues, the differences between positive and negative windfall gains are statistically significant and point towards the expected direction. In summary, we think of Table 6 as evidence of commodity windfall gains increasing government revenues. Thereby, they contribute to the state's ability to sustain military expenditures.

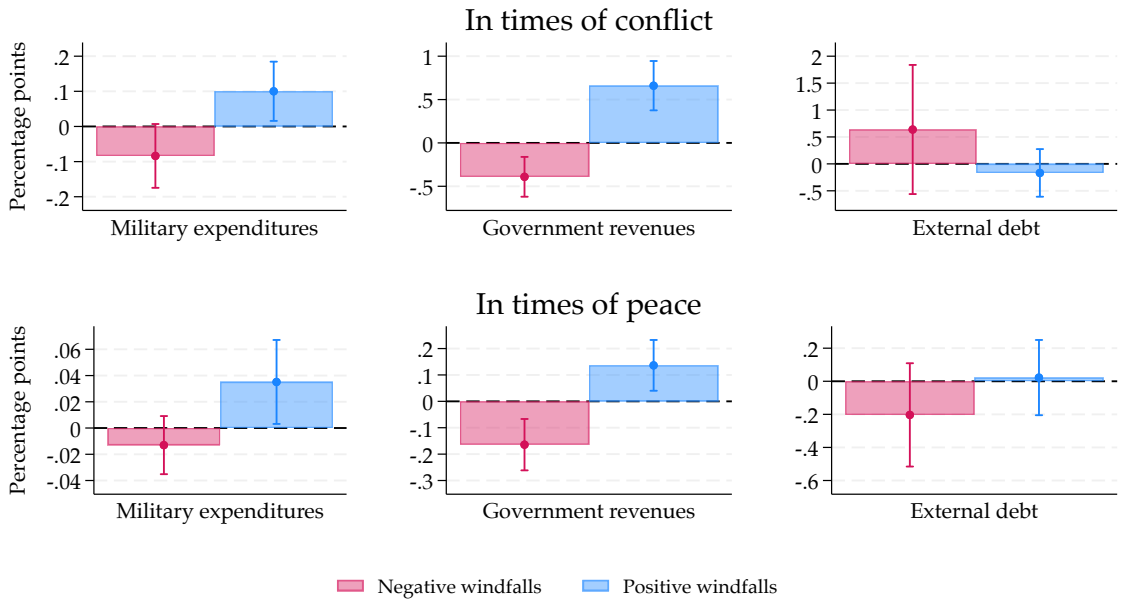
7.3 Windfall gains in times of conflict

We now go one step further and condition the response to windfalls on the presence of conflict (i.e. we focus on exactly the same set of militarized interstate disputes (MIDs), as in the baseline analysis). To this end, we estimate the following linear model,

$$\begin{aligned} x_{i,t} = \alpha_i + \eta_t + \beta conflict_{i,t} + \psi_c^+ conflict_{i,t}\varepsilon_{i,t}^+ + \psi_c^- conflict_{i,t}\varepsilon_{i,t}^- \\ +\psi_p^+(1-conflict_{i,t})\varepsilon_{i,t}^+ + \psi_p^-(1-conflict_{i,t})\varepsilon_{i,t}^- + \mu' Controls_{i,t} + u_{i,t}\,, \end{aligned} \tag{7.7}$$

where again, $x_{i,t}$ is the two-period change of either military expenditures, government revenues, or external debt, relative to the pre-shock GDP of country i in year t, α_i and η_t are country and time fixed effects, $u_{i,t}$ is the error term, $\varepsilon_{i,t}^+$ ($\varepsilon_{i,t}^-$) denote the positive (negative) year-on-year windfalls, and—newly introduced—

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



Note: 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 peace and conflict, as yielded by estimating Equation (7.7). Red bars indicate responses to negative windfalls, blue bars indicate responses to positive windfalls. Vertical lines signify 90% confidence bands. Standard errors two-way clustered at country and year level. More detailed results provided in Appendix Tables C.20–C.23.

$conflict_{i,t}$ is a dummy indicating whether country i is in a conflict in year t. Controls, as before, comprise four lags of the independent variables as well as four lags of the first difference in the macroeconomic outcomes to account for short-term trends.

In this way, ψ_c^+ (ψ_c^-) measure the effect of positive (negative) one percentage point windfall relative to GDP on the macroeconomic change in military spending, government revenues, and external debt, provided that the country is in conflict. In turn, ψ_p^+ (ψ_p^-) measure the effects in times of peace.

Figure 6 visualizes the estimated coefficients for the effects in times of conflict and peace. 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. The top panel of the figure shows the responses to windfalls incurred in times of conflict whereas the bottom panel shows the response in times of peace. Turning to the responses in conflict times, we see that military expenditures react significantly to both positive and negative windfall shocks. On average, a windfall of 1 percentage point relative to GDP

increases military spending by 0.1 percentage points of GDP. Again, the effect is symmetric, with a negative windfall leading to a similar effect into the other direction. We observe the same pattern when we turn to government revenues, where a 1 percentage point of GDP translates into increased revenues of about 0.5 percentage points of GDP, again an effect which his highly symmetric across positive and negative windfalls. For debt we still do not see significant effects. At most, we find suggestive evidence of countries compensating negative revenue surprises with an additional debt take on.

