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## Battlefield Bargains: The High-Stakes Dance of Rebel Finances and Ceasefire Deals \*

Sebastian Kupek<sup>†</sup>

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#### Abstract

Wars fueled by resource wealth pose complex challenges for conflict resolution. I combine data on conflict events, rebel resource dependence, world market prices, and ceasefire details to investigate how fluctuations in rebel funding affect the likelihood of ceasefires. Increased funding opportunities strengthen the rebel position and decrease the bargaining range between belligerents. Higher resource rents prolong the conflict and make resolution less likely. Using the exogenous variation of world market prices, I find that rebels react strongly to changes in their budget situation. Price increases reduce the likelihood of rebels entering ceasefires connected to conflict resolution.

*Keywords*: ceasefires, civil conflict, natural resources, conflict resolution *JEL*: D74, L70, O13, Q34, F51, F52

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## 1 Introduction

Civil wars remain the most prevalent form of armed conflict globally. According to UCDP data, in 2020, roughly 20 times more intrastate wars were actively fought than interstate wars. Many of these interstate wars are financed and fueled by natural resources. From the infamous "blood diamonds" in Sierra Leone, the oil reserves in Syria used by the Islamic state, to the gold mines in the Democratic Republic of Congo, rebel groups often rely on these types of income sources to finance their war or satisfy their economic hunger. Revolutionary and ideological ideas do not solely drive these groups; they are also heavily motivated by a strong desire to profit economically from the conflict (Ross, 2004, Buhaug et al., 2009, Dube and Vargas, 2013). Understanding the factors shaping rebel group behavior in civil conflict is critical for successful peace-building efforts. The escalating civil war in Sudan, accompanied by famine, is one of the most recent examples of how natural resources fuel conflict and help only a handful of generals and accomplices to achieve economic success (see Walsh, 2024). The rebels heavily exploit gold reserves from the Darfur region, while the government troops control the largest gold mine in Sudan. Without control over a working airport or other formal trade routes, the rebels must smuggle their resources via neighboring countries, mainly South Sudan and Uganda. The government troops, on the other hand, engage directly in trade with the United Arab Emirates. While government troops and rebels are undoubtedly committing horrendous war crimes, the illicit gold trade does not only help to keep the conflict well-funded and armed, but it also provides riches to a small selected group, profiting immensely from the war. Incentives for conflict resolution could not be worse aligned.

This paper tries to provide insight into how economic resources shape the conflict behavior of rebel groups, primarily how funding fluctuations affect peace-building efforts and, ultimately, how incentives for conflict resolution can be better aligned in the presence of economic opportunities of war. I combine several datasets to investigate how economic shocks to the funding sources of rebel groups affect their conflict behavior and especially their willingness to enter conflict resolution.

The following two case studies illustrate how economic opportunities can differently shape the trajectory of a conflict. In the Angolan civil war in 1990, the rebel group "União Nacional para a Independência Total de Angola" (UNITA), heavily funded by alluvial diamonds, was fighting against the "Movimento Popular de Libertação de Angola" (MPLA) government, financed by oil revenues (Wennmann, 2009, Ross, 2004). During 1998-2002, the MPLA faced favorable economic conditions due to increasing oil prices and rising oil revenues. The UNITA group was not so

fortunate and had to bear the increased transaction cost of their alluvial diamonds. The early version of the Kimberly Process<sup>1</sup>, starting in May 2000, made it harder for the rebel group to export diamonds to the global markets, substantially limiting their funding opportunities (Binzel *et al.*, 2023). This economic downturn for UNITA significantly contributed to the conflict's resolution in 2002.

The conflict between the "Allied Democratic Forces" (ADF) and the government of Uganda nicely shows how rebel groups outgrow their ideological goals and focus solely on economic gains (see Titeca and Vlassenroot, 2012 for a detailed description). The ADF was founded in 1995 and started with the first attacks in 1996. The group came from a Ugandan Islamic background and had the initial goal of overthrowing the government of Uganda. After intense counter-insurgency efforts by the Uganda People's Defence Force (UPDF) against the ADF, the group retreated into the mountainous border between Uganda and the Democratic Republic of Congo (DRC). From the border region, the ADF relied on guerrilla tactics and committed terror attacks targeting police posts, UPDF encampments, civilian administration, and, increasingly, civilians directly. In the border region and the DRC, the ADF managed to get control over several smaller gold mines, where they extorted or taxed the profits and minerals. Later in the conflict, they added timber and coffee to their portfolio mix. With rising gold prices, the ADF disconnected from their initial goal and Ugandan politics and switched their focus on profit generation from their operations. Distancing themselves from politics and focusing solely on revenue generation brought the rebel group the attribute "rebellion without a cause" (Titeca and Vlassenroot, 2012).

In this paper, I explore the connection between rebel budgets and their conflict behavior, trying to generalize the two case studies from above to a global scale. A price increase of the commodities used by rebel groups has a series of effects. The rebel group will have more budget available and more financial possibilities at hand. This improves the bargaining position of the rebel group vis-à-vis the government, potentially making conflict resolution more difficult. Due to the price increase, the rebel group has a higher incentive to use the natural resource opportunities they got through the conflict. Engaging in conflict resolution with the government risks the newly gained wealth of the group since it is not probable that the rebel group would keep complete control over their resources after the end of the conflict. Therefore, I hypothesize that rebel groups experiencing a price increase in their commodities are less likely to engage in conflict

<sup>&</sup>lt;sup>1</sup>The Kimberley Process Certification Scheme (KPCS) tries to limit the trade of conflict diamonds by tracking diamonds and gemstones and allowing world market trade only for certified stones. The KPCS started in May 2000, involving South Africa, Botswana, and Namibia. Prior to the official UN resolution on the KPCS in 2003, countries already able to issue certificates were urged to do so immediately. Before the KPSC, UN sanctions and embargoes were trying to limit the trade of conflict diamonds. Their effectiveness in reducing trade is yet debated.

resolution. This idea is formalized in the model by Addison *et al.* (2002). In their paper, they develop a two-actor conflict model of a government and a rebel actor receiving different levels of utility depending on the state of the conflict. A price or endowment increase of the commodities controlled by one actor reduces the peace effort of the respective actor for any level of effort of their counterpart. Increased commodity prices will make conflict resolution less likely, driven by the conflict party that experienced the gain. However, Addison *et al.* (2002) do not provide empirical evidence for their model.

Similar to the behavior of the ADF in the case study above, rebel groups experiencing these economic gains might switch their focus permanently, or at least in the short to medium term, to profit generation. It can be economically beneficial for a rebel group to abstain from intense warfare and focus on rent extraction while upholding the conflict state. Focusing on rent extraction and economic gains leads to prolonged low-intensity conflicts and decreased fatalities. Some evidence for this phenomenon can be found in the literature. For example, Conrad et al. (2019) and Lujala (2009) show that rebels with access to natural resources prioritize continuing to finance their operations over acquiring weapons and bolstering their military strength and even prefer to continue the conflict over victory. The idea that with rising commodity prices, rebel groups tend to enter an exploitative phase of the rebellion and focus on rent extraction has also been shown by Reeder et al. (2022). They use data on resource usage of rebel groups together with world market prices to build a sharp ratio and investigate rebel behavior, given the efficiency of their portfolio. Their results show that groups with efficient and high-returning portfolios decrease conflict intensity to exploit the resource rents better. The idea that rebel groups prefer low-intensity conflict to peace or full war when it creates a higher profit for them is further formalized in Addison et al. (2002).

I hypothesize that rebel groups experiencing positive return shocks on their commodities are less likely to enter ceasefires connected to conflict resolution (high-cost ceasefires). Still, return increases do not affect ceasefires that mainly have tactical or other purposes, unconnected to conflict resolution (low-cost ceasefires). Rebel groups want to continue to profit from rent extraction and the economic opportunities of war. Increased returns tend to reduce conflict intensity, especially when the rebel group is entering an exploitative phase and, like the ADF, switches the focus to rent extraction.

I aim to contribute to the literature by providing granular empirical insights into rebel group behavior in active conflicts. I empirically explore how budget shocks affect the conflict trajectory and the willingness of rebel groups to enter conflict resolution. To examine this, I introduce the notion of high-cost and low-cost ceasefires based on the intended purpose of the rebel groups. I further show that increasing rents, contrary to common belief, tends to reduce the conflict intensity and that rebel groups might prefer low-intensity warfare to full-scale war. This analysis differs significantly from the conflict literature, which has primarily focused on conflict onset in combination with natural resources but has neither investigated how natural resources shape the conflict trajectory nor pathways to conflict resolution.

Civil war has been intensely studied, particularly the factors influencing its onset. Blair et al. (2021), Denly et al. (2022) and Berman et al. (2017) investigated natural resources as culprits and catalysts contributing to initiating, facilitating, or financing civil wars and exploring their heterogeneity like loot-ability and capital vs labor intensity with ambiguous results. Wennmann (2009) argues armed conflict unlocks economic opportunities for rebel groups, and Conrad et al. (2019) finds evidence that some rebels prioritize control over natural resources over achieving outright victory. Still, empirical evidence on the impact of economic factors and budgetary fluctuations on rebel group behavior remains scarce. Ross (2004) examines these strategies in the context of Liberia and the Democratic Republic of Congo and found that rebel groups wanting to preserve access to natural resources lengthened the civil war because the resources provided them with economic incentives not to sign a peace contract, supporting my hypotheses with additional case studies. Additionally, Stearns (2022) suggest that conflicts in Africa move towards low-intensity warfare, whose main goal is not to overthrow the state but rent-seeking. Rebel behavior seems to depend on whether the group expects to transition from a weak to a strong and better-militarized group (Qiu, 2022). Even highly ideologically motivated rebel groups, such as the Colombian groups "Fuerzas Armadas Revolucionarias de Colombia" (FARC) and "Ejército de Liberación Nacional" (ELN), show strong economic motivation (Dube and Vargas, 2013). Reeder et al. (2022) build an efficiency measure of rebel portfolios based on resource usage and commodity prices. Further, they divide rebel groups into vulnerability, emboldened, and exploitative phases dependent on their portfolio efficiency and connected to the intensity of the conflict. They find that increasing commodity prices shifts rebel groups into an exploitative phase, focusing on rent extraction and reducing conflict intensity. This finding strongly aligns with my hypothesis of rebel groups switching the focus of their operation. Empirical evidence connected to conflict resolution or ceasefires as a first step remains scarce. Clayton et al. (2023a) is a notable exception. They examine the effects of conflict factors, such as casualties, on ceasefire agreements. Their analysis does not include changing economic conditions.

To investigate my research questions, I employ a novel dataset constructed by merging data on conflict events from the Uppsala Conflict Data Program (UCDP) (Sundberg and Melander, 2013) with information on rebel resource dependence from the Rebel Contraband Dataset (RCD) (Walsh *et al.*, 2018). I construct price shocks from world market commodity prices (World Bank, 2021, U.S. Bureau of Labor Statistics, 2024, Bloomberg, 2024) given the "portfolio" of illicit financing of each group. Together with the detailed dataset on ceasefires (Clayton *et al.*, 2023b), I estimate how shocks to a rebel group's budget influence their likelihood of entering an agreement. I examine the effect of the return shocks on conflict intensity by using fatalities from the UCDP dataset as a proxy.

My empirical results support my hypotheses. A positive shock (price increase) to the rebel group's budget significantly reduces their likelihood of entering a high-cost ceasefire. A 10 percentage point increase in the mean return of the rebel groups' "portfolio" decreases the propensity to enter a high-cost ceasefire agreement by around 1-4 percentage points. This effect remains stable for over 6 months after the initial price shock. For simpler low-cost ceasefires, as predicted, I do not find a significant effect in either direction. Further, I find that return increases decrease the conflict intensity, measured by fatalities and number of attacks. Rebels are also not more likely to end the conflict either by ceasefire or by just stopping to fight. I can confirm my results by testing how portfolio shocks affect African peace negotiations. Here, I find the same results. Favorable economic conditions lead to a decreased probability of the start of negotiations. My findings align with the notion that when natural resources are involved, conflicts tend to last longer (Fearon, 2004), shedding more light on the reasons for the prolonged conflicts and the particular rebel behavior.

The rest of the paper proceeds as follows. Section 2 outlines the theoretical considerations in more detail. Section 3 describes the dataset construction, explains the calculation of the portfolio returns and gives a short descriptive overview of the data. Section 4 explains the empirical strategy and discusses the necessary assumptions. Section 5 presents the baseline results, Section 6 shows robustness checks, and Section 7 analyzes the heterogeneity and shows evidence of the mechanism. Section 9 concludes the paper.

## 2 Theoretical considerations

More funding can strengthen the rebel position by being able to better equip themselves for attacks, by better being able to avoid government repression and attacks, or a mix of both (see Cunningham *et al.* 2009 for a formalization of these different "powers"). In both cases, the rebel group has a better bargaining position against the government, complicating conflict resolution. The price increase gives the rebel group a higher incentive to focus on rent extraction and use

the economic opportunities of war. Any bargaining solution moving towards conflict resolution will risk the newly gained wealth of the rebel group. It is unlikely that the government will agree to continued control of the resources by the rebel group after the end of the conflict. Even if both parties can agree on some concessions, the rebel group will lose parts of the resource wealth. Therefore, groups experiencing commodity price increases should be less likely to engage in conflict resolution. Higher commodity prices might lead groups to switch their focus from conflict to rent-seeking. Abstaining from intense warfare and focusing on rent extraction can be the economically best path for a rebel group. However, it needs to uphold the conflict state to access the natural resources. Focusing on rent extraction, therefore, leads to prolonged low-intensity conflicts with decreased fatalities.

