Pricing Conflict Risk: Evidence from Sovereign Bonds*

Jonah M. Rexer† — *The World Bank*

Ethan B. Kapstein‡ — *Princeton University*

Andrés F. Rivera§ — *Universidad ICESI*

January 8, 2026

Abstract

We examine how sovereign bond markets react to violent conflict using daily bond prices and conflict data over the past two decades. Event studies reveal bond prices fall by up to 1.2% within two weeks of state-involved conflict, driven by rising default risk, with effects concentrated in short maturities and sovereigns with weak credit ratings. A simple pricing model reveals that the share of the conflict shock priced in rises from 14% initially to 75% after 15 days. Bondholders respond more to economically costly conflict events featuring severe violence near the capital city targeting the state, while outbreaks in conflict-prone countries are priced in. Responses in long-maturity bonds predict conflict duration. Media coverage drives investor learning about conflict, but only in sparse news environments where conflict news is not crowded out, highlighting the role of limited attention.

*We are grateful for excellent research assistance from Adityamohan Tantravahi and to Bobray Bordelon for facilitating access to data. We thank Juan Vargas, Santosh Anagol, Jacob Shapiro, Monu Prem, Till Saenger, Ronnie Sircar, Marcel Schlepper, Christoph Trebesch, and participants at Georgetown University, the ESOC lab meeting, the Virtual Workshop on Finance and Development, the CESifo Political Economy conference, the Royal Economic Society Annual Conference, the Latin American Peace Science 4th Annual Congress, DebtCon 6 at Princeton University, UT Dallas, and the NBER Summer Institute Economics of National Security for helpful comments. This project is part of the Development Finance Research Community at Princeton University and is funded by the Princeton Institute for International and Regional Studies.

†Economist, the World Bank: jrexer@worldbank.org

‡Co-director, Empirical Studies of Conflict Project, Princeton University: kapstein@princeton.edu

§Lecturer, Universidad ICESI: andresfel.rivera@urosario.edu.co

1 Introduction

Armed conflict is costly for societies, and as such is of increasing interest to economists. It disrupts productive activity (Utar, 2020), stifles investment (Blair et al., 2022), discourages human capital accumulation (Leon, 2012; Akresh et al., 2012), and ultimately reduces economic growth (Collier, 1999; Abadie and Gardeazabal, 2003; Cerra and Saxena, 2008).[1] Violence also upends global markets, causing trade disruptions (Qureshi, 2013; Ksoll et al., 2022), commodity price spikes, and financial panics (Chesney et al., 2011; Ouedraogo et al., 2022).

The risk of political violence looms particularly large in sovereign bond markets, where conflict fundamentally affects sovereigns' ability to pay creditors. In 2020, there were 30 active violent armed conflicts in countries borrowing on international capital markets. The ongoing war in Ukraine provides a dramatic example of financial market responses to conflict; the price of Ukrainian bonds plummeted to 22 cents on the dollar in the month following Russia's February 2022 invasion.

Yet, despite the prevalence and economic cost of conflict, these potentially dramatic bond market reactions to outbreaks of political violence are not well understood, notwithstanding a voluminous literature on sovereign risk (Aguiar and Amador, 2014, 2021; Meyer et al., 2022). Existing evidence linking financial market behavior to war and peace primarily comprises a handful of individual country case studies or historical episodes.[2] What determines bond market responses to conflict, and how much do markets really know about the underlying political and economic dynamics of violence? A popular opinion is that markets are poorly informed in this domain. A recent article in The Economist summarizes the conventional wisdom: "investors are terrible at forecasting wars [and]...just as clueless after conflicts happen." According to this view, markets are susceptible to bad information, incorrect beliefs, and behavioral biases (Gennaioli and Shleifer, 2018).

To understand what markets know about war, we combine daily price data on international sovereign bond issues with news media data on the onset of several hundred armed conflicts from 2004-2020. Using a bond-day panel, we estimate the dynamic effects of conflict onset on bond prices using event-study regressions. We define "conflict event onset"

[1] The literature on the economic costs of conflict is reviewed in Davenport et al. (2019) and Rohner (2018), among others. Estimates of economic losses vary substantially, but typically range between 2-6% of GDP annually.

[2] See e.g. Guidolin and La Ferrara (2007); Chaney (2008); Arin et al. (2008); Frey and Kucher (2001); Brown and Burdekin (2000); Castañeda and Vargas (2012).

as the first violent incident observed between a unique actor pair (e.g., government forces vs. an insurgent group) in a given country, or a subsequent violent incident in that country-actor pair after a prolonged period of peace. To estimate daily bond price effects, we use a "stacked" event study estimator, which mitigates the bias of two-way fixed effects estimators in staggered adoption difference-in-differences settings (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021).[3]

Our estimates reveal that the effect of conflict on bond markets is highly heterogeneous; investors only respond when conflict threatens the state, increasing credit risk. Bond prices fall by a daily average of 0.7 points (relative to par) after the onset of *state-involved* conflicts (where a state actor battles another state or an organized rebel or terrorist group). This point estimate rises to 1.2 points 15 days after onset, suggesting that investors learn about conflict over time. In contrast, we observe null effects for conflict between non-state groups and for violence against civilians.

This pattern is consistent with rational responses to conflict. Investor sentiment does not naively react to bad news; rather, we find that violence against civilians – disproportionately covered