Turning to times of peace, we find similar, but substantially smaller, effects across the board. In fact, in times of conflict, the sensitivity of military expenditures to windfall gains is more than two times higher than in times of peace. This suggests that in times of peace, governments funnel fewer windfall gains into government revenues and military expenditures. This observation is well-positioned in a broad strand of literature suggesting that interstate wars may under some conditions trigger an increase in state capacity; see also Gennaioli and Voth (2015).

7.4 Windfall gains and military technology

In principle, our mechanism implies two margins of adjustment. First, scale: windfalls relax fiscal constraints and expand the overall level of defense budgets. Second, scope: with greater resources, militaries can purchase a broader portfolio of platforms and weapon technologies.[24] We test for this channel using data on military technological capacity from Hariri and Wingender (2025), who assemble historical information on the presence of 29 distinct weapons and platform technologies across national militaries.[25]

Our variable of interest, the number of distinct military technologies available to a country in a given year, includes all technologies currently in use, plus any earlier generations that have been superseded by a technology still in service. Thus, if a country moves from only owning fighter jets of the fourth generation to owning fighter jets of the fifth generation, this increments their technology count by one. This captures the accumulated breadth of military know-how that remains available to the military, even when older systems are no longer front-line equipment. For reference, in 2023, only six countries possessed the full set of 29 technologies: China, Israel, the Netherlands, Russia, the United

[24]See also Antolin-Diaz and Surico (2025) for a discussion on the effects of military spending on technological advancements.

[25]We provide a detailed summary on all technologies covered in Appendix Table B.2.

Kingdom, and the United States. At the other extreme, the worst-equipped country in our sample is East Timor, which had adopted 11 of the technologies.

Relative to the fiscal and conflict outcomes analyzed in the previous section, changes in a country's technology portfolio are likely to unfold more slowly: acquiring, integrating, and training on new platforms takes time. To uncover the medium-run effects of exogenous fiscal windfalls on technology adoption, we estimate the dynamic responses using a set of local projections in the spirit of Jordà (2005):

$$ArmsTech_{i,t+h} - ArmsTech_{i,t-1} = \alpha_{i,h} + \eta_{t,h} + \beta_h \epsilon_{i,t} + \mu_h' Controls_{i,t} + u_{i,t+h} \quad (7.8)$$

where $ArmsTech_{i,t}$ is the count of military technologies available to country i in year t, $\alpha_{i,h}$ and $\eta_{t,h}$ are country and year fixed effects at projection horizon h, $\epsilon_{i,t}$ is the annual windfall relative to GDP (see Eq. (7.2)), and $Controls_{i,t}$ includes four lags of both the dependent and independent variables. The error term is $u_{i,t+h}$. We relate the outcome variable at each projection horizon h to the pre-conflict value to allow for potential permanent effects that windfalls may have on military technology (Stock and Watson, 2018). We compute standard errors that are robust to heteroskedasticity as well as serial and cross-sectional correlation (Driscoll and Kraay, 1998).

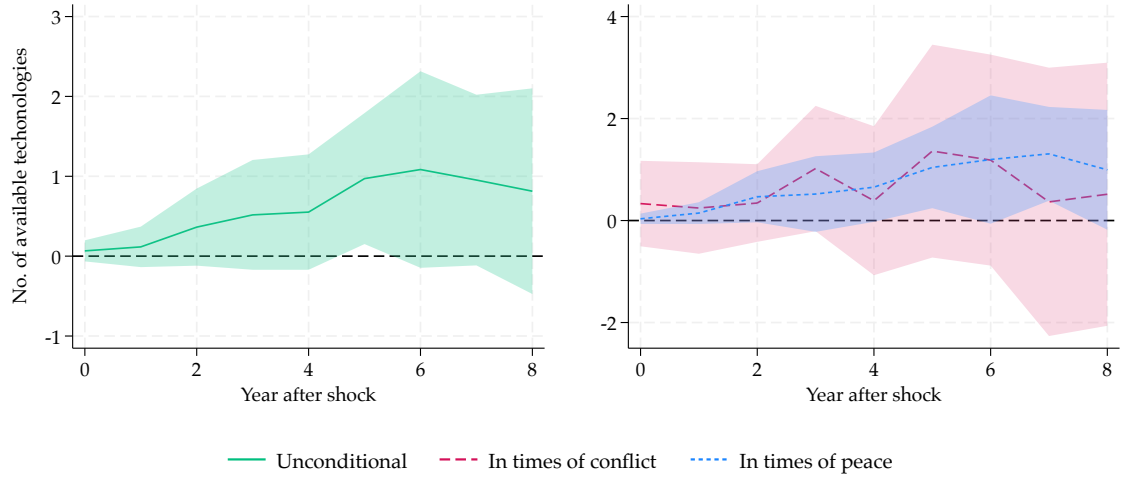
In a similar way to above, we further estimate conditional responses to elicit whether windfalls have different effects on military innovation depending on whether they occur in times of conflict or peace. For this purpose, we estimate the following set of conditional local projections

$$\begin{aligned} ArmsTech_{i,t+h} - ArmsTech_{i,t-1} = \alpha_{i,h} + \eta_{t,h} + \\ \psi_h conflict_{i,t} \varepsilon_{i,t} + \phi_h (1 - conflict_{i,t}) \varepsilon_{i,t} + \mu_h' Controls_{i,t} + u_{i,t+h}\,, \end{aligned} \quad (7.9)$$

that now includes a dummy $Conflict_{i,t}$ which indicates whether country i is in conflict in year t. This allows for differential responses of military technology advances to windfalls depending on whether countries are at conflict at the time of the windfall (ψ_h) or not (ϕ_h). Here, the controls also comprise the conflict dummy as well as four lags of it, such that our measure of effects of windfalls in conflict times is not confounded by a potential effect of conflict itself on military technology.