Conflict resolution has many dimensions. In this paper, I focus on the most observable and accessible one: ceasefires between the belligerents. Ceasefires are an intermediate step towards conflict resolution and can approximate the willingness to end a conflict. However, one has to be careful about the different purposes of the agreements. It is crucial to distinguish between the immediate objective, stopping hostilities, and the underlying intent of a ceasefire. Building on Clayton et al. (2021), ceasefire agreements can have legitimate underlying intents like conflict resolution or ending the status of the war, but also dishonest motives like gaining a military advantage. Rebel groups might enter ceasefires and use the conflict breaks for strategic reasons like regrouping, rearming, or other tactical maneuvers without any intention to start a peace process. I follow the arguments of Clayton and Sticher (2021) to distinguish which ceasefire resembles a peace intention and which does not. They argue that groups with devious motives prefer simple cessations of hostilities that do not involve a compliance mechanism. Compliance mechanisms mainly refer to monitoring and verification systems to ensure that both conflict parties adhere to and comply with the ceasefire terms. Monitoring is often achieved by including neutral third parties, such as international agencies or even the general population, as monitoring and verification authority (see United Nations Department of Political and Peacebuilding Affairs, 2022, Buchanan et al., 2021). Cessations of hostilities without a compliance mechanism can be swiftly called on or off and do not require negotiations on precise conditions or any monitoring and verification measures. Rebel groups wanting to use the ceasefire for a military advantage (or those uncertain of the intent of their counterpart) will opt for a simple cessation of hostilities to keep the cost of defection low. This way, they can quickly escalate violence and attacks again, minimize the cost of breaking the ceasefire later on, and avoid potential blame for its breakdown. Cessations of hostilities involving compliance mechanisms are a more robust sign of willingness to engage in conflict resolution because this type of agreement is more costly

for the group. Based on the cost notation, I call simple cessations of hostilities from now on "low-cost ceasefires" and ceasefires involving a compliance mechanism "high-cost ceasefires". To summarize, rebel groups wanting to honestly engage in conflict resolution most likely choose a high-cost ceasefire, while groups planning to use the ceasefire for strategic or military purposes opt for a low-cost ceasefire.

These theoretical considerations let me form the following hypotheses. Rebel groups experiencing positive return shocks on their commodities are less likely to enter high-cost ceasefires. After a positive return shock, the group has an increased incentive to profit from rent extraction and resources primarily available in a conflict state. The better economic opportunities reduce the bargaining range vis-à-vis the government, making conflict resolution less likely. A commodity price increase of funding sources leads to a lower probability of high-cost ceasefires. Low-cost ceasefires are not affected by the price increases since they are not meaningfully connected to conflict resolution and, to a large part, can still serve for strategic and military purposes. I do not expect return shocks to affect low-cost ceasefires in either direction significantly. Lastly, increased returns tend to decrease the conflict intensity. This is particularly true when the rebel group enters an exploitative phase and focuses on rent extraction.

I assume that the government counterpart has relatively stable behavior and does not react strongly to the price changes of the rebel commodities. This assumption is, of course, simplifying, but some arguments support this idea. Since I am only looking at short-term price shocks, the government will likely not react quickly to price changes in the commodities that the rebel group uses. This is especially true if the government is unaware of the kind of natural resources the group uses and is unsure about the rebel's budget. In the example of the ADF, a substantial part of the controlled mines is located in the DRC. It is not completely obvious that the government of Uganda was aware of all the resources used to fund the ADF operations. As formalized in the model by (Addison *et al.*, 2002), the counterpart experiencing the gain in funding reduced its peace efforts. It is plausible to assume that in the medium to short term, mainly the rebel group and not the government react. One can also think about the price increase narrowing the bargaining space between the belligerents, making an agreement less likely because of the higher demands by the rebel side.

## 3 Data & descriptives

#### 3.1 Data

I construct a novel dataset combining conflict data from the UCDP Georeferenced Event Dataset version 23.1 (Sundberg and Melander (2013), Davies *et al.* (2024)), together with information on ceasefires stemming from the ETH/PRIO Civil Conflict Ceasefire Dataset (Clayton *et al.*, 2023b) and information on resource usage by rebel groups using the Rebel Contraband Dataset (Walsh *et al.*, 2018).

The structure of the data setup closely follows Clayton et al. (2023a). Similarly to their data structure, I use a month dyad structure and include every conflict once it enters the UCDP Database. I only have dyads with one rebel group fighting against one government, and I exclude conflict dyads with fights only between rebel groups. A conflict dyad remains in the dataset until it experiences two months without conflict fatalities. Every subsequent month without conflict fatalities is then dropped from the dataset. If conflict resumes and fatalities occur, the dyad months are from then onward again in the dataset. Suppose a ceasefire was reached in a given dyad, followed by zero fatalities. In that case, I assume the ceasefire did not break and exclude all subsequent months without fatalities from this conflict dyad. I do not distinguish ceasefires by the length or type of the agreement and only look at the month in which the ceasefire was announced. Clayton et al. (2023a) employ this monthly structure since the UCDP data is not dated precisely enough to allow for a more granular time structure. Similarly, I only include ceasefires that I can match to the respective dyad. Following Clayton et al. (2023a), I exclude ceasefires that are continuations of previous agreements since they might have a different data-generating process and have a different underlying incentive structure, which is not mainly related to economic factors.

Ceasefires are classified as low-cost ceasefires and high-cost ceasefires, where high-cost ceasefires are agreements that entail a compliance mechanism, and low-cost ceasefires do not entail such a mechanism. In the dataset by Clayton *et al.* (2023b), low-cost ceasefires correspond to "cessation of hostilities" and high-cost ceasefires to "cessation of hostilities with compliance mechanisms" and "definitive ceasefires". A compliance mechanism usually refers to monitoring and verification systems in place (see United Nations Department of Political and Peacebuilding Affairs (2022), Buchanan *et al.* (2021) for a detailed description of monitoring and verification). Monitoring refers to observing both parties to ensure adherence to the ceasefire terms, whereas verification is assessing and confirming that both parties comply with the agreement provisions. Usually, these two compliance tools are used together. Monitoring and verification are often conducted by international agencies such as the UN or African Union, neutral third parties, a joint monitoring committee comprising representatives from all conflict parties, or just by the general population. Definitive ceasefires are part of peace agreements and contain additional provisions for disarmament, demobilization, and reintegration (DDR).

Many rebel groups do not only use "legal" natural resources like metals or agricultural products but also resort to illegal substances and products, such as coca or opium, to finance their operations. Unfortunately, monthly world market prices for illegal goods are hardly available. Therefore, I can only include rebel groups that use legal commodities. I use the Rebel Contraband Dataset to filter for the rebel groups that use natural resources for which price data is available. This dataset shows which resources were used by which rebel group in what year, and, if available, it gives a rough estimate of the funding that the rebel group earned with the resource and respective method of usage (such as extortion, smuggling, or theft). My dataset includes cassiterite, coal, cobalt, cocoa, coffee, diamonds, gems, iron, oil, gold, tea, timber, tin, and wolframite.

I use world market price data to construct rebel-specific budget shocks. Most of my data on commodity prices comes from the World Bank "Pink Sheet" Data (World Bank, 2021), which offers monthly price data for various commodities. Monthly data on diamond prices is scarce. Even RapNet, one of the diamond industry's most prominent marketplaces, only offers yearly historical data. Therefore, I use the import price index of diamonds provided by Federal Reserve Economic Data (U.S. Bureau of Labor Statistics, 2024) as a proxy for diamond and gem prices. The price data for wolframite (tungsten) and cobalt are taken from Bloomberg (Bloomberg, 2024). Unfortunately, direct prices for cassiterite do not exist, or at least are not available on a global scale. I use tin prices as a proxy for cassiterite prices. Tin is extracted from cassiterite, making it effectively a precursor of tin. Therefore, cassiterite prices should closely follow tin prices.

The total dataset contains 4220 conflict dyad months, with 66 conflict dyads where 61 rebel groups are fighting with 30 different governments, covering the years 1990 to 2020. A more detailed description of the dataset construction can be found in Appendix A. Table A.1 in the Appendix displays all dyads included in the dataset.

#### 3.2 Return construction

One crucial question to address is how to construct the "portfolio" shocks to the rebel's budget. A substantial number of rebel groups finance themselves via more than one natural resource. Often, they exploit different natural resources using distinct methods. I construct a measure combining all natural resources used by a respective group and comparing it to the base year of conflict entry in the following way:

First, I calculate the average price of each commodity used over the first 12 months after the rebel group's entry into conflict. Then, I calculate each commodity's month-specific net returns compared to the average price at conflict start. Here,  $p_{i,\tau,c}$  refers to the price relative to the start of the conflict of the specific dyad. I compare the average over the first 12 months to the commodity prices after 12 months  $p_{i,t,c}$ . The subscript  $_i$  refers to the conflict dyad. Time  $_{t,\tau}$  is relative to the conflict start of the specific dyad. Subscript  $_c$  refers to the distinct commodity. I calculate the net return r for each commodity before aggregation.

$$r_{i,t,c} = \frac{p_{i,t,c} - \left(\frac{1}{12}\sum_{\tau=1}^{T=12} p_{i,\tau,c}\right)}{\frac{1}{12}\sum_{\tau=1}^{T=12} p_{i,\tau,c}}$$
(1)

i : Conflict Dyad

c : Commodity

t, $\tau$  : Conflict period (month),  $t \neq \tau$ 

These commodity-specific net returns are aggregated across all commodities the respective rebel group uses. Hereby, I use as much information as possible about how much funding a rebel group earns with its different types of resources. The Rebel Contraband Dataset has a rough indicator for the funding size earned with a commodity. The dummy coded variable "funds" indicates whether the group earned more than \$5 million for a specific activity or less than \$5 million. If the group earns more than \$5 million from one or more sources, I will only consider these resources for the aggregation. Otherwise, all net returns are considered equally. The RCD does not provide more detailed information on the amounts earned from specific natural resources. This way, I focus on the essential funding streams for the rebel group. The commodity-specific net returns are simply averaged over the conflict-dyad-month:

$$r_{i,t} = \frac{1}{C} \sum_{c=1}^{C} r_{i,t,c}$$
(2)

Calculating the dyad-specific returns in this way allows me to compare price and budget shocks across different commodities and rebel groups and control for the amount of budget the respective group had at the time of conflict onset.

#### 3.3 Descriptives

Conflict location is depicted in Figure 1. The map shows the conflict location based on the state acting as a counter-party for the rebel group. The data coverage is worldwide, with some

conflicts even in Europe and South America. African and Asian countries accommodate most of the armed conflicts. The map further shows the number of conflicts in a respective country. The conflict hot spots in Africa are the Democratic Republic of Congo and the neighboring country Burundi. In the Democratic Republic of Congo, multiple rebel groups used the rich deposits of rare earths such as gold, diamonds, cassiterite, cobalt, and agricultural commodities such as timer to finance their illicit operations. In Burundi, however, most rebel groups exploited tea and coffee production to meet their financial needs. India is the country with the most civil conflict, as measured by the number of rebel groups. In India, there were at least 11 active rebel groups between 1990-2020, mainly using tea and timber and occasionally metals such as gold or iron to finance themselves. In Southeast Asia, Myanmar and Cambodia experienced significant outbreaks of violence. In both countries, timber and diamonds were the most common sources of income for rebel groups using natural resources.



Figure 1: Map of conflict locations & number of civil conflict dyads

Notes: The Figure shows the conflict location based on the state acting as the counter-party of the rebel group. The graph is based on the UCDP Georeferenced Event Database. "# Conflicts" refers to the number of dyads in conflict with the state counter-party.

Table 1 shows summary statistics. There are, in total, 103 high-cost ceasefires (with compliance) and 296 low-cost ceasefires (without compliance). The number of months with high-cost ceasefire is smaller than the total ceasefire amount since, given the sometimes short duration, they can occur more than once. As it becomes clear from the table, ceasefires of both types are rare events. Out of 4220 conflict months, only 268 experienced some ceasefire. Only 94 months involved a high-cost ceasefire. In total, there are 66 conflict dyads, of which 39 experienced a ceasefire. Only 22 dyads experienced a high-cost ceasefire. Note that high-cost and low-cost ceasefires are not exclusive. Low-cost ceasefires are often present in conflicts that eventually

have a high-cost ceasefire and conflict termination.

	Ceasefire type						
-	Total ceasefires	Low-cost ceasefire	High-cost ceasefire	No ceasefire			
Ceasefire count							
Total ceasefires	399	296	103	-			
Months with ceasefire	268	198	94	3952			
Dyad distribution							
Dyads with ceasefire	39	36	22	27			
Ceaserfire frequency							
Mean ceasefires per dyad	6.05	4.48	1.56	-			
Median ceasefires per dyad	1.50	1.00	0.00	-			
Time until 1st ceasefire							
Mean months	33.10	37.39	28.36	-			
Median months	11.00	14.00	12.00	-			

Table 1: Summary statistics and distribution of ceasefires by type

Notes: The table shows descriptive statistics for ceasefires across conflict dyads. The ceasefire types "low-cost" and "high-cost" are not mutually exclusive; both can occur in the same dyad or, in extreme cases, even in the same month. There are 66 dyads in total. Ceasefires can occur more than once in the same month. For high-cost ceasefires, that is a rare case (8 months have more than one occurrence, 86 have only one occurrence).

Figure 2 shows the distribution of types of resources rebel groups use. I do not distinguish between the methods used to acquire or profit from natural resources (i.e., theft, smuggling, extortion, or booty futures). Usage here refers to usage in the first year of conflict. Oil and timber are my sample's most used natural resources for conflict finance, and nearly one-third of all rebel groups use them. Coal, diamonds, gold, and tea are used by roughly one-sixth of the groups, whereas any of the groups hardly use cobalt, iron, tin, and wolframite. The selection of which resource to use for funding crucially depends on the availability and possibility of acquiring the commodity. A distribution of the most used resources by country can be found in Appendix B.

Figure 3 shows price developments of all commodities. The first month is set to a value of 100 to standardize prices. For all commodities except wolframite and diamonds, this corresponds to the 01-1990. Due to data availability, for diamonds, the first month is the 12-1992, and for wolframite, the 01-2001. Prices for most of the natural resources show substantial variation in time. The exception is the price development of diamonds based on the FRED Import Price Index of Diamonds. The diamond price exerts only slight variation compared to the other commodities. The graph does not display prices for cassiterite and gems. As described in Section 3.1, cassiterite prices are unavailable. I cannot distinguish between gems and diamonds in the FRED import price index of diamonds, and consequently, I use the index as a proxy



Figure 2: Resource usage by rebel groups across conflict dyads

Notes: The graph shows the resource usage grouped by dyad. One group in a dyad can use more than one resource for funding. It is shown by dyad and not rebel group, since groups sometimes use different resources in different conflicts. For example, the Islamic State used diamonds and timber in Afghanistan but used oil in Syria, Egypt, and Iraq. The observation that groups fight in more than one dyad occurs rarely. In my data, only the Islamic State, Croatian irregulars, and Serbian irregulars appear in more than one conflict dyad. The resources are based on the first year of conflict start.

for both types of gemstones. The types of price shocks the particular rebel group experiences depend on the starting time, duration of the conflict, and their specific "portfolio". Figure B.4 in Section B displays the realized net returns of all rebel groups by dyad.

Figure 4 depicts the distribution of all realized net returns. The returns are centered relatively closely around zero with a median of 0.02 and a mean of 0.24. The distribution of the returns is right-skewed with long right tails. Negative returns do not exceed beyond -0.8. For a rebel group, this would mean their portfolio of commodities is only worth 20% of what it was at the start of the conflict. Negative returns below -1 are impossible by construction since a return of -1 means that the group's commodities lost all their value and have a zero price. The positive returns can take high values up to 3.75, meaning more than 3 times the portfolio value at conflict start. Most rebel groups experience moderate positive and moderate negative returns, with some groups having extreme portfolio returns of over 300% in certain months. Returns between 100% - 200%, meaning rebels have two times the funding compared to the start of the conflict, are observed somewhat frequently.



Figure 3: Indexed price trends of key commodities used by rebel groups

Notes: The graph shows the price trends of the key commodities used by rebel groups. The prices are standardized to the first month, equaling 100. For all commodities except diamonds and wolframite this is 01-1990. For wolframite, the first month is 01-2001, and for diamonds, 12-1992 due to data availability.

Figure 4: Distribution of monthly net returns across rebel groups and dyads



The graph shows the realized monthly net returns distribution across all rebel groups and dyads. Returns are calculated as described in Section 3.2. The minimum net return is -1, indicating a 100% loss of portfolio value.

## 4 Empirical strategy

To estimate how budgetary fluctuations affect the conflict behavior of rebel groups and, ultimately, the probability of low-cost and high-cost ceasefires, I use the returns constructed in Section 3.2 as a financial shock to the rebel group and estimate its effect on different conflict outcomes. The return derived from the portfolio development and prices at conflict onset is a proxy for the financial resources of the particular rebel group. To avoid endogeneity in the returns due to rebels switching their funding source because of increased prices, I only take the funding sources in the first year of the conflict into account for the resource calculation, basically using the "initial allocation" of the rebel groups.

I make the following assumptions to ensure the exogeneity of the return shock to the rebel portfolio.

(i) The rebel groups act as price takers, meaning their illicit selling or smuggling of resources should not meaningfully impact the world market price of those commodities. Influencing world market prices could be the case if all or most of the worldwide deposits of one particular resource are located in a specific conflict region and are in the course of conflict captured by rebels. Given the resource usage depicted in Figure 2, this is quite unlikely to be the case. Most of the natural resources used by the rebel groups have large deposits outside the conflict areas. Cobalt might be a bit of concern since the world's largest deposits are located in the Democratic Republic of Congo, which experiences substantial conflict episodes. Since some cobalt reserves exist in Australia and Indonesia and I observe only one rebel group (Alliance des Forces Démocratiques pour la Libération du Congo, AFDL) using cobalt as a funding source, this is a minor concern. I test in Section 6.1 the robustness of the results when excluding cobalt as a funding source. The results remain the same.

(ii) Black market prices are proportionally affected by movements in the world market prices. Rebel groups do not sell at world market prices. However, I assume that drops or increases in the world market prices will affect the black market prices proportionally and will meaningfully affect the budget of the respective rebel group. If prices decrease, the buyer of the illicit goods has no incentive to keep paying the same amount as before the decrease because his outside option is now cheaper. The buyer will likely pass price decreases to the rebel group and vice versa for price increases. Systematic evidence on black market prices and trade patterns is rare, but Ralby (2017) provides some case studies on hydrocarbons.<sup>2</sup>

 $<sup>^{2}</sup>$ Hydrocarbon theft is widespread in Mexico, with criminal gangs selling oil and diesel either directly to consumers or making use of their criminal network to sell it to industrial buyers. They sell at a discounted market price, in extreme cases, up to 50%. Some fuel or crude oil is smuggled to the US and sold below-market prices. Moroccan refineries bought illegal crude oil from Nigeria, laundered with legitimate oil on a Ghanaian offshore

(iii) The government side is not or not meaningfully affected by the price shocks of the commodities. I assume that the respective government in the conflict is at most marginally affected by the price shocks. Price movements could affect the government if the government has substantial tax income from natural resources. Anecdotal evidence from Section 1, where the MPLA government financed by oil fought against the UNITA rebels financed by diamonds, shows that this might be a reasonable assumption. To test this more systematically, in Section 6.2, I check whether the return shocks can predict government revenue, government revenue from natural resources, or tax income from natural resources. I do not find any evidence in all three specifications that the rebel portfolio can predict government revenues or tax income. Section C.6.2 shows some additional specifications, all hinting that the assumption is reasonable.

(iv) The diamond price index by FRED accurately captures the price developments. A rough comparison of the diamond import price index with the yearly price index for diamonds suggests that this is overly true. Another problem concerning diamonds and gems is that the price fundamentally depends on various factors such as quality, type, and clarity (Berman *et al.* 2017 argues similarly). Rebels in my sample only use alluvial diamonds and gems, meaning gemstones removed by erosion from their primary source and now deposited in river beds or the shoreline. This identical origin might make these diamonds more "similar", but the general problem remains. In Section 6.1, I show that the direction and magnitude of the effects remain similar when diamonds are excluded.

I estimate the following equation based on monthly return shocks and conflict outcomes:

$$ConflictOutcome_{i,t} = r_{i,t} + \delta_i + \theta_t + \epsilon_{i,t} \tag{3}$$

Standard errors are clustered at the dyad level (i).

Where *ConflictOutcome* is either fatalities in the respective conflict month or a dummy being 1 if a high-cost ceasefire was declared in that month or a dummy for low-cost ceasefires, respectively. Similarly to (Clayton *et al.*, 2023a), ceasefires are measured once declared in that month. They do not necessarily need to start in that month or come into effect. This way, I can better measure the behavioral response and intent of the rebel group following the return shock, especially in cases where there is a time delay between the declaration and the actual start of

oil field. Finished fuel products can then be sold at market prices. In Mozambique, price decreases of crude oil reduced the hijacking of tankers by pirate groups because it was not lucrative anymore (Ralby, 2017). These examples show how illegal actors react to and use world market prices for their trades. Since trading of illicit goods involves discounts, which introduces measurement error, I expect attenuation bias in my estimates.

the ceasefire. In most cases, however, the agreement started the same day or shortly after the declaration. In Section C.5, I show that the results are nearly identical when using the start of the ceasefire instead of the declaration time.

The term r refers to the monthly returns of the rebel group's portfolio compared to the prices at conflict onset (as calculated in Section 3.2).  $\delta$  are dyad fixed effects and  $\theta$  are year-month fixed effects.

For the estimation, I use a Poisson regression in the case of the fatalities outcome. Here, I have count data, which also includes zero outcomes. A Poisson model is not applicable to the ceasefire outcomes. Ceasefires are a relatively rare outcome in my dataset. This is not too surprising since in ongoing conflicts, ceasefires will typically only be proclaimed at the end of conflict or before (attempted) conflict breaks. I use a probit and logit model to handle the problem with excessive zeros.

Including fixed effects in a probit or logit regression can lead to biased results due to the incidental parameter problem (see Neyman and Scott (1948)). To circumvent this potential pitfall, I use the analytical and jackknife corrections for fixed effects estimators of model parameters proposed by Fernández-Val and Weidner (2016). Czarnowske and Stammann (2020) provide a detailed discussion on the correction method and further implemented it in their *alpaca* package in R.

## 5 Results

Table 2 shows the average partial effects of the probit regression of return increases on low-cost ceasefires. The dependent variable is dummy-coded one if a low-cost ceasefire occurred at least once in the conflict month of the respective dyad. The estimation includes dyad and year-month fixed effects, and columns (1) - (7) show the results for different lags of the shock. Hereby, *Lag* 0 means the effect of a return increase on the likelihood of cessation of hostilities in the same month, and *Lag* 1 refers to a return increase one month before. The lag structure lets me assess the stability of the effect over time and allows for time delays until the effect of the price increases reaches the rebel group<sup>3</sup>. The intuition behind this is that a price increase does not increase their budget directly but the rebel groups' "potential" budget. Therefore, a short time delay is reasonable. I find that an increase in the portfolio return of the rebel group does not have a significant effect in either direction on the likelihood of such a low-cost ceasefire being announced. This effect seems to hold for the entire lag structure, allowing for some time for the return shock

 $<sup>^{3}</sup>$ The correlation of the net returns across the lags is displayed in Table B.2 in Appendix B. The net returns are highly correlated across the lag structure.

to reach the rebel group. This finding aligns precisely with my theoretical prediction described in Section 1. Increased returns increase the rebel groups' available budget and strengthen their position vis-à-vis the government, thus reducing the bargaining range between the two counterparties. Since low-cost ceasefires are not connected to conflict resolution and are often used for tactical or military purposes, they do not necessarily relate to the economic status of the rebel group.

	Low-cost ceasefire (without compliance)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Lag 0 Return	-0.0090 (0.0087)							
Lag 1 Return	· · · ·	-0.0040 (0.0086)						
Lag 2 Return			0.0013 (0.0082)					
Lag 3 Return				0.0035 ( $0.0088$ )				
Lag 4 Return				· · · ·	0.0012 (0.0085)			
Lag 5 Return						-0.0009 $(0.0076)$		
Lag 6 Return						()	-0.0019 (0.0076)	
Observations	858	811	782	714	700	671	646	
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Dyad FE Posidual Daviance	√ 541.77	√ 510.15	√ 501.97	√ 479.47	√ 462 54	√ 440.94	√ 499.72	
Null Deviance	773.16	736.68	710.02	665.45	$402.54 \\ 650.77$	636.29	614.23	

Table 2: Average partial effects of net return increase on low-cost ceasefire probability

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a low-cost ceasefire dummy using a probit regression. The low-cost ceasefire dummy equals one if, in that month, a low-cost ceasefire was declared. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Table 3 shows the average partial effect of the probit regression of return increases on highcost ceasefires using the same specification. The dependent variable is again dummy-coded if a ceasefire occurred at least once in the respective conflict month. In line with my theoretical argument presented in Section 1, I find that the likelihood of a high-cost ceasefire significantly decreases after a positive return shock. The effect seems to be stable and significant throughout the lag structure.

To interpret the magnitude of the effect, let me clarify what a marginal increase in the return variable means. An increase by 1 unit in returns, given that I use net returns, translates into a return of 100% compared to the onset year of the conflict. Such an increase would make the rebel group twice as wealthy as when the conflict started. A 100% portfolio return decreases the ceasefire probability by between 9 and 45 percentage points, depending on the time allowed for a price increase to manifest in the rebel group's budget. While a 100% portfolio return is not

entirely unrealistic, looking at the price developments in Figure 3, a 10% increase or decrease might be the more frequent scenario.

For completeness, I estimate both models in Table 2 and Table 3 using a logit specification. The results are shown in Appendix C.2, but the results are, however, nearly identical.

Table 3: Average partial effects of net return increase on high-cost ceasefire probability

			High-cost c	easefire (with o	compliance)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	$-0.0911^{***}$ (0.0145)						
Lag 1 Return	. ,	$-0.1170^{***}$ (0.0269)					
Lag 2 Return			$-0.2527^{*}$ (0.1455)				
Lag 3 Return			, , , , , , , , , , , , , , , , , , ,	$-0.2933^{*}$ (0.1573)			
Lag 4 Return				. ,	$-0.2693^{*}$ (0.1390)		
Lag 5 Return					· · · · ·	$-0.4520^{***}$ (0.0453)	
Lag 6 Return							$-0.2735^{***}$ (0.0908)
Observations	219	205	197	190	164	144	137
Year-month FE	$\checkmark$	Ń	Ń	$\checkmark$	V	V	$\checkmark$
Dyad FE Besidual Deviance	✓ 166 42	√ 157 44	√ 151.46	√ 147 94	√ 139.58	√ 125 75	√ 119.67
Null Deviance	277.39	260.51	251.25	244.12	213.10	190.53	181.89

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a probit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Besides the ceasefire outcomes, I look at other direct conflict outcomes, such as fatalities. Table 4 presents the results of the Poisson regression of return increases on "best estimation of fatalities". The variable "best estimation of fatalities" is constructed monthly and refers to the best estimate of conflict fatalities by UCDP, including civilians, rebels, and government deaths. The results show that a return increase significantly decreases the monthly fatalities in a conflict dyad. Considering a 10% return increase compared to the conflict onset year, this would translate into a reduction of monthly fatalities by around 11.3% (=  $1 - e^{(-0.1208)}$ , since return increase by 1 would be 100%). This effect seems stable and significant for at least six months after the shock, albeit decreasing in magnitude.

Taking the results together, an improved budget situation for the rebel group seems to reduce their willingness to engage in conflict but simultaneously reduces fatalities and number of attacks (see Table C.2 in the Appendix). These patterns suggest that the groups transition towards lowintensity conflict, with increasing returns of their commodities. Importantly, however, conflict does not seem to end. The improved budget does not seem to facilitate conflict resolution

Dependent Variable:	Best estimate fatalities						
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables							
Lag 0 Return	$-1.208^{***}$ (0.3289)						
Lag 1 Return		$-1.126^{***}$ (0.3086)					
Lag 2 Return			$-1.007^{***}$ (0.2948)				
Lag 3 Return			. ,	$-0.9039^{***}$ (0.2789)			
Lag 4 Return				~ /	$-0.8519^{***}$ (0.2568)		
Lag 5 Return					· · · ·	$-0.7762^{***}$ (0.2636)	
Lag 6 Return						( )	$-0.7444^{***}$ (0.2518)
Fixed-effects							
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Observations	3,572	3,536	3,502	3,466	3,432	3,396	3,362
Pseudo $\mathbb{R}^2$	0.86408	0.86267	0.86146	0.86142	0.86167	0.86030	0.85901

Table 4: Effects of net return increase on monthly fatalities: Poisson regression

Notes: This table presents the results of a Poisson regression of the dyad-specific monthly net returns on monthly fatalities. Monthly fatalities refers to the best estimate of total fatalities in the UCDP data. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

or termination. High-cost ceasefires, namely agreements incorporating a compliance mechanism and being the first meaningful step towards an effective conflict resolution, seem to become more unlikely with the increasing wealth of the rebel group. In Section 6.3, I further test whether rebel groups instead take on the simplest form of ceasefires, which is just stopping to fight. These "no fight" ceasefires are significantly negatively affected by increasing returns, suggesting that rebel groups are less likely to initiate a meaningful conflict resolution and do not stop fighting. In line with my theory, rebel groups keep the conflict active on a low level, involving fewer attacks and fatalities, continuing to profit from economic opportunities, and are unwilling to lay down arms.