Figure 7 reports the estimated impulse responses. The left panel shows that, following an exogenous fiscal windfall, the number of adopted technologies

Figure 7: How military technologies respond to a 1 ppt windfall relative to GDP



Note: Left panel shows the dynamic response of number of adopted military technologies (y-axis) to 1 percentage point windfall relative to GDP within 8 years after the windfall (x-axis); see Equation (7.8). Right panel allows response to vary between times of conflict (dashed line) and times of peace (dotted line).

increases gradually. The green area captures the 90th percentile confidence intervals. Quantitatively, a windfall of 1 percentage point of GDP is associated with roughly one additional technology in service after about five years. The right panel allows the response to differ between years in which the country is engaged in conflict and years of peace. We find a statistically significant positive response in peace years—again on the order of one new technology after roughly five years (the blue area captures the 90th percentile confidence intervals). In conflict years, the point estimates are similar, but the 90th percentile confidence intervals (in red) widen substantially, reflecting the smaller number of conflict observations and the presumably greater variance in acquisition patterns during wartime. Taken together, the two sets of responses suggest that the effect of fiscal windfalls on military innovation does not meaningfully differ between peace and conflict. This is in line with the notion that military innovation is a medium-run process that may take place during both times of peace and conflict.

8 Conclusion

The outcomes of military conflicts between competing neighboring states or superpowers have shaped the history of the world. It is not certain that 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 do not exist so far any causal estimates of the impact of exogenous financial shocks on military winning odds. The purpose of the current paper is 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 detect a statistically significant and quantitatively large impact of windfall gains on winning odds. In particular, we find that quantitatively a 1 percentage point increase in government revenues increases the probability of ending up in a draw instead of losing or winning instead of ending up in a draw by about 1.1 percentage points. The 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 Does Money Win 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 gains 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).[26] 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 soldiers who otherwise could carry out productive work at salaries w_i.

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

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

[26] 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:

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

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

This leads to the following equilibrium winning odds:

$$p_A \equiv \frac{\phi_A w_B}{\phi_A w_B + \phi_B w_A}. \tag{A.6}$$

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: *A 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: In equation (A.6), 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	Müller et al. (2025)	
Population	Bolt and Luiten van Zanden (2020)	Accessed on March 28, 2024.

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

Table B.2: Overview over military technologies

Small arms	**Machine guns**	**Artillery**	**Tanks**
Flintlock musket	Hand-cranked	Field guns	Early tank
Percussion lock musket	Automatic	Rifled artillery	WWII tank
Minié bullet rifle		Steel tubes	1^{st} gen. tank
Breechloading rifle		Practical breechloading	2^{nd} gen. tank
Tubular magazine rifle		Recoil mechanism	3^{rd} gen. tank
Box magazine rifle			
Assault rifle			
Fighter aircraft	**Combat helicopters**	**Armed UAVs**	
Early aircraft	1^{st} gen. helicopter	Armed UAV	
WWII era fighter	2^{nd} gen. helicopter		
1^{st} gen. jet fighters			
2^{nd} gen. jet fighters			
3^{rd} gen. jet fighters			
4^{th} gen. jet fighters			
5^{th} gen. jet fighters			

Note: Table shows different military technologies tracked. Source: Hariri and Wingender (2025)

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.3: Excluding products where parties have market power

	Conflict outcome (q_{pt})		
	Restricting joint net export share to be...		
	$< 10\%$	$< 5\%$	$< 1\%$
Windfalls (W_{pt})	0.599***	0.555***	0.469**
	(0.143)	(0.185)	(0.195)
Controls	✓	✓	✓
Dyad fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.444	0.394	0.366
N dyads (clusters)	121	121	121
N	603	603	603

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%, or 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.4: Controlling for who started the war

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.344***	0.344***	0.383***	0.416***
	(0.0711)	(0.0635)	(0.0943)	(0.0687)
Side A started (dummy)	0.0481	0.0694	-0.0762	-0.0528
	(0.0428)	(0.0504)	(0.110)	(0.0742)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.126	0.155	0.341	0.483
N dyads (clusters)	238	224	121	121
N	761	707	604	603

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.5: Ordered logit

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	1.019**	1.019***	1.048**	1.058***
	(2.50)	(2.94)	(2.47)	(3.20)
Controls		✓	✓	✓
Dyad dummies			✓	✓
Year dummies				✓
Pseudo R^2	0.018	0.031	0.380	0.465
N dyads (clusters)	238	224	121	121
N	761	707	604	603

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 z-statistics reported in parentheses. 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: Unweighted estimates

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.242***	0.251***	0.290***	0.321***
	(0.0800)	(0.0759)	(0.0814)	(0.0833)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.017	0.024	0.083	0.080
N dyads (clusters)	238	224	121	121
N	761	707	604	603

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: Exclude dyads with outcome "yield"

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.361***	0.368***	0.385***	0.403***
	(0.0707)	(0.0633)	(0.0954)	(0.0672)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.172	0.210	0.504	0.659
N dyads (clusters)	237	223	118	118
N	729	675	570	569