## 6 Robustness checks

#### 6.1 Excluding diamonds, gems and cobalt

As discussed in the assumptions for the empirical strategy in Section 4, the diamond price index might imperfectly reflect prices in the world market. This might be especially true since I use the FRED diamond index for diamonds and other types of gemstones, and as (Berman et al., 2017) argue in their paper, factors such as quality, purity, and type significantly influence the price of diamonds and gemstones. Therefore, I test the key specifications of the return shocks and the probability of high-cost ceasefires, excluding diamonds and gemstones as natural resources. If a rebel group only relied on one of these commodities, this group is dropped from the sample. I only consider the additional natural resources for the group's portfolio returns if other resources besides diamonds and gems were used. Eleven rebel groups in my sample use diamonds to finance their operations, and seven use other gemstones as funding sources. Table 5 shows the results of the high-cost ceasefire estimation. Similar to the baseline specification, I find a significant negative relationship between increases in portfolio returns and the probability of high-cost ceasefires. The effects are similar to the baseline specification. One has to be careful with the interpretation of the effect size of the higher lags. Here, the sample size gets quite small, which can produce problems in the correction algorithm. Therefore, the effects resulting from small sample sizes of the later lags should to be interpreted with caution.

Table 5: A	Average partia	al effects of n	et return iı	ncrease on	high-cost	ceasefires (	excluding	diamonds
and gems	)							

	High-cost ceasefire (with compliance)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	$-0.1040^{***}$ (0.0161)						
Lag 1 Return	× ,	$-0.1079^{***}$ (0.0266)					
Lag 2 Return		· · · ·	$-0.5004^{**}$ (0.1943)				
Lag 3 Return			(011010)	-0.4335 (0.2674)			
Lag 4 Return				(0.2011)	$-0.6959^{***}$		
Lag 5 Return					(0.0032)	$-1.2599^{***}$	
Lag 6 Return						(0.0037)	$-5.9312^{***}$ (0.0090)
Observations	181	169	162	157	133	116	109
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	134.24	123.58	117.44	114.73	105.46	91.68	85.06
Null Deviance	238.57	223.22	214.60	208.85	178.86	158.01	149.04

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a probit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Here, rebel groups using only diamonds and gems are excluded from the regression. If they use additionally other commodities, the net return for them is calculated as before, not including diamonds and gems in the aggregated net return. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

A related concern is that rebel groups controlling the supply of rare earths might, in turn, influence the world market prices, making my constructed return shock lose exogeneity. As argued in assumption (i) in Section 4, for most commodities in my sample, that is highly unlikely, but it could be the case for cobalt. The largest cobalt deposits worldwide are located in the Democratic Republic of Congo, where the Alliance des Forces Démocratiques pour la Libération du Congo (AFDL) used them to fund their operations. I test whether excluding cobalt from my sample changes the results. The AFDL uses other natural resources besides cobalt and is therefore not dropped from the sample. Table 6 shows the estimation results with high-cost ceasefires as the dependent variable. The results are nearly identical to the baseline specification, and the effect sizes are comparable. Both estimations are repeated with low-cost ceasefires as the dependent variable in Appendix C.3. The baseline results remain stable.

Table 6: Average partial effects of net return increase on high-cost ceasefires (excluding cobalt)

	High-cost ceasefire (with compliance)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	$-0.0911^{***}$ (0.0145)						
Lag 1 Return	× ,	$-0.1170^{***}$ (0.0269)					
Lag 2 Return		. ,	$-0.2527^{*}$ (0.1455)				
Lag 3 Return			. ,	$-0.2933^{*}$ (0.1573)			
Lag 4 Return				. ,	$-0.2693^{*}$ (0.1390)		
Lag 5 Return					、 <i>,</i> ,	$-0.4520^{***}$ (0.0453)	
Lag 6 Return						× ,	$-0.2735^{***}$ (0.0908)
Observations	219	205	197	190	164	144	137
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	166.42	157.44	151.46	147.94	139.58	125.75	119.67
Null Deviance	277.39	260.51	251.25	244.12	213.10	190.53	181.89

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a probit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Here, rebel groups using only cobalt are excluded from the regression. If they use additionally other commodities, the net return for them is calculated as before, not including cobalt in the aggregated net return. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

### 6.2 Government & portfolio shocks

One crucial concern raised in Section 4 is that the government might be as well affected by the resource shock. This could affect the conflict trajectory and the likelihood of a ceasefire being announced. To test this assumption, I use three different strategies. First, I examine whether the return shocks can predict government revenue. If the government uses the same natural resources as the rebel group and receives relevant income from the resources directly or from the companies involved in resource extraction, the government's revenue would react to price changes. To test this, I use the Government Revenue Dataset (GRD) (GRD, 2023), which offers yearly data on revenues of governments for around 196 countries from 1980 - 2022. Second, I test whether the return shocks directly affect the government's resource revenues. The GRD allows for this more granular distinction, albeit with fewer observations for the resource revenues. Third, I test whether tax income stemming from natural resources is affected by the return shocks of the rebels. Tax income data also stems from the GRD dataset. Natural resource tax income consists primarily of corporate taxation of resource extraction, whereas resource revenues include non-tax-based income.

Table 7:	Effect of	of yearly	net returns on government revenues (	% of	GDP)
		•/			

Dependent Variable:	C	ov. revenue	es in % GD	Р
Model:	(1)	(2)	(3)	(4)
Variables				
Lag 0 Return	1.821			
	(1.198)			
Lag 1 Return		0.3154		
T OD I		(0.6218)	0.4000	
Lag 2 Return			-0.4823	
Log 2 Dotum			(0.4574)	0 5002
Lag 5 Return				(1.106)
				(1.100)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
State ID	Yes	Yes	Yes	Yes
Fit statistics				
Observations	182	164	148	129
$\mathbb{R}^2$	0.945	0.937	0.940	0.937
Within $\mathbb{R}^2$	0.042	0.002	0.005	0.003

Notes: This table presents the results of estimating government revenues in percentage of GDP using yearly net returns. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government revenue data stems from the GRD dataset. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

The GRD data is only available yearly. Therefore, I aggregate the returns yearly to measure yearly returns compared to the year of conflict onset. Some governments are fighting with more than one rebel group simultaneously. In that case, I aggregate the returns on year and state level, leaving me with 30 individual states. I estimate the model using an OLS regression, including year and state fixed effects. All revenue and tax data are measured in percentages of GDP. Like the baseline estimations, I also look at lags to allow transmission time.

Table 7 shows the results of government revenues in % of GDP. I do not find any evidence that the government is affected by the portfolio shocks of the rebel groups. The same is true for estimations of resource revenues in % of GDP in Table 8 and for tax income stemming directly from resource extraction in % of GDP in Table 9. All three estimations hint that the return shocks do not affect the government budget, therefore confirming my assumption.

Dependent Variable:	Gov. rev	venues from	n resources	in % GDP
Model:	(1)	(2)	(3)	(4)
Variables				
Lag 0 Return	1.001			
	(1.489)			
Lag 1 Beturn	(11100)	-0.1646		
ng i noran		(1.101)		
Lag 2 Beturn		(11101)	-0.3832	
			(0.6824)	
Lag 3 Beturn			(0.002-)	0.2858
hag o horan				(0.7044)
				(0.1.0.1.)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
State ID	Yes	Yes	Yes	Yes
Fit statistics				
Observations	124	114	105	94
$\mathbb{R}^2$	0.973	0.971	0.973	0.975
Within $\mathbb{R}^2$	0.011	0.001	0.004	0.003

Table 8: Effect of yearly net returns on government revenues from resources (% of GDP)

Notes: This table presents the results of estimating government resource revenues in percentage of GDP using yearly net returns. Government resource revenues refers to all revenues specifically acquired from natural resources including tax and non-tax revenues. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government resource revenue data stems from the GRD dataset. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Table 9: Effect of yearly net returns on government tax income from resources (% of GDP)

Dependent Variable:	Gov. ta	axes from re	sources in	%  GDP
Model:	(1)	(2)	(3)	(4)
Variables				
Lag 0 Return	-0.0831			
Lag o Return	(0.1120)			
Lag 1 Return	(0.1120)	-0.0437		
hag i ftertain		(0.0631)		
Lag 2 Return		(0.0001)	-0.0282	
Eag - Hottain			(0.0862)	
Lag 3 Return			()	-0.0208
0				(0.0417)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
State ID	Yes	Yes	Yes	Yes
$Fit \ statistics$				
Observations	197	177	157	139
$\mathbb{R}^2$	0.957	0.952	0.955	0.960
Within $\mathbb{R}^2$	0.000	0.000	0.000	0.000

Notes: This table presents the results of estimating government tax income form natural resources in percentage of GDP using yearly net returns. Government tax income form natural resources refers to all tax income specifically acquired from natural resources excluding non-tax revenues. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government resource revenue data stems from the GRD dataset. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Additionally to the tests above, I show estimations using the total revenue or total tax income in Appendix C.6.2. Further, I test the assumption using the Global Revenue Statistics Database of the OECD (OECD, 2023) providing revenue data for 127 economies from 1990 onward. I can estimate whether shocks affect the total revenue in US Dollars or local currency. The data coverage is less broad than in the GRD Dataset. Further, I use the Military Expenditure Database from the Stockholm International Peace Research Institute (SIPRI, Stockholm International Peace Research Institute, 2024) as a proxy for conflict-relevant expenses by the government. If the government is also affected by the increase in commodity prices, it will only be relevant for the conflict trajectory if it increases its military capacities. Estimations can be found in Appendix C.6.1. All estimations align with the assumption that the government is not or not meaningfully affected by the return shock of the rebel group.

#### 6.3 Light ceasefires

Instead of looking only at "formalized"<sup>4</sup> ceasefires, I can look at the most straightforward form of a ceasefire, which is just stopping to fight. I code every break in fighting, where at least three months no fatalities occur, and where there is no more "formal" ceasefire already in place, as a "light" ceasefire. I estimate the same model as in equation 3 only using the dummy for light ceasefires. The results are displayed in Table 10. I find a significant negative effect of increasing returns on "light" ceasefires. A 100% return increase is associated with a 2 to 3.5 percentage points decrease in the probability of a "light" ceasefire. This effect suggests that rebel groups keep conflict active after a return shock, which aligns with my theory. They are less willing to enter a high-cost ceasefire but are also less willing to stop fighting. Together with my results on the number of attacks and fatalities, this shows that rebel groups continue with low-intensity conflict to take advantage of the economic opportunities during wartime.

#### 6.4 Price volatility

The average price of the first 12 months after conflict onset could correlate with conflict onset itself and introduce endogeneity into my return measure. To ensure this is not the driving factor for my results, I compute the returns using the 60 months (or five years) before conflict as the base price. I can only include natural resources, where I observe the prices five years before the conflict started. That is not the case for all resources; diamonds, gems, cobalt, and wolframite are only observed later. I calculate the return in the same way described in Section

<sup>&</sup>lt;sup>4</sup>Ceasefires form the ETH/PRIO Civil Conflict Ceasefire Dataset also include oral agreements. They are not necessarily formalized in a classical sense.

	No Fight ceasefires							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Lag 0 Return	$-0.0267^{**}$ (0.0113)							
Lag 1 Return		$-0.0297^{***}$ (0.0115)						
Lag 2 Return		()	$-0.0353^{***}$ (0.0104)					
Lag 3 Return			(0.0101)	$-0.0320^{***}$				
Lag 4 Return				(0.0100)	$-0.0268^{***}$			
Lag 5 Return					(0.0104)	$-0.0231^{**}$		
Lag 6 Return						(0.0113)	-0.0188 (0.0120)	
Observations	476	457	435	435	427	427	411	
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Residual Deviance	363.89	347.45	334.72	335.16	331.30	331.82	322.26	
Null Deviance	446.70	427.01	412.23	412.23	406.01	406.01	393.54	

Table 10: Average partial effects of net return increase on "light" ceasefires

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a "light" ceasefire dummy using a probit regression. The "light" ceasefire dummy equals one if a break of fighting with three consecutive months of no fatalities occurred and no other formal type of ceasefire was declared. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

3.2, only using the average price over the 60 months before the conflict starts as the base price. I only calculate base prices, where I have price data for at least half of the extended time frame. The results are shown in Table 11. I find a significant negative relationship between increasing returns and the probability of a high-cost ceasefire. The magnitude of the effects is a bit reduced compared to the baseline specification but still sizable. The results for low-cost ceasefire can be found in Appendix C.9.

### 6.5 Excluding Ukraine

Additionally, I want to rule out that countries or dyads with an exceptionally high count of high-cost ceasefires drive the effects. Figure B.1 shows that many high-cost ceasefire months are concentrated in Ukraine. To be more precise, the Donetsk People's Republic (DPR) and the Luhansk People's Republic (LPR) each had 15 high-cost ceasefires during their conflict with the Ukrainian Government. In Table 12, I estimate the baseline specification but exclude both the dyad involving the DPR and the LPR. I still find a negative significant effect between return increases and the probability of a high-cost ceasefire. The parameters are not precisely estimated in the lags 2-4, but the magnitude and direction of the effects is still similar to the baseline estimation. The effect sizes are slightly smaller but comparable to the baseline, suggesting that

	High-cost ceasefire (with compliance)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	-0.0028 (0.0065)						
Lag 1 Return	· · · ·	0.0037 (0.0065)					
Lag 2 Return		· · · ·	$-0.0303^{**}$ (0.0142)				
Lag 3 Return			(010111)	$-0.0423^{**}$ (0.0185)			
Lag 4 Return				(0.0100)	$-0.0995^{***}$		
Lag 5 Return					(0.0314)	$-0.1252^{***}$	
Lag 6 Return						(0.0500)	$-0.1233^{***}$ (0.0307)
Observations	215	215	213	211	209	207	204
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	174.28	173.55	163.01	157.83	155.74	152.38	151.03
Null Deviance	279.32	279.32	277.56	275.79	273.99	271.06	267.23

Table 11: Average partial effects of net return increase on high-cost ceasefires (base-price avg. 5 years before conflict)

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a probit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Here, the base-price for calculating the monthly net returns is calculated as the average over the five years before conflict start. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Table 12: Average partial effects of net return increase on high-cost ceasefires (excluding Ukraine)

	High-cost ceasefire (with compliance)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	$-0.0791^{***}$ (0.0203)						
Lag 1 Return	× ,	$-0.1524^{**}$ (0.0642)					
Lag 2 Return		~ /	-0.2245 (0.1873)				
Lag 3 Return			· · · ·	-0.2348 (0.1603)			
Lag 4 Return				(0.1000)	-0.2215 (0.1453)		
Lag 5 Return					(012-000)	$-0.2883^{*}$	
Lag 6 Return						(0.1010)	$-0.2231^{**}$ (0.0986)
Observations	161	147	139	132	107	90	83
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	104.53	93.76	87.58	84.67	77.69	66.04	59.27
Null Deviance	186.87	170.09	161.08	154.69	126.97	109.96	101.57

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a probit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Here, the rebel groups Donetsk People's Republic (DPR) and Luhansk People's Republic (LPR) fighting against the Ukrainian government are excluded. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

the LPR and DPR do not drive my results. In Section C.8, I exclude the three dyads with the highest count of high-cost ceasefires, LPR, DPR, and Syrian insurgents fighting against the Government of Syria. The results remain stable. The estimation for low-cost ceasefires can be found in Table C.19 in Appendix C.7. The results are robust to the exclusion.

## 7 Mechanism & heterogeneity analysis

In the baseline analysis, I only observe whether a ceasefire was declared in the respective month. I have no information about when negotiations started or if rebel groups delayed or canceled talks in response to being better off economically. Using the African Peace Processes (APP) dataset (Duursma and Gamez, 2023), I can shed some light on the negotiation dynamics. The APP dataset covers peacemaking efforts in African armed conflicts from 1989 to 2019. The dataset includes the start of negotiations, rounds of ceasefires, and peace agreements, whether successful or not. Following my theory, positive return shocks reduce the probability of entering high-cost ceasefires. High-cost ceasefires need more intensive negotiations, or at all negotiations, compared to low-cost ceasefires. The return shocks must, therefore, also affect the likelihood of starting negotiations because groups are less likely to exert effort and resources for conflict resolution if they have no goal to reach conflict resolution. One exception might be if negations are used as a distraction. However, low-cost ceasefires would be a more effective signal, additionally halting violence.

Table 13 shows the results of the APP estimation. I use a dummy to determine whether negotiations started in the respective month as the outcome variable. The sample is restricted to African dyads by construction. The econometric specification is the same as in Equation 3 using a probit model, only with the changed outcome. An increase in the rebel portfolio return by 100% would decrease the likelihood of starting negotiations by between 37.6 and 16.33 percentage points on average. This result confirms that rebel groups are less likely to enter conflict resolution in good economic times.

My theory suggests that rebel groups are reluctant to end conflicts because they profit from the economic opportunities of war, especially if they have high returns. This idea follows the anecdotal evidence of the ADF group in Section 1, which abandoned their revolutionary goals for profit-seeking. Generalizing on this finding, groups experiencing extremely high returns should be more abstaining from active conflict, compared to groups with moderate returns. Reeder *et al.* (2022) show a similar idea in their paper. They calculate a sharp ratio measuring the efficiency and profit of rebel portfolios using the yearly RCD data. Based on the value of the sharp ratio,

	Start of negotiations							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Lag 0 Return	$-0.3760^{***}$ (0.0812)							
Lag 1 Return		$-0.2780^{***}$ (0.0893)						
Lag 2 Return		· · ·	$-0.1887^{**}$ (0.0877)					
Lag 3 Return			(010011)	$-0.1633^{*}$				
Lag 4 Return				(0.0000)	$-0.2346^{***}$			
Lag 5 Return					(0.0000)	$-0.1944^{**}$		
Lag 6 Return						(0.0004)	$-0.1867^{**}$ (0.0945)	
Observations	207	193	192	192	190	189	188	
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Residual Deviance	191.89	186.65	188.66	189.39	185.93	185.06	184.10	
Null Deviance	252.73	232.48	231.80	231.80	230.41	229.71	227.26	

Table 13: Average partial effects of net return increase on start of African peace negotiations

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a peace negotiation dummy using a probit regression. The peace negotiation dummy equals one if, in that month, peace negotiations of the specific dyad started. The outcome of the peace negotiations is not considered. All negotiations are related to African dyads, all other dyads are excluded due to data availability. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

rebel behavior is divided into three phases: vulnerability, emboldened, and exploitation. In the vulnerability phase, the group has little resources and can hardly engage in active fights. During the emboldened phase, the rebel budget is more settled. They have moderate earnings to finance their operation and actively engage in fighting. Once they reach high earnings and an efficient portfolio, they enter the exploitation phase, where the goals of the rebellion become unimportant, and the focus shifts to profit maximization. To confirm my theory and the results of Reeder *et al.* (2022) with more granular data, I divide the return shocks into four groups and regress them on fatalities. High-cost ceasefires happen too rarely to use them in this exercise. Fatalities are, however, a good proxy for the conflict intensity. Fatalities include deaths from government, rebel, and civilian sides.

I classify returns below -0.3 as negative returns and use them as the base category. Between -0.3-0.3, returns are labeled as moderate returns. Returns from 0.3-0.9 are counted as positive returns, and returns exceeding 0.9 are classified as high positive returns. The classes correspond to the 10, 55, and 88 percentiles. I estimate Equation 3 only using fatalities as an outcome and the three dummies as the return shocks. Table 14 displays the results of this estimation. The results show that comparing a group with negative returns to a group with moderate returns will significantly increase the intensity level of conflict. However, compared to negative

Dependent Variable:	()	(-)	T	otal Fatalitie	es	(-)	(-)
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables	0.9000**						
Lag 0 Moderate Return	(0.1440)						
Lag 0 Positive Return	-0.7687**						
Lag 0 High Positive Return	(0.3814) -1.030*** (0.3412)						
Lag 1 Moderate Return	(0.0412)	0.1690 (0.1355)					
Lag 1 Positive Return		$-0.6932^{**}$ (0.3525)					
Lag 1 High Positive Return		(0.3020) $-1.100^{***}$ (0.3038)					
Lag 2 Moderate Return		()	$0.2685^{*}$				
Lag 2 Positive Return			(0.1468) -0.5192 (0.4202)				
Lag 2 High Positive Return			(0.4202) - $0.7553^{**}$ (0.3438)				
Lag 3 Moderate Return			(0.3438)	0.2421			
Lag 3 Positive Return				(0.1434) -0.3868 (0.3504)			
Lag 3 High Positive Return				(0.3304) $-0.6698^{**}$ (0.3054)			
Lag 4 Moderate Return				(0.0001)	0.0922 (0.1553)		
Lag 4 Positive Return					(0.1003) -0.6029 (0.4019)		
Lag 4 High Positive Return					$-0.8488^{**}$ (0.3313)		
Lag 5 Moderate Return					(0.0010)	0.0770	
Lag 5 Positive Return						(0.1698) -0.5559 (0.2011)	
Lag 5 High Positive Return						(0.3911) $-0.7739^{**}$ (0.2270)	
Lag 6 Moderate Return						(0.3219)	0.1006
Lag 6 Positive Return							(0.1910) -0.5347 (0.2004)
Lag 6 High Positive Return							(0.3994) $-0.6655^{*}$ (0.3442)
Fixed-effects	X	X	N	X	V	37	
Year-month Dyad ID	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes
Fit statistics							
Observations Pseudo $\mathbb{R}^2$	3,572 0.86838	3,536 0.86492	3,502 0 86344	$3,466 \\ 0.86269$	3,432 0.86275	3,396 0.86118	3,362 0.85983

Table 14: Nonlinear effect of net returns on monthly fatalities: Poisson regression

Notes: This table presents the results of a Poisson regression of the dyad-specific monthly net returns on monthly fatalities. Here, returns are grouped in bins: < -0.3 negative returns, -0.3 - 0.3 moderate returns, 0.3 - 0.9 positive returns and > 0.9 high positive returns. The lowest bin (< -0.03 negative returns) is used as base category. Monthly fatalities refers to the best estimate of total fatalities in the UCDP data. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

returns, groups experiencing positive or even high positive returns reduce the conflict intensity dramatically. The effect is especially pronounced for high positive returns. A rebel group experiencing moderate returns increases fatalities in this conflict month by 33.6 %, compared to negative returns in the same month. Positive returns lead to a decrease of 53.7%, and high positive returns lead to a reduction of 64.3%. The results show fatalities or conflict intensity follow an inverted u-shape, confirming the results of Reeder *et al.* (2022), using direct returns and not a sharp ratio. Table C.24 in Appendix C.11 shows the estimation using the number of attacks as the outcome. The patterns are similar.

This estimation confirms my theory. Rebels with high returns are not interested in conflict resolution. Instead, they reduce conflict intensity and focus on profit maximization and economic gains. Moderate returns lead to increased conflict intensity.

## 8 Policy implications

From the evidence of this paper and earlier research, it becomes clear that rebel groups are heavily economically motivated. As I showed in this paper, rebel groups are particularly likely to enter a ceasefire or start negotiation if their economic position is unfavorable. If random commodity price shocks can achieve this change of willingness to bargain, then targeted economic sanctions can correspondingly. Focusing solely on rebel groups' funding streams is less harmful than general sanction and embargo regimes (see Rohner, 2024). Policies extending transparency and traceability of resources address precisely this problem. As shown by Binzel et al. (2023), introducing a certification scheme for diamonds (the Kimberley Process Certification Scheme) substantially decreased conflict, suggesting that funding of rebel groups was affected. Further Berman et al. (2017) show how corporate social responsibility, meaning spreading transparency among mining companies and avoiding conflict resources, decreases violence in the origin countries. More targeted economic sanctions aimed explicitly at the funding sources of rebels are helpful in draining rebel budgets and create valuable moments to start actual negotiations and mediation opportunities to end the violent conflict. High-cost ceasefires, as well as negotiations, become more probable when rebel funding is low. Reducing funding sources can create possibilities for entering the long path of conflict resolution toward peace.

## 9 Conclusion

This paper analyzes how budget shocks and rebel groups' economic conditions affect the trajectory of conflict. I build a novel dataset combining conflict data from UCDP, ceasefire data from the ETH/PRIO Civil Conflict Ceasefire Dataset, resource usage data from the Rebel Contraband Dataset, and world market prices on natural resources. I show that favorable economic conditions for rebels hamper a swift conflict resolution and decrease the probability of a highcost ceasefire (ceasefires with a compliance mechanism), a first serious step towards sustainable peace.

Rebel groups use war for their economic profit and make use of economic opportunities that are not available to them during peace times. Increasing the financial resources of a rebel group makes them less likely to end the conflict swiftly. Further, I show that increasing portfolio returns slow down the conflict by decreasing monthly fatalities and attacks. However, rebel groups do not stop fighting and maintain an ongoing low-level conflict so as not to lose access to their economic possibilities. This pattern follows an inverted u-shape depending on the height of the return shock. Rebel groups experiencing moderate returns increase the conflict intensity compared to low returns. Groups realizing high or very high returns decrease the intensity significantly. I validate the ceasefire results using peace negotiations data from Africa. Similar to the ceasefire results, rebel groups with increasing returns are less likely to engage in peace negotiations. These findings suggest that the economic dimension is crucial to conflict resolution. Targeted economic sanctions aimed explicitly at the funding sources of rebels might help drain rebel budgets and create valuable moments to start actual negotiations. Since random changes in commodity prices have resulted in a shift in willingness to enter negotiations and ceasefires, economic sanctions can do the same. Such aimed sanctions are less harmful to the general population, and their effectiveness has been scientifically proven. For example, the Kimberly Process restricting the diamond trade significantly reduced violence in Angola. Other initiatives by mining companies implementing transparency and avoiding conflict resources have reduced violence in origin countries. High-cost ceasefires and negotiations become more probable when rebel funding is low, creating possibilities for entering the long path of conflict resolution. Admittedly, these economic sanctions are substantially easier to enforce for diamonds than other less niche commodities. However, transparency initiatives can help reduce the trade of conflict resources and support upholding those sanctions. More research is needed to understand better the driving dynamics of conflict resolution and in which settings economic incentives for conflict termination can be used in different ways to create sustaining peace.
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# Appendices

# A Appendix: Dataset construction

The dataset is mainly based on the UCDP Georeferenced Event Dataset (GED) version 23.1 (Sundberg and Melander (2013), Davies *et al.* (2024)). Conflict dyads enter the UCDP dataset if they have at least 25 conflict fatalities in a given year. Incidents enter the GED dataset if they have at least one conflict fatality. I only select active conflict dyads between one state actor and one non-state actor, and I restrict the sample to 1990-2020. I create a grid-like structure from the start month of the conflict to the last entry in the UCDP Event Dataset data based on each dyad and match it with the monthly aggregated fatalities. As described in section 3.1, I drop conflict months with zero fatalities if the two months before were also without fatalities.

The ceasefire data stems from the ETH/PRIO Civil Conflict Ceasefire Dataset (Clayton *et al.*, 2023b). The data set includes all ceasefires in civil conflicts between 1989 and 2020, with types ranging from verbal to detailed written agreements, with different levels of compliance and disarmament, demobilization, and reintegration mechanisms. Agreements can be unilateral, bilateral, or multilateral. The data includes one row for each participant of the agreement. I aggregate the data monthly for all dyads, given that the rebel side was part of the agreement, distinguishing the different types of ceasefires. I match the aggregated ceasefire data with the UCDP data. As described in section 3.1, if a ceasefire is followed by zero fatalities in the next month, I assume the ceasefire did hold and drop all month with zero fatalities following the ceasefire. I do not distinguish in this case by ceasefire type or how long the ceasefire did hold.

I combine several data sources to calculate the return shocks for the rebel portfolio. Information on resource usage by rebel groups comes from the Rebel Contraband Dataset (RCD) (Walsh *et al.*, 2018). The dataset measures what kind of natural resources rebel groups exploit and by which means they are appropriated. It further includes crimes used, unrelated to natural resources, to fund their operations. The dataset provides dyad-year observations from 1990 to 2015, based on the UCDP Dyadic Dataset. It also includes a rough indicator of whether more than \$5 million was generated from a specific resource-extraction type combination, but this variable is often missing. I aggregate the RCD data on the dyad year level, irrespective of which extraction type was used. As described in section 4, I only consider the resources used in the first year of conflict start of each dyad to avoid potential endogeneity. This means I can only include dyads that use natural and non-illicit resources in the first year of conflict. Given that I do not change the portfolio composition of a rebel group in a dyad, I can observe the conflict until the maximum of the ceasefire data (2020) if the conflict started in 2015, at the latest. The resources rebels use in my dataset are cassiterite, coal, cobalt, cocoa, coffee, diamonds, gems, iron, oil, gold, tea, timber, tin, and wolframite. The price data for most of the commodities comes from World Bank "Pink Sheet" Data (World Bank, 2021). The coal price is averaged over the Australian and the South African market prices, coffee prices are averaged over the two types of arabica and robusta, and timber prices are the average log prices of the Cambodia and Myanmar markets. Since monthly cassiterite prices are not available, I use tin prices as a proxy for cassiterite prices. Tin is extracted from cassiterite, making it effectively a precursor of tin. Therefore, I assume prices move very closely to each other. Diamond prices directly are not available on a monthly basis, I use the import price index of diamonds from Federal Reserve Economic Data (U.S. Bureau of Labor Statistics, 2024). The data does not distinguish between gems and diamonds, and separate gem prices are unavailable. Assuming that both prices are very similar, I use the diamond index for diamonds and gems. Missing values in the diamond price series are imputed from the months before. Data on cobalt and wolframite prices stem from Bloomberg (Bloomberg, 2024).