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.8: Restricting to minimum hostility level: 3

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.351***	0.355***	0.374***	0.412***
	(0.0710)	(0.0631)	(0.0970)	(0.0700)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.122	0.146	0.335	0.480
N dyads (clusters)	230	216	118	118
N	733	679	581	580

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.9: Restricting to minimum hostility level: 4

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.364***	0.368***	0.399***	0.442***
	(0.0749)	(0.0652)	(0.109)	(0.0855)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.142	0.174	0.310	0.548
N dyads (clusters)	176	163	77	76
N	461	423	337	334

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.10: Windfall gains and conflict outcomes – Contemporaneous instead of pre-conflict production levels

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.165*	0.172*	0.178*	0.195**
	(0.0994)	(0.101)	(0.0989)	(0.0825)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.053	0.075	0.233	0.400
N dyads (clusters)	238	224	121	121
N	761	707	604	603

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.11: Windfall gains and conflict outcomes – windfalls per capita

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (10k USD) per capita	0.368***	0.357***	0.420***	0.412***
	(0.0429)	(0.0482)	(0.0588)	(0.0566)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.113	0.116	0.338	0.449
N dyads (clusters)	224	224	121	121
N	707	707	604	603

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.12: Windfall gains and conflict outcomes – Standard errors clustered at start year level

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.351***	0.355***	0.374***	0.411***
	(0.0604)	(0.0543)	(0.0864)	(0.0787)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.122	0.146	0.337	0.482
N dyads (clusters)	36	36	36	35
N	761	707	604	603

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.13: 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})	0.351***	0.355***	0.374***	0.411***
	(0.0686)	(0.0614)	(0.0984)	(0.0800)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.122	0.146	0.337	0.482
N dyads (clusters)			36	35
N	761	707	604	603

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.14: Windfall gains and conflict outcomes – Standard errors clustered at dispute level

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.351***	0.355***	0.374***	0.411***
	(0.0782)	(0.0691)	(0.0958)	(0.0671)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.122	0.146	0.337	0.483
N dyads (clusters)	671	619	547	546
N	761	707	604	603

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.15: Windfall gains and conflict outcomes – robust standard errors

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt})	0.351***	0.355***	0.374***	0.411***
	(0.0629)	(0.0563)	(0.0855)	(0.0623)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.122	0.146	0.337	0.483
N dyads (clusters)				
N	761	707	604	603

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.16: Signed windfalls and conflict outcomes

	Conflict outcome (q_{pt})			
	(1)	(2)	(3)	(4)
Windfalls (W_{pt}^{+})	0.376***	0.375***	0.342***	0.381***
	(0.0596)	(0.0473)	(0.0541)	(0.0368)
Windfalls (W_{pt}^{-})	0.0697	0.135	0.741	0.700
	(0.388)	(0.445)	(0.610)	(0.530)
Windfalls (W_{pt}^{+}) - Windfalls (W_{pt}^{-})	0.306	0.240	-0.399	-0.318
	(0.362)	(0.429)	(0.582)	(0.528)
Controls		✓	✓	✓
Dyad fixed effects			✓	✓
Year fixed effects				✓
Adj. R^2	0.126	0.148	0.341	0.484
N dyads (clusters)	238	224	121	121
N	761	707	604	603

Note: The table shows results of estimating Regression (4.3) with windfalls allowed to vary depending on sign ($W_d^{+} = \frac{max(\Delta V_d^a,0)}{GDP_{t_d-1}^a} - \frac{max(\Delta V_d^b,0)}{GDP_{t_d-1}^b}$ and $W_d^{-} = \frac{min(\Delta V_d^a,0)}{GDP_{t_d-1}^a} - \frac{min(\Delta V_d^b,0)}{GDP_{t_d-1}^b}$) with conflict d being implicity defined by opponent pair p and conflict start year t. Standard errors are clustered at the 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.17: Unconditional effects of windfall gains – standard errors clustered at country level

	Military expenses / GDP	Government revenue / GDP	External debt / GDP
Panel A: Descriptive statistics			
Mean	0.031	0.270	0.543
Median	0.018	0.238	0.427
SD	0.070	0.143	0.444
Panel B: Unsigned regression results			
Windfall	0.0366***	0.162***	0.0413
	(0.00990)	(0.0226)	(0.0774)
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.273	0.148	0.270
N	3,519	4,328	3,012
Panel C: Signed regression results			
Windfall (pos.)	0.0295*	0.164***	-0.0176
	(0.0161)	(0.0452)	(0.121)
Windfall (neg.)	-0.0361***	-0.201***	-0.130
	(0.0118)	(0.0628)	(0.168)
Difference	0.066***	0.365***	0.112
F-Statistic	11.403	51.940	0.483
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.273	0.152	0.271
N	3,519	4,328	3,012

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

Table C.18: Unconditional effects of windfall gains – standard errors clustered at year level

	Military expenses / GDP	Government revenue / GDP	External debt / GDP
Panel A: Descriptive statistics			
Mean	0.031	0.270	0.543
Median	0.018	0.238	0.427
SD	0.070	0.143	0.444
Panel B: Unsigned regression results			
Windfall	0.0366***	0.162***	0.0413
	(0.00962)	(0.0475)	(0.0752)
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.273	0.148	0.270
N	3,519	4,328	3,012
Panel C: Signed regression results			
Windfall (pos.)	0.0295*	0.164**	-0.0176
	(0.0154)	(0.0654)	(0.112)
Windfall (neg.)	-0.0361**	-0.201***	-0.130
	(0.0172)	(0.0635)	(0.173)
Difference	0.066***	0.365***	0.112
F-Statistic	12.014	14.150	0.449
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.273	0.152	0.271
N	3,519	4,328	3,012