The return shocks are calculated by combining the UCDP grid, the RCD data, and the world market prices from different sources. For each specific dyad commodity, I calculate a base price using the first 12 months after conflict onset. I calculate monthly returns in relation to that base price (see equation 1). Depending on whether the rebel group uses only one commodity or multiple commodities, I aggregate the returns over all resources used (see equation 2). Suppose that information is available whether the group acquired more than \$5 million from a particular resource or a specific set of resources. Then, I only aggregate these high-yielding resources for the return shock. If no information is available, I take the mean return over all commodities used. In both cases, I cannot weigh the mean by relative commodity usage because such detailed information is simply unavailable. Therefore, I can only use a simple mean over the different returns. This dyad-specific return is matched to the UCDP conflict data, including fatalities and ceasefires.

Only dyads found in the three central datasets (UCDP, RCD, ETH/Prio Ceasefire dataset) can be included in the final dataset. Table A.1 shows all rebel dyad combinations included in the final dataset.

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Table A.1: All government-rebel dyads in sample

# **B** Appendix: Additional descriptives

The map in Figure B.1 shows the spatial distribution of high-cost ceasefires. Fifteen countries experienced at least one high-cost ceasefire during their conflict. Of the other 15 countries, six experienced only low-cost ceasefires. The remaining nine countries have had no ceasefire during their conflict. High-cost ceasefires are not distributed equally across countries. The country that sticks out on the map is Ukraine. Here, the Donetsk People's Republic (DPR) and the Lugansk People's Republic (LPR) declared 15 high-cost ceasefires each. The distribution of the high-cost ceasefires is a bit more even in other countries, albeit Syria and Bosnia-Herzegovina have many high-cost ceasefires months.<sup>1</sup>

Figure B.1: Map of conflict countries & amount of month with high-cost ceasefires declared



Notes: The figure shows the location based on the state acting as the counter-party of the rebel group. The graph is based on the UCDP Georeferenced Event Database. "# High-cost CF" refers to the number of months in which high-cost ceasefires were declared.

Some groups use more resources. Figure B.7 shows the distribution of commodities used for funding. Most rebel groups rely only on one resource, 44 out of 66. Most other groups use between two and four different commodities. Only the group Forces Démocratiques de Libération du Rwanda - Forces Combattants Abacunguzi (FDLR-FOCA) is at the extreme using seven different commodities to finance their operations. The mean number of resources is 1.62 with a median of 1. This graph only covers resources used in the year of conflict onset and excludes illegal commodities. Additionally, groups can finance themselves by crimes unrelated to natural resources. Figure B.8 shows the distribution of crime intensity.

 $<sup>^{1}</sup>$ The dyads with a large amount of high-cost cease fires do not drive my results. Section 6.5 and section C.8 show some robustness checks.



Figure B.2: Map of conflict countries & amount of month with low-cost ceasefires declared

Notes: The figure shows the location based on the state acting as the counter-party of the rebel group. The graph is based on UCDP Georeferenced Event Database."# Low-cost CF" refers to the number of months in which low-cost ceasefires were declared.





Notes: The figure shows the location based on the state acting as the counter-party of the rebel group. The graph is based on UCDP Georeferenced Event Database. "# Total CF" refers to the number of months in which a ceasefires were declared.



Figure B.4: Realized monthly net returns in comparison to dyad specific base-year

Notes: The graph shows monthly net returns realized by each of the 66 dyads. Net returns are calculated in comparison to the start year of the conflict; therefore, the first observation is 01-1991.



Figure B.5: Map of most used resources for rebel funding across dyads

Notes: The figure shows the most used resource for rebel funding by counter-party state. Most here means that the majority of rebel groups in conflict with this specific counter-party state use the resource. In many countries, rebel groups use more than one resource. In some countries, the highest usage was the same for more than one resource; those are labeled with "multiple." Resources are based on the resources used in the first year of conflict onset.



Figure B.6: Distribution of monthly net returns across rebel groups and dyads by ceasefire type

Notes: The graph shows the distribution of monthly returns by type of ceasefire. The types of ceasefires are mutually exclusive here. A dyad containing a high-cost ceasefire ("High-cost CF") only belongs to the high-cost ceasefire category, even if there occur some low-cost ceasefires ("Low-cost CF") as well in the dyad. Dyads in the low-cost ceasefire only experienced low-cost ceasefires. Dyads in the no ceasefire category ("No CF") had no ceasefire of any type throughout their conflict.

		Ceasefi	re type	
_	Overall	Low-cost ceasefires	High-cost ceasefires	No ceasefire in conflict
Conflict length in me	onths			
mean length	63.94	88.89	63.09	37.07
median lenght	42.50	65.00	37.00	19.00
Number of resources				
mean resources	1.73	1.81	1.86	1.67
median resources	1.00	1.00	1.00	1.00
Net returns				
mean return	0.24	0.23	0.27	0.25
median return	0.02	0.01	0.02	0.05
max return	3.75	3.75	3.75	3.36
min return	-0.80	-0.80	-0.80	-0.71
Monhtly conflict dea	$\mathbf{ths}$			
mean deaths	127.45	160.17	246.95	24.81
mean deaths gov	32.58	41.39	64.52	5.02
mean deaths rebel	45.17	54.79	76.15	15.26
mean deaths civil	31.25	40.51	76.61	2.16

Table B.1: Summary statistics and distribution of conflict dyads

Notes: The table shows descriptive statistics for conflict dyads that experienced the ceasefire type in at least one conflict month. If a conflict dyad experienced both a low-cost ceasefire and a high-cost ceasefire, it is included in both columns.



Figure B.7: Amount of resources used by rebel groups across conflict dyads

Notes: The graph shows the number of resources used across dyads. The number of resources is based on the first year of conflict start of the respective group



Figure B.8: Amount of crime used by rebel groups across conflict dyads

Notes: The graph shows the number of crimes used across dyads. Crimes here mean crimes not directly connected to natural resources, such as human trafficking and smuggling of non-resource-based goods for example. Rebel groups that commit zero crimes here still commit crimes to gain control of natural resources, such as theft, extortion, or similar. I do not distinguish here by the severity of the crimes committed. The number of crimes is based on the first year of conflict start of the respective group

	Lag 0 Return	Lag 1 Return	Lag 2 Return	Lag 3 Return	Lag 4 Return	Lag 5 Return	Lag 6 Return
Lag 0 Return	1.000	-	-	-	-	-	-
Lag 1 Return	0.994	1.000	-	-	-	-	-
Lag 2 Return	0.985	0.995	1.000	-	-	-	-
Lag 3 Return	0.975	0.986	0.995	1.000	-	-	-
Lag 4 Return	0.965	0.975	0.986	0.995	1.000	-	-
Lag 5 Return	0.956	0.965	0.975	0.985	0.995	1.000	-
Lag 6 Return	0.946	0.956	0.965	0.975	0.986	0.995	1.000

Table B.2: Correlation of the calculated net returns across lags 0-6

Notes: The table shows the correlation of the calculated monthly net return across lag 0-6. The net returns are highly correlated. The sample for this calculation includes all rebel groups and monthly net returns.

# C Appendix: Additional estimations

# C.1 Overall ceasefire & number of attacks estimations

Table C.1 shows the results for the probit model using ceasefires overall as the dependent variable. If there was any ceasefire in that particular month, the dummy turns 1. The results are very similar to the ones presented in table 2 and show no effect of the return shock on ceasefires in general. It is not surprising, given that most ceasefires do not use a compliance mechanism. Therefore, the low-cost ceasefires might overshadow the negative effect of high-cost ceasefires here.

Table C.2 shows the results of a Poisson regression of the return on the number of attacks in the respective month. Similarly to the results on fatalities in table 4, I find a significant negative relationship, suggesting a slowdown of the conflict after a positive return shock.

	All ceasefires									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)			
Lag 0 Return	-0.0132 (0.0095)									
Lag 1 Return	· · · ·	-0.0076 (0.0094)								
Lag 2 Return			-0.0024 (0.0093)							
Lag 3 Return			· · /	-0.0006 $(0.0098)$						
Lag 4 Return					0.0007 (0.0097)					
Lag 5 Return					(0.000)	0.0007 (0.0087)				
Lag 6 Return						(0.000)	-0.0002 (0.0089)			
Observations	1077	1031	997	957	943	894	865			
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$			
Dyad FE	<b>√</b>	<b>√</b>	<b>√</b>	<b>√</b>	√ 	√ 	✓ 			
Residual Deviance	669.11	646.59	630.85	606.98	595.72	568.86	553.89			
Null Deviance	1039.66	1000.01	971.18	934.01	913.61	881.25	854.49			

#### Table C.1: Average partial effects of net return increase on ceasefires overall

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a ceasefire dummy using a probit regression. The ceasefire dummy equals one if, in that month, any type of ceasefire was declared. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

#### Table C.2: Effects of net return increase on number of attacks: Poisson regression

Dependent Variable:			Ň	umber of Att	acks		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables							
Lag 0 Return	$-1.029^{***}$ (0.2853)						
Lag 1 Return	()	$-1.003^{***}$ (0.2705)					
Lag 2 Return		()	$-0.9597^{***}$ (0.2568)				
Lag 3 Return			· · · · ·	$-0.8951^{***}$ (0.2492)			
Lag 4 Return				~ /	$-0.8281^{***}$ (0.2495)		
Lag 5 Return					~ /	$-0.7768^{***}$ (0.2535)	
Lag 6 Return						( )	$-0.7561^{***}$ (0.2549)
Fixed-effects							
Year-month FEy	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Tes	Tes	Tes	Tes	Tes	Tes	Tes
Fit statistics	2 570	9 590	2 500	9.400	9 499	2 200	2 2 6 0
Squared Correlation	3,372 0.98447	3,330 0 08407	3,202 0.98553	3,400 0.98664	3,432 0.98725	3,390 0.98740	3,302 0.98724
Pseudo $\mathbb{R}^2$	0.92646	0.92619	0.93555 0.92569	0.92547	0.92533	0.92489	0.92416
BIC	36,591.0	36,083.4	35,419.3	34,754.6	33,959.5	33,322.4	32,824.4

Notes: This table presents the results of a Poisson regression of the dyad-specific monthly net returns on number of attacks. Number of attacks refers to the total number of dyad related incidents recorded in a given month in the UCDP data. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

#### C.2 Baseline estimation with logit model

This section displays the estimation results of the key specification using a logit instead of a probit model. Table C.4 shows the results for low-cost ceasefires, and table C.3 displays the estimation results for high-cost ceasefires. The results are stable, and effect sizes are similar, albeit slightly smaller in magnitude.

			High-cost o	ceasefire (with o	$\operatorname{compliance})$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	$-0.0758^{***}$ (0.0148)						
Lag 1 Return		$-0.0693^{***}$ (0.0178)					
Lag 2 Return			$-0.1508^{**}$ (0.0601)				
Lag 3 Return			× ,	$-0.1746^{***}$ (0.0654)			
Lag 4 Return				, ,	$-0.1286^{***}$ (0.0348)		
Lag 5 Return					· · · ·	$-0.1879^{***}$ (0.0501)	
Lag 6 Return						· · · ·	$-0.0892^{***}$ (0.0147)
Observations	219	205	197	190	164	144	137
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	165.60	156.35	150.28	146.92	138.29	124.54	118.68
Null Deviance	277.39	260.51	251.25	244.12	213.10	190.53	181.89

Table C.3: Average partial effects of net return increase on high-cost ceasefires (logit model)

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a logit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

		Lo	w-cost ceasefire	e (without con	npliance)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	-0.0120 (0.0091)						
Lag 1 Return	( )	-0.0067 (0.0089)					
Lag 2 Return		(0.000)	-0.0001				
Lag 3 Return			(0.0000)	0.0024			
Lag 4 Return				(0.0055)	-0.0008		
Lag 5 Return					(0.0003)	-0.0019	
Lag 6 Return						(0.0079)	-0.0028 (0.0079)
Observations	858	811	782	714	700	671	646
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	542.43	519.16	503.46	475.69	464.77	443.17	431.19
Null Deviance	773.16	736.68	710.02	665.45	650.77	636.29	614.23

Table C.4: Average partial effects of net return increase on low-cost ceasefires (logit model)

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a low-cost ceasefire dummy using a logit regression. The low-cost ceasefire dummy equals one if, in that month, a low-cost ceasefire was declared. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

# C.3 Estimation low-cost ceasefires excluding diamonds, gems and cobalt

This section displays the robustness checks form Section 6.1 only using low-cost ceasefires as the dependent variable. The results for low-cost ceasefires are unchanged by the exclusion of diamonds, gems and cobalt.

Table C.5: Average partial effects of net return increase on low-cost ceasefires (excluding diamonds and gems)

		Lo	w-cost ceasefire	e (without con	npliance)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	-0.0076 (0.0082)						
Lag 1 Return	· · /	-0.0023 (0.0081)					
Lag 2 Return		· · · ·	0.0028 (0.0077)				
Lag 3 Return				0.0050 (0.0085)			
Lag 4 Return					0.0028 (0.0082)		
Lag 5 Return					,	0.0017 (0.0073)	
Lag 6 Return						· · ·	-0.0003 (0.0072)
Observations	679	641	615	591	578	559	535
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	470.65	451.47	436.82	423.54	412.72	394.91	383.99
Null Deviance	648.51	615.17	589.70	568.02	553.81	542.99	521.27

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a low-cost ceasefire dummy using a probit regression. The low-cost ceasefire dummy equals one if, in that month, a low-cost ceasefire was declared. Here, rebel groups using only diamonds and gems are excluded from the regression. If they use additionally other commodities, the net return for them is calculated as before, not including diamonds and gems in the aggregated net return. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

	Low-cost ceasefire (without compliance)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)		
Lag 0 Return	-0.0090 (0.0087)								
Lag 1 Return	· · · ·	-0.0040 (0.0086)							
Lag 2 Return		(0.0000)	0.0013 (0.0082)						
Lag 3 Return			(0.0002)	0.0035					
Lag 4 Return				(0.0000)	0.0012				
Lag 5 Return					(0.0000)	-0.0009			
Lag 6 Return						(0.0070)	-0.0019 (0.0076)		
Observations	858	811	782	714	700	671	646		
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$		
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$		
Residual Deviance	541.77	518.15	501.87	473.47	462.54	440.84	428.73		
Null Deviance	773.16	736.68	710.02	665.45	650.77	636.29	614.23		

Table C.6: Average partial effects of net return increase on low-cost ceasefires (excluding cobalt)

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a low-cost ceasefire dummy using a probit regression. The low-cost ceasefire dummy equals one if, in that month, a low-cost ceasefire was declared. Here, rebel groups using only cobalt are excluded from the regression. If they use additionally other commodities, the net return for them is calculated as before, not including cobalt in the aggregated net return. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

### C.4 Estimation excluding definitive ceasefires

Table C.7 shows the baseline results for high-cost ceasefires, excluding definitive ceasefires from the high-cost ceasefire group. This group only contains cessation of hostilities with a compliance mechanism such as monitoring or verification missions. Definitive ceasefires are usually part of a peace agreement. Therefore, they might be following a different data-generating process. The results are stable to the exclusion of definitive ceasefires. I cannot test the effect of return shocks on definitive ceasefires since they rarely occur in the data.

			High-cost c	easefire (with o	compliance)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	$-0.4891^{***}$ (0.0173)						
Lag 1 Return	× ,	$-0.3765^{***}$ (0.0482)					
Lag 2 Return		()	$-0.5658^{***}$ (0.0731)				
Lag 3 Return			(0.0101)	$-1.2352^{***}$			
Lag 4 Return				(0.0201)	$-0.5350^{***}$		
Lag 5 Return					(0.0310)	$-0.8505^{***}$	
Lag 6 Return						(0.0145)	$-0.6738^{***}$ (0.0134)
Observations	182	173	165	158	134	119	109
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
<b>Residual</b> Deviance	138.02	135.44	133.94	131.82	124.62	115.06	105.69
Null Deviance	234.79	223.33	213.97	206.62	177.04	159.68	147.78

Table C.7: Average partial effects of net return increase on high-cost ceasefires (excluding definitive ceasefires)

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a probit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Here, definitive ceasefires are excluded and not counted as high-cost ceasefires, since they might follow a different data-generating process. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

# C.5 Estimation using effective date of ceasefire onset

This section displays the results when the effective starting date of the ceasefire is used instead of the agreement's declaration date. The results are nearly identical to the month of declaration of the ceasefire agreement. Table C.8 shows the results for high-cost ceasefire. Here, the effect of a 10% return increase is between a decline of 0.9 percentage points in the month of the shock to around 6 percentage points decline after 5 months. The effect sizes of ceasefire onset are stronger than in the declaration case. Table C.9 displays the results for low-cost ceasefires. Again, I do not find a significant effect in either direction, as predicted by the theory. Table C.10 presents the result for all ceasefires together for completeness. All three models are estimated using a probit regression.

	High-cost ceasefire (with compliance)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)			
Lag 0 Return	$-0.0894^{***}$ (0.0152)									
Lag 1 Return	× ,	$-0.1298^{***}$ (0.0335)								
Lag 2 Return		. ,	$-0.2915^{*}$ (0.1757)							
Lag 3 Return			. ,	-0.3366 (0.2360)						
Lag 4 Return					$-0.2332^{**}$ (0.1152)					
Lag 5 Return					~ /	$-0.5919^{***}$ (0.0579)				
Lag 6 Return						· · · ·	$-0.2428^{***}$ (0.0687)			
Observations	210	201	193	186	166	138	128			
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$			
Dyad FE	√ ↓	√ 1 10 <b>−</b> 0	<b>v</b>	<b>v</b>	<b>v</b>	<b>v</b>	<b>v</b>			
Residual Deviance	154.06	148.73	141.75	137.75	125.51	115.20	108.04			
Null Deviance	204.48	253.02	243.80	230.78	212.23	182.84	170.35			

Table C.8: Average partial effects of net return increase on high-cost ceasefires onset

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a probit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Here, the month of the ceasefire coming into effect is used rather than the declaration date. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Table C.9: Average partial effects of net return increase on low-cost ceasefires onset

	Low-cost ceasefire (without compliance)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)		
Lag 0 Return	-0.0051 (0.0089)								
Lag 1 Return	<b>、</b> ,	-0.0003 (0.0089)							
Lag 2 Return		()	0.0072 (0.0087)						
Lag 3 Return			(0.0001)	0.0094					
Lag 4 Return				(0.0001)	0.0062				
Lag 5 Return					(0.0031)	0.0033			
Lag 6 Return						(0.0080)	$\begin{array}{c} 0.0022\\ (0.0080) \end{array}$		
Observations	813	783	754	687	673	644	619		
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$		
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$		
Residual Deviance	511.79	497.59	480.96	452.47	441.85	420.63	408.23		
Null Deviance	737.42	710.39	686.91	642.63	627.96	613.42	591.34		

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a low-cost ceasefire dummy using a probit regression. The low-cost ceasefire dummy equals one if, in that month, a low-cost ceasefire was declared. Here, the month of the ceasefire coming into effect is used rather than the declaration date. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

	All ceasefires						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	-0.0062 (0.0094)						
Lag 1 Return	× ,	-0.0013 (0.0092)					
Lag 2 Return		× /	0.0076 (0.0094)				
Lag 3 Return			· · /	0.0088 (0.0104)			
Lag 4 Return				()	0.0073 (0.0102)		
Lag 5 Return					()	0.0061 (0.0091)	
Lag 6 Return						(0.0001)	$\begin{array}{c} 0.0053 \\ (0.0094) \end{array}$
Observations	1020	991	958	921	897	856	820
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	645.02	632.14	616.30	594.03	580.22	553.54	534.20
Null Deviance	974.86	945.19	919.83	886.89	865.19	836.34	803.75

Table C.10: Average partial effects of net return increase on overall ceasefire onset

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a ceasefire dummy using a probit regression. The ceasefire dummy equals one if, in that month, any type of ceasefire was declared. Here, the month of the ceasefire coming into effect is used rather than the declaration date. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

#### C.6 Additional tests of government revenue & portfolio shocks

#### C.6.1 Predicting military spending with net return shocks

As an additional robustness check, I use the Military Expenditure Database from the Stockholm International Peace Research Institute (SIPRI, Stockholm International Peace Research Institute, 2024) as a proxy for conflict-relevant expenses by the government. If the government is also affected by increased commodity prices, it will only be relevant for the conflict trajectory if it increases its military capacities. Since the SIPRI data is only available on a yearly basis, I aggregate the returns first on an annual level so that I measure yearly returns compared to the year of conflict onset. Some governments are fighting with more than one rebel group simultaneously. I secondly aggregate the returns on year and state level, leaving me with 30 individual states and a total of 305 yearly observations. I estimate the model using an OLS regression, including year and state fixed effects. Military spending is measured in percentage of GDP. Like the baseline estimations, I also look at lags to allow transmission time. Table C.11 shows the results of this exercise. I do not find that the return shocks can meaningfully predict military spending, suggesting that the government is unaffected by the price increase.

Similar to the prediction exercise above, I can use the military spending leads instead of the re-

Dependent Variable:	Military exp in perc. GDP						
Model:	(1)	(2)	(3)	(4)			
Variables							
Lag 0 Return	-0.0030						
	(0.0023)						
Lag 1 Return		-0.0008					
T OD I		(0.0010)					
Lag 2 Return			-0.0009				
Len 9 Determ			(0.0011)	0.0015			
Lag 3 Return				-0.0015			
				(0.0018)			
Fixed-effects							
Year	Yes	Yes	Yes	Yes			
State ID	Yes	Yes	Yes	Yes			
Fit statistics							
Observations	231	210	189	171			
$\mathbb{R}^2$	0.717	0.742	0.699	0.661			
Within $\mathbb{R}^2$	0.008	0.001	0.002	0.003			

Table C.11: Predicting government military spending with yearly net returns (% of GDP)

Notes: This table presents the results of estimating government military spending in percentage of GDP using yearly net returns. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government military spending data stems form SIPRI data. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

turns lag. This means I test whether today's return shock can estimate the military expenditure in t + 1. The regression is again estimated by OLS, and I include state and year fixed effects. There seems to be a negative relationship between increasing returns and military spending. The direction is a bit puzzling, but it suggests that the return shocks do not positively influence the government. Additionally, considering the magnitude of the effect, it seems negligible and close to zero. A marginal increase in average yearly returns by 1 (meaning a 100% increase compared to the year of conflict onset) decreases military spending by 0.0035 percentage points. Additionally, I estimate the same regression using the logarithm of total military spending in million US-Dollar as the dependent variable. The results are quite similar to table C.12, albeit the magnitude of the negative effect has increased, but it is only significant in the first year.

Dependent Variables: Model:	Lead 1 military exp in % GDP $(1)$	Lead 2 military exp in % GDP $(2)$	Lead 3 military exp in % GDP (3)
Variables Lag 0 Return	$-0.0035^{*}$ (0.0017)	$-0.0037^{*}$ (0.0019)	-0.0021 (0.0017)
<i>Fixed-effects</i> Year FE State ID	Yes Yes	Yes Yes	Yes Yes
$\begin{array}{c} Fit \ statistics\\ Observations\\ R^2\\ Within \ R^2 \end{array}$	$213 \\ 0.752 \\ 0.016$	$204 \\ 0.864 \\ 0.049$	$196 \\ 0.693 \\ 0.005$

Table C.12: Predicting government military spending with yearly net returns (leads, % of GDP)

Notes: This table presents the results of estimating government military spending in percentage of GDP using yearly net returns. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government military spending data stems form SIPRI data. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Here, leads of the dependent variable are used to allow for transmission time of the shock, the return variable stays the same across all estimations. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Dependent Variable:	Military exp in log M. USD 2021					
Model:	(1)	(2)	(3)	(4)		
Variables						
Lag 0 Return	$-0.2231^{**}$					
Lag 1 Return	(0.0947)	-0.1094 (0.0691)				
Lag 2 Return		(0.0001)	-0.0732			
Lag 3 Return			(0.0111)	-0.1058 (0.1045)		
Fixed-effects						
Year	Yes	Yes	Yes	Yes		
State ID	Yes	Yes	Yes	Yes		
Fit statistics						
Observations	230	209	188	171		
$\mathbb{R}^2$	0.978	0.979	0.990	0.990		
Within $\mathbb{R}^2$	0.055	0.019	0.019	0.030		

Notes: This table presents the results of estimating the logarithm of government military spending in million US-Dollar as of 2021 using yearly net returns. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government military spending data stems form SIPRI data. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

#### C.6.2 Predicting government revenue with net return shocks

In table C.14 and table C.15, I repeat the estimations of section 6.2 only using OECD Global Revenue Statistics Database (OECD, 2023) in US Dollar and local currency, respectively. The total values are transformed by taking the logarithm. The results are similar to the estimations using the GRD data (GRD, 2023). I do not find any evidence that government revenue is affected.

Dependent Variable: Model:	$\begin{array}{c} \text{Log total revenue uSD} \\ (1) \qquad (2) \qquad (3) \end{array}$			(4)
Vaniablas		( )	( )	( )
Lag 0 Return	-0 2333			
Lag O Return	(0.2555)			
Lag 1 Return	(0.1020)	-0.1500		
		(0.1502)		
Lag 2 Return		( )	-0.0575	
			(0.1098)	
Lag 3 Return				-0.0192
				(0.0762)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
State ID	Yes	Yes	Yes	Yes
Fit statistics				
Observations	93	87	80	72
$\mathbb{R}^2$	0.988	0.987	0.990	0.995
Within $\mathbb{R}^2$	0.137	0.067	0.015	0.004

Table C.14: Effect of yearly net returns on government revenues (OECD data, USD)

Notes: This table presents the results of estimating the logarithm of government revenues in US-Dollar using yearly net returns. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government revenue data stems from the OECD Global Revenue Statistics Database. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

As a further robustness check, I repeat the estimations from section 6.2 with the GRD data in total local currency unit instead of percentage of GDP. Table C.16 shows the results for total government revenues, table C.17 for revenues stemming from natural resources, and table C.18 displays the results for natural resource tax income. The total values have been transformed using the inverse hyperbolic sine transformation. The results are stable, and I do not find evidence that the rebel return shocks affect government revenues or tax income.