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

Table C.19: Unconditional effects of windfall gains – robust standard errors

	Military expenses / GDP	Government revenue / GDP	External debt / GDP
Panel A: Descriptive statistics			
Mean	0.031	0.270	0.543
Median	0.018	0.238	0.427
SD	0.070	0.143	0.444
Panel B: Unsigned regression results			
Windfall	0.0366***	0.162***	0.0413
	(0.00851)	(0.0306)	(0.0865)
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.273	0.148	0.271
N	3,519	4,328	3,012
Panel C: Signed regression results			
Windfall (pos.)	0.0295*	0.164***	-0.0176
	(0.0177)	(0.0575)	(0.116)
Windfall (neg.)	-0.0361**	-0.201***	-0.130
	(0.0151)	(0.0597)	(0.168)
Difference	0.066***	0.365***	0.112
F-Statistic	14.189	33.018	0.434
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.273	0.152	0.271
N	3,519	4,328	3,012

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

Table C.20: Spending impact of windfall changes in times of conflict and peace

	$\frac{\text{Military expenses}}{\text{GDP}}$	$\frac{\text{Government revenue}}{\text{GDP}}$	$\frac{\text{External debt}}{\text{GDP}}$
Windfall (+), conflict	0.143	1.010***	-0.398
	(0.0859)	(0.262)	(0.436)
Windfall (-), conflict	-0.108*	-0.472*	0.708
	(0.0600)	(0.235)	(1.000)
Windfall (+), peace	0.0250	0.196**	-0.0322
	(0.0163)	(0.0793)	(0.191)
Windfall (-), peace	-0.0189	-0.159**	-0.354
	(0.0118)	(0.0739)	(0.232)
Conflict	-0.000796	-0.000397	0.00239
	(0.000928)	(0.00370)	(0.00776)
Controls	✓	✓	✓
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.208	0.157	0.289
N	3,519	4,328	3,012

Note: The table shows results of estimating equation (7.7). 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.21: Spending impact of windfall changes in times of conflict and peace – Standard errors clustered at country level

	$\frac{\text{Military expenses}}{\text{GDP}}$	$\frac{\text{Government revenue}}{\text{GDP}}$	$\frac{\text{External debt}}{\text{GDP}}$
Windfall (+), conflict	0.143**	1.010***	-0.398
	(0.0719)	(0.265)	(0.310)
Windfall (-), conflict	-0.108*	-0.472**	0.708
	(0.0614)	(0.207)	(0.733)
Windfall (+), peace	0.0250	0.196***	-0.0322
	(0.0164)	(0.0649)	(0.196)
Windfall (-), peace	-0.0189*	-0.159*	-0.354
	(0.00971)	(0.0812)	(0.241)
Conflict	-0.000796	-0.000397	0.00239
	(0.000779)	(0.00328)	(0.00676)
Controls	✓	✓	✓
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.208	0.157	0.289
N	3,519	4,328	3,012

Note: The table shows results of estimating equation (7.7). Standard errors clustered at country level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.22: Spending impact of windfall changes in times of conflict and peace – Standard errors clustered at year level

	$\frac{\text{Military expenses}}{\text{GDP}}$	$\frac{\text{Government revenue}}{\text{GDP}}$	$\frac{\text{External debt}}{\text{GDP}}$
Windfall (+), conflict	0.143	1.010***	-0.398
	(0.0874)	(0.207)	(0.463)
Windfall (-), conflict	-0.108*	-0.472*	0.708
	(0.0630)	(0.259)	(0.920)
Windfall (+), peace	0.0250	0.196**	-0.0322
	(0.0150)	(0.0838)	(0.158)
Windfall (-), peace	-0.0189	-0.159**	-0.354*
	(0.0130)	(0.0653)	(0.190)
Conflict	-0.000796	-0.000397	0.00239
	(0.000825)	(0.00311)	(0.00714)
Controls	✓	✓	✓
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.208	0.157	0.289
N	3,519	4,328	3,012

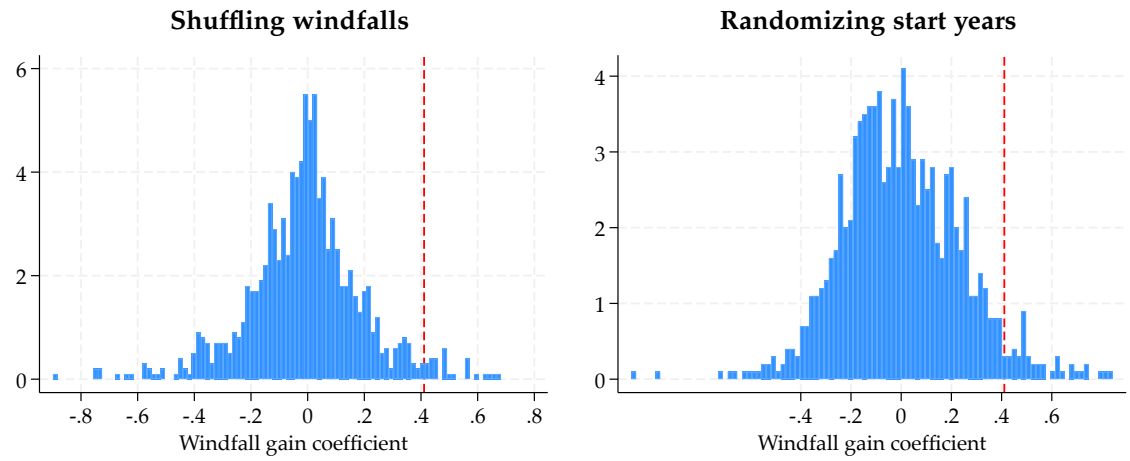
Note: The table shows results of estimating equation (7.7). Standard errors clustered at year level and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.23: Spending impact of windfall changes in times of conflict and peace – Robust standard errors

	$\frac{\text{Military expenses}}{\text{GDP}}$	$\frac{\text{Government revenue}}{\text{GDP}}$	$\frac{\text{External debt}}{\text{GDP}}$
Windfall (+), conflict	0.143*	1.010***	-0.398
	(0.0775)	(0.269)	(0.384)
Windfall (-), conflict	-0.108	-0.472*	0.708
	(0.0688)	(0.246)	(0.614)
Windfall (+), peace	0.0250	0.196***	-0.0322
	(0.0168)	(0.0710)	(0.177)
Windfall (-), peace	-0.0189	-0.159**	-0.354
	(0.0126)	(0.0763)	(0.227)
Conflict	-0.000796	-0.000397	0.00239
	(0.000705)	(0.00278)	(0.00611)
Controls	✓	✓	✓
Country fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Adj. R^2	0.208	0.157	0.289
N	3,519	4,328	3,012

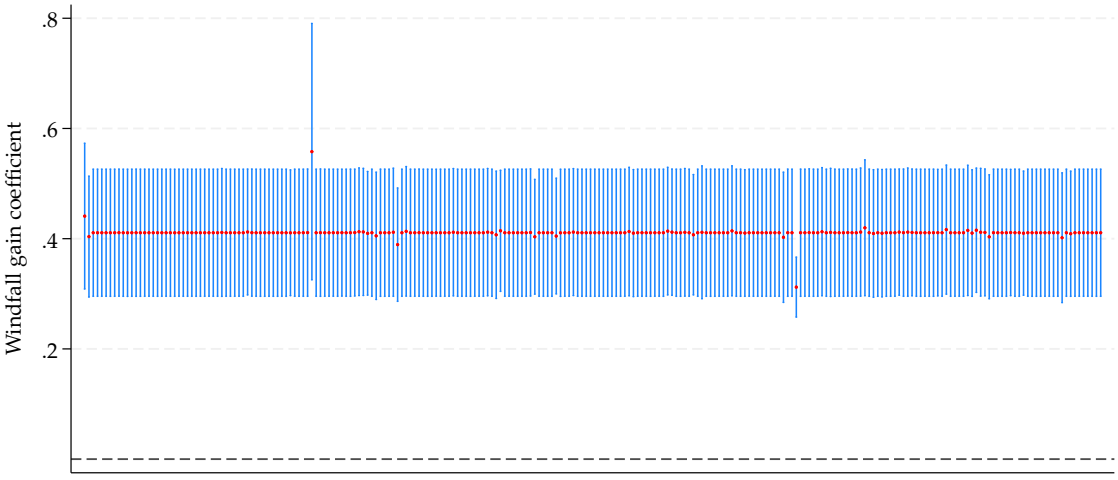
Note: The table shows results of estimating equation (7.7). Standard errors are robust to heteroscedasticity and reported in round brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure C.1: Placebo tests



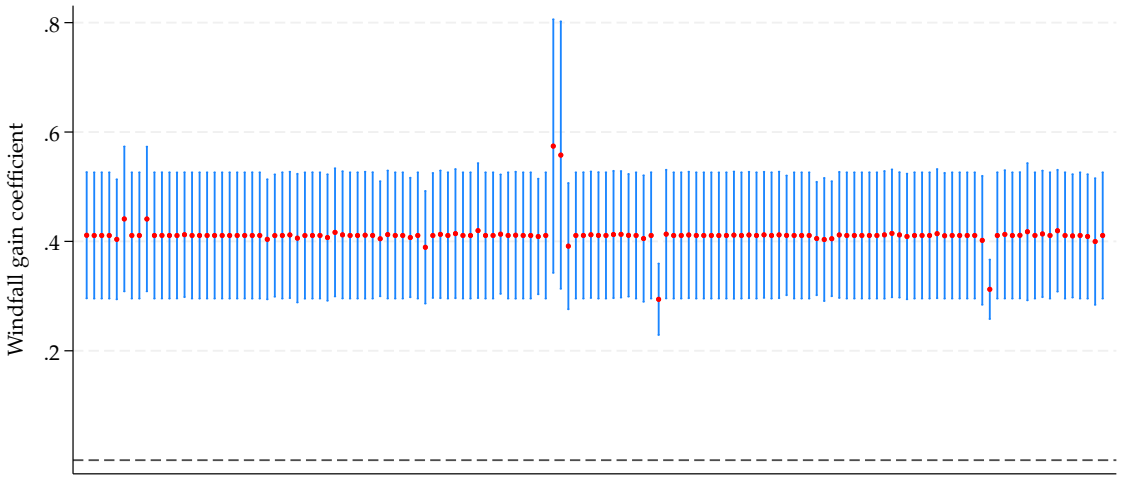
Note: Figure shows distributions of windfall coefficients across two different placebo tests across 1,000 simulations. Share of observations is denoted in percent on y-axis, estimated windfall gain coefficient is shown on x-axis. Left panel shows simulations where windfall gains are randomly shuffled 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 in 2.8% of 1,000 simulations in left panel and in 4.4% in right panel.