Dependent Variable:		Log total re		
Model:	(1)	(2)	(3)	(4)
Variables				
Lag 0 Return	-0.5451			
8	(0.3440)			
Lag 1 Return	()	-0.3946		
0		(0.3480)		
Lag 2 Return			-0.1791	
-			(0.2547)	
Lag 3 Return			. ,	-0.0815
				(0.1367)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
State ID	Yes	Yes	Yes	Yes
Fit statistics				
Observations	93	87	80	72
$\mathbb{R}^2$	0.982	0.979	0.991	0.996
Within $\mathbb{R}^2$	0.208	0.124	0.063	0.041

Table C.15: Effect of yearly net returns on government revenues (OECD data, LCU)

Notes: This table presents the results of estimating the logarithm of government revenues in local currency using yearly net returns. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government revenue data stems from the OECD Global Revenue Statistics Database. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Table C.16: Effect o	yearly	v net returns on	government	revenues (	GRD	data,	LCU	)
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Dependent Variable:	Asir	nh - Gov. re	evenues in I	LCU
Model:	(1)	(2)	(3)	(4)
Variables				
Lag 0 Return	0.3308			
	(0.3653)			
Lag 1 Return		0.2024		
		(0.1576)		
Lag 2 Return			0.0454	
			(0.1067)	
Lag 3 Return				-0.1235
				(0.1774)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
State ID	Yes	Yes	Yes	Yes
Fit statistics				
Observations	175	159	144	126
$\mathbb{R}^2$	0.912	0.910	0.924	0.947
Within $\mathbb{R}^2$	0.010	0.006	0.000	0.003

Notes: This table presents the results of estimating government revenues in local currency unit on yearly net returns. Government revenues in local currency unit are transformed using the inverse hyperbolic sine transformation. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government revenue data stems from the GRD dataset. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Dependent Variable:	Asinh - G	ov. revenue	es from resou	rces in LCU
Model:	(1)	(2)	(3)	(4)
Variables				
Lag 0 Return	0.8203 (0.6065)			
Lag 1 Return		$0.2802 \\ (0.2183)$		
Lag 2 Return			0.0551 (0.1040)	
Lag 3 Return			、 <i>,</i>	$\begin{array}{c} 0.0140 \\ (0.0462) \end{array}$
Fixed-effects				
Year	Yes	Yes	Yes	Yes
State ID	Yes	Yes	Yes	Yes
Fit statistics				
Observations	123	114	105	94
$\mathbb{R}^2$	0.985	0.984	0.990	0.996
Within $\mathbb{R}^2$	0.043	0.008	0.001	0.000

Table C.17: Effect of yearly net returns on government resource revenues (GRD data, LCU)

Notes: This table presents the results of estimating government resource revenues in local currency unit on yearly net returns. Government resource revenues refers to all revenues specifically acquired from natural resources including tax and non-tax revenues. Government resource revenues in local currency unit are transformed using the inverse hyperbolic sine transformation. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government resource revenue data stems from the GRD dataset. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Table C.18: Effect of yearly net returns on government resource tax income (GRD data, LCU)

Dependent Variable: Model:	Asinh - G (1)	ov. taxes fr (2)	com resourc (3)	es in LCU (4)
Variables				
Lag 0 Return	-0.1915			
	(0.3040)			
Lag 1 Return		-0.0170		
		(0.1192)		
Lag 2 Return			-0.0306	
I AD I			(0.0618)	0.0050
Lag 3 Return				-0.0058
				(0.0230)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
State ID	Yes	Yes	Yes	Yes
Fit statistics				
Observations	190	172	153	136
$\mathbb{R}^2$	0.917	0.939	0.961	0.982
Within $\mathbb{R}^2$	0.004	0.000	0.000	0.000

Notes: This table presents the results of estimating government tax income form natural resources in local currency unit on yearly net returns. Government tax income form natural resources refers to all tax income specifically acquired from natural resources excluding non-tax revenues. Government tax income in local currency unit is transformed using the inverse hyperbolic sine transformation. The monthly net returns are first aggregated on a yearly level and then on a state level to match the dimensions of the dependent variable. Government resource revenue data stems from the GRD dataset. Year fixed effects and state fixed effects are included. Standard errors are clustered on the state level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

#### C.7 Estimation low-cost ceasefires excluding Ukraine

Similarly to the robustness check in Section 6.5, I repeat the same estimation for low-cost ceasefires. The results are displayed in Table C.19. The baseline results remain unchanged.

Table C.19: Average partial effects of net return increase on low-cost ceasefires (excluding Ukraine)

	Low-cost ceasefire (without compliance)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Lag 0 Return	-0.0059 (0.0079)							
Lag 1 Return	. ,	-0.0010 (0.0077)						
Lag 2 Return		× ,	0.0006 (0.0075)					
Lag 3 Return			× ,	0.0020 (0.0078)				
Lag 4 Return				· · · ·	0.0006 (0.0076)			
Lag 5 Return					(0.001.0)	-0.0010 (0.0067)		
Lag 6 Return						(0.000)	-0.0012 (0.0068)	
Observations	789	742	721	653	639	610	585	
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Residual Deviance	480.84	458.32	448.75	420.75	410.68	388.73	376.47	
Null Deviance	712.59	676.09	655.58	610.94	596.27	581.72	559.63	

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a low-cost ceasefire dummy using a probit regression. The low-cost ceasefire dummy equals one if, in that month, a low-cost ceasefire was declared. Here, the rebel groups Donetsk People's Republic (DPR) and Luhansk People's Republic (LPR) fighting against the Ukrainian government are excluded. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

### C.8 Estimation excluding Ukraine & Syria

In the same manner, as excluding the dyads involving the LPR and DPR in section 6.5, one could also argue similarly for the dyad Syrian government against Syrian insurgents. Syrian insurgents were involved in 14 high-cost ceasefires during their conflict. As described in the UCDP Dataset, Syrian insurgents comprise several different rebel groups that were active in Syria. Therefore, I want to test whether the results are robust to excluding the three groups with the most high-cost ceasefire months. Table C.20 shows the results of the baseline estimation, excluding the three dyads involving Syrian insurgents, LPR and DPR. I find a significant negative relation between return increases and the probability of a high-cost ceasefire. The effect sizes are smaller but comparable to the baseline results. Notably, the sign flips in the last lag of the estimation. However, it is crucial to recognize that here the log-likelihood only takes up the variation of 45 observations. This might be problematic since I am using the correction

algorithm by (Fernández-Val and Weidner, 2016), which relies on high N structures. I would, therefore, not attach too much weight to this result. Table C.21 shows this estimation only using low-cost ceasefires as the dependent variable. The results remain unchanged compared to the baseline estimation.

Table C.20: Average partial effects of net return increase on high-cost ceasefires (excluding Ukraine & Syria)

	High-cost ceasefire (with compliance)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)		
Lag 0 Return	$-0.0771^{***}$ (0.0110)								
Lag 1 Return	× ,	$-0.2090^{***}$ (0.0090)							
Lag 2 Return			$-0.1764^{***}$ (0.0077)						
Lag 3 Return				$-0.1500^{***}$ (0.0084)					
Lag 4 Return					$-0.0677^{***}$ (0.0076)				
Lag 5 Return						$-0.1015^{***}$ (0.0078)			
Lag 6 Return							$0.0133^{*}$ (0.0071)		
Observations	88	78	71	66	61	51	45		
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$		
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$		
Residual Deviance	72.57	68.86	65.27	62.36	57.26	50.01	43.48		
Null Deviance	111.56	97.85	87.90	82.56	74.01	63.45	55.80		

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a high-cost ceasefire dummy using a probit regression. The high-cost ceasefire dummy equals one if, in that month, a high-cost ceasefire was declared. Here, the rebel groups Donetsk People's Republic (DPR) and Luhansk People's Republic (LPR) fighting against the Ukrainian government and Syrian insurgents fighting against the Syrian government are excluded. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

Table C.21: Average partial effects of net return increase on low-cost ceasefires (excluding Ukraine & Syria)

	Low-cost ceasefire (without compliance)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Lag 0 Return	-0.0069 (0.0069)							
Lag 1 Return	· · · ·	-0.0018 (0.0066)						
Lag 2 Return		· · · ·	0.0001 (0.0065)					
Lag 3 Return				0.0005 ( $0.0069$ )				
Lag 4 Return					-0.0020 (0.0068)			
Lag 5 Return					(0.0000)	-0.0018 (0.0063)		
Lag 6 Return						(0.0000)	-0.0013 (0.0065)	
Observations	647	607	587	519	506	479	456	
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Residual Deviance	397.78	379.35	370.10	343.50	335.18	318.49	307.08	
Null Deviance	563.87	533.36	513.11	469.29	458.11	444.79	423.54	

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a low-cost ceasefire dummy using a probit regression. The low-cost ceasefire dummy equals one if, in that month, a low-cost ceasefire was declared. Here, the rebel groups Donetsk People's Republic (DPR) and Luhansk People's Republic (LPR) fighting against the Ukrainian government and Syrian insurgents fighting against the Syrian government are excluded. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

#### C.9 Estimation testing price volatility for low-cost ceasefires

As explained in section 6.4, one possible concern is that the start of the conflict is non-random, meaning comparing the commodity price changes to the average price in the first conflict year could potentially introduce endogeneity. To test this, I repeat the baseline estimations but take the five years before conflict start to calculate the commodity price average. For completeness, table C.22 shows the results for low-cost ceasefires. The results for high-cost ceasefires can be found in section 6.4. Low-cost ceasefires are unaffected by the rebel return shocks, even with an extended time horizon for the price calculation.

Table C.22: Average partial effects of net return increase on low-cost ceasefires (base-price avg. 5 years before conflict)

	Low-cost ceasefire (without compliance)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 0 Return	-0.0067 (0.0067)						
Lag 1 Return	. ,	-0.0004 (0.0071)					
Lag 2 Return		()	0.0014 (0.0071)				
Lag 3 Return			(0.0011)	0.0016			
Lag 4 Return				(0.0073)	0.0010		
Lag 5 Return					(0.0074)	0.0046	
Lag 6 Return						(0.0074)	$\begin{array}{c} 0.0042\\ (0.0073) \end{array}$
Observations	1181	1156	1102	1078	1033	947	903
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Residual Deviance	676.13	665.53	648.64	636.67	613.06	591.88	578.97
Null Deviance	1041.94	1023.26	967.97	943.00	910.74	869.79	840.51

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a low-cost ceasefire dummy using a probit regression. The low-cost ceasefire dummy equals one if, in that month, a low-cost ceasefire was declared. Here, the base-price for calculating the monthly net returns is calculated as the average over the five years before conflict start. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

# C.10 Average partial effects of net return increase on start of African peace negotiation (logit model)

As in section 7, here I test how the start of negotiations is affected by the rebel return shocks. I use the APP data (Duursma and Gamez, 2023) again, but I vary the model choice to a logistic regression. The results are stable and not affected by the model choice.

	Start of negotiations							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Lag 0 Return	$-0.3357^{***}$ (0.0811)							
Lag 1 Return	× ,	$-0.2733^{***}$ (0.0901)						
Lag 2 Return		· · · ·	$-0.1828^{**}$ (0.0886)					
Lag 3 Return			()	$-0.1591^{*}$				
Lag 4 Return				(0.0000)	$-0.2317^{***}$			
Lag 5 Return					(0.0010)	$-0.1876^{**}$		
Lag 6 Return						(0.0031)	$-0.1861^{*}$ (0.0964)	
Observations	207	193	192	192	190	189	188	
Year-month FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
Dyad FE	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	
<b>Residual</b> Deviance	190.96	185.67	187.64	188.33	184.70	183.80	183.06	
Null Deviance	252.73	232.48	231.80	231.80	230.41	229.71	227.26	

Table C.23: Average partial effects of net return increase on start of African peace negotiations (logit)

Notes: This table presents the average partial effects (APEs) regressing the dyad-specific monthly net returns on a peace negotiation dummy using a logit regression. The peace negotiation dummy equals one if, in that month, peace negotiations of the specific dyad started. The outcome of the peace negotiations is not considered. All negotiations are related to African dyads, all other dyads are excluded due to data availability. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

# C.11 Estimating nonlinear effects on number of attacks

Similar to the test of nonlinearity of the return shocks on conflict fatalities in section 7, I can, for robustness, also check the effect of the number of attacks. Table C.24 shows the result of this estimation. One attack is resembled by one incidence in the UCDP dataset. The boundaries for negative returns ( $\leq -0.3$ ), moderate returns ( $\leq 0.3$ ), positive returns ( $\leq 0.9$ ), and high positive returns (> 0.9) remain the same. The results show the same general pattern as in table 14, but the inverted u-shape is less pronounced.

P	Number of attacks								
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)		
Variables									
Lag 0 Moderate Return	$0.4349^{**}$								
Lag 0 Positive Return	-0.0620								
Lan O High Desitive Deturn	(0.3679)								
Lag o High Fositive Return	(0.4313)								
Lag 1 Moderate Return		$0.3813^{**}$							
Lag 1 Positive Return		(0.1749) -0.0386							
Log 1 High Desitive Detune		(0.3448)							
Lag I figh Positive Return		(0.4186)							
Lag 2 Moderate Return			$0.4237^{**}$						
Lag 2 Positive Return			(0.1772) 0.0828						
Lag 2 High Positivo Boturn			(0.3572)						
Lag 2 High Fositive Return			(0.4335)						
Lag 3 Moderate Return				$0.4505^{**}$					
Lag 3 Positive Return				0.2020					
Lag 3 High Positive Return				(0.3457) - $0.4650$					
Len 4 Medeurte Deturn				(0.4459)	0.4019**				
Lag 4 Moderate Return					(0.1983)				
Lag 4 Positive Return					0.1723				
Lag 4 High Positive Return					(0.3493) -0.4465				
Lag 5 Moderate Return					(0.4765)	0.3727*			
Lag o Moderate Return						(0.2018)			
Lag 5 Positive Return						0.1715 (0.3520)			
Lag 5 High Positive Return						-0.4432			
Lag 6 Moderate Return						(0.4791)	$0.3661^{*}$		
							(0.2031)		
Lag 6 Positive Return							(0.1832) (0.3352)		
Lag 6 High Positive Return							-0.4108		
Fired_offects							(0.4701)		
Year-month	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Dyad ID	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Fit statistics									
Observations	3,572	3,536	3,502	3,466	3,432	3,396	3,362		

Table C.24: Nonlinear effect of net returns on number of attacks: Poisson regression

Notes: This table presents the results of a Poisson regression of the dyad-specific monthly net returns on number of attacks. Here, returns are grouped in bins: < -0.3 negative returns, -0.3 - 0.3 moderate returns, 0.3 - 0.9 positive returns and > 0.9 high positive returns. The lowest bin (< -0.03 negative returns) is used as base category. Number of attacks refers to the total number of dyad related incidents recorded in a given month in the UCDP data. Year-month fixed effects and dyad fixed effects are included. Standard errors are clustered on the dyad level. Each model has the same dependent variable; only net returns are lagged by one period to allow for the transmission time of the shock. Significance codes are \*\*\*: 0.01, \*\*: 0.05, \*: 0.1.

# **D** Limitations

This study has some limitations. I try to construct a sufficient proxy for the composition of the rebel income sources. Due to data limitations, I do not know how much each single natural resource contributes to the funding of a group. A weighted portfolio would allow me to create a more precise return shock. Illicit goods, such as cocoa or opium, are not included due to the lack of monthly price data. Further, no information is available on how much funding rebels receive from other criminal activities such as theft or extortion. More granular data would allow me to explore the mechanism in more detail. I can only observe the announcement of ceasefires. I do not have information on ceasefires that failed before the start. I can only proxy for the intentions of the rebel group, but precise information on the motivations is not available. I can only observe whether both parties agreed to the negotiations or a ceasefire, but I do not have each side's individual decision-making process. Having intentions and declarations of each belligerent would be helpful in disentangling the mechanics of the conflict. University of Innsbruck - Working Papers in Economics and Statistics Recent Papers can be accessed on the following webpage:

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Sebastian Kupek

Battlefield Bargains: The High-Stakes Dance of Rebel Finances and Ceasefire Deals

## Abstract

Wars fueled by resource wealth pose complex challenges for conflict resolution. I combine data on conflict events, rebel resource dependence, world market prices, and ceasefire details to investigate how fluctuations in rebel funding affect the likelihood of ceasefires. Increased funding opportunities strengthen the rebel position and decrease the bargaining range between belligerents. Higher resource rents prolong the conflict and make resolution less likely. Using the exogenous variation of world market prices, I find that rebels react strongly to changes in their budget situation. Price increases reduce the likelihood of rebels entering ceasefires connected to conflict resolution.

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