Figure C.2: Sensitivity analysis: Dropping dyads



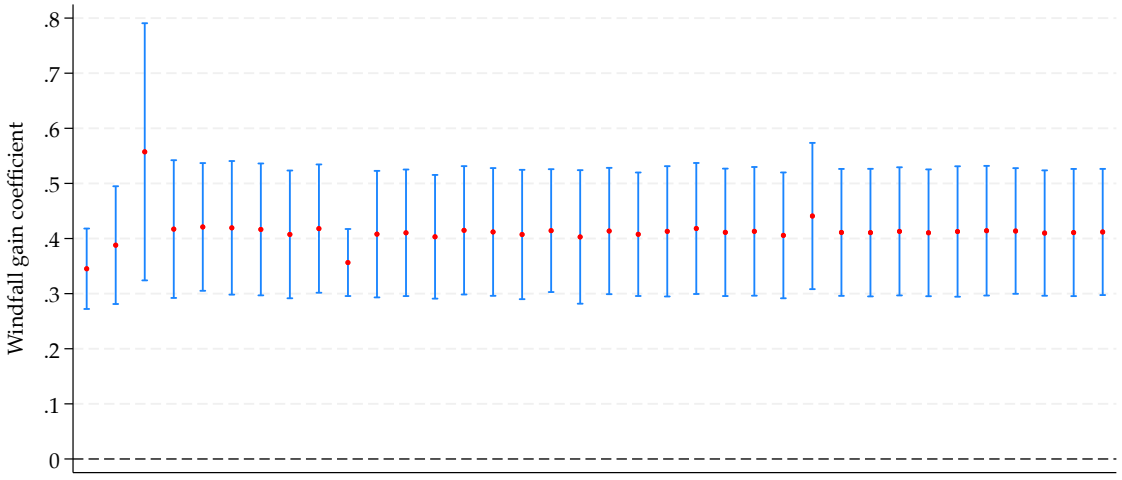
Note: Figure shows how windfall coefficient behaves when dropping entire dyads, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.3: Sensitivity analysis: Dropping entire countries



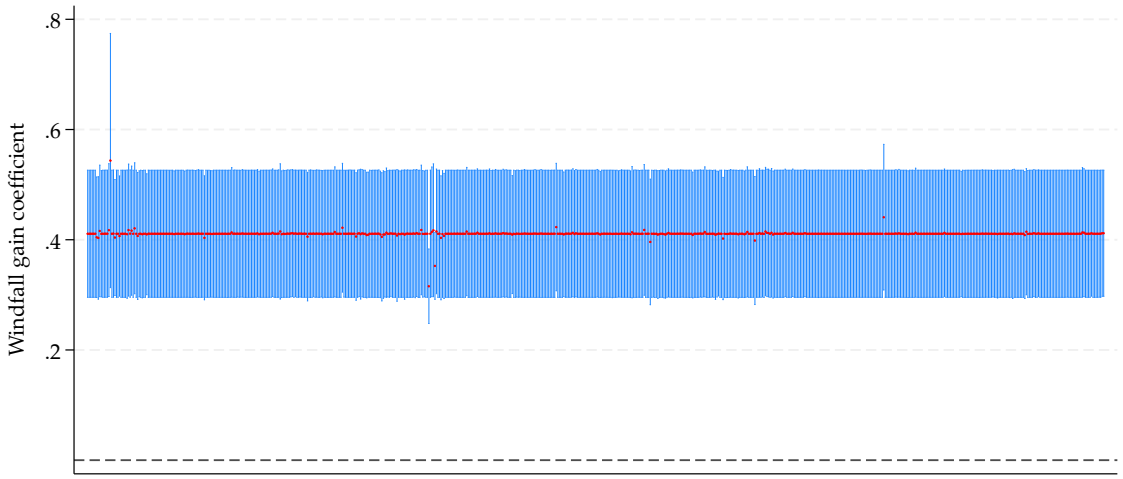
Note: Figure shows how windfall coefficient behaves when dropping entire countries, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.4: Sensitivity analysis: Dropping entire years



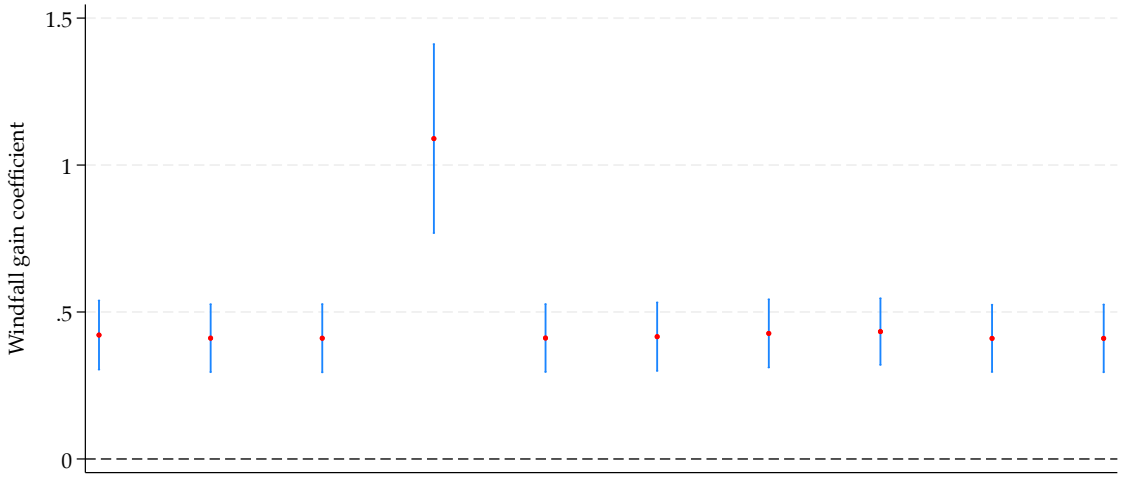
Note: Figure shows how windfall coefficient behaves when dropping all conflicts starting in a given year, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.5: Sensitivity analysis: Dropping entire conflicts



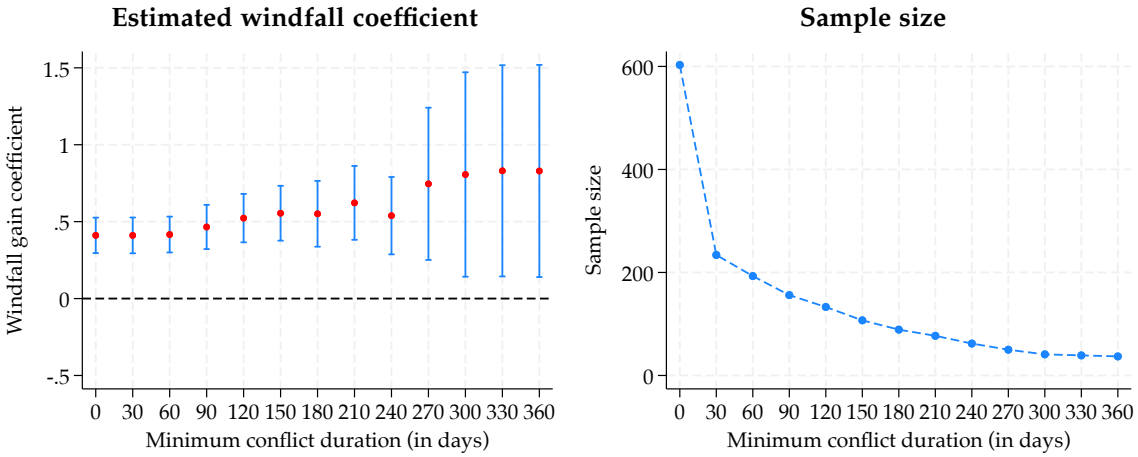
Note: Figure shows how windfall coefficient behaves when dropping entire conflicts, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.6: Sensitivity analysis: Dropping products



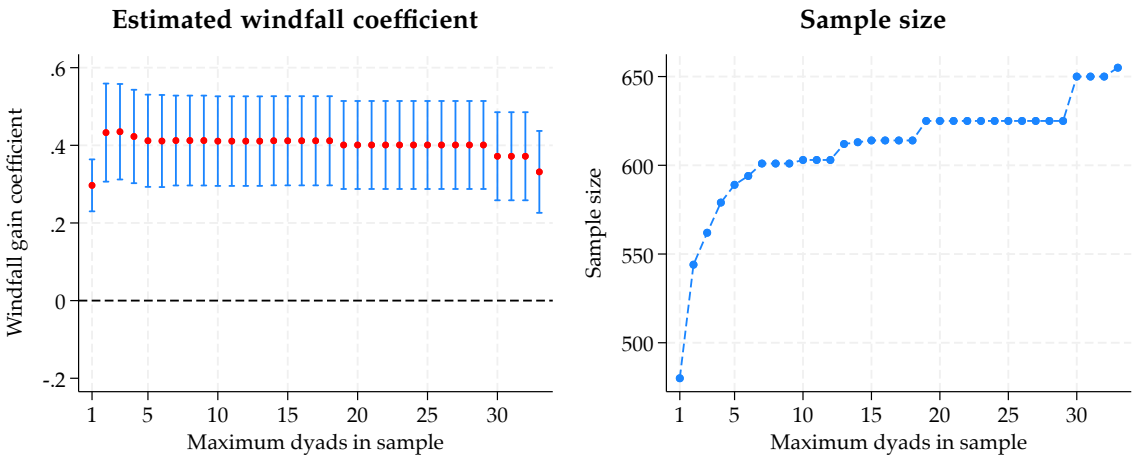
Note: Figure shows how windfall coefficient behaves when dropping excluding products, one at a time. Based on the baseline specification of Table 2, Column 4.

Figure C.7: Coefficients depending on conflict duration



Note: Figure shows how sample and results depend on excluding very short conflicts. For both panels the x-axis depicts the restriction of the sample depending on the minimum conflict size. Left panel shows estimated windfall coefficient on y-axis. Vertical lines mark 90% confidence bounds. Right panel shows sample size.

Figure C.8: 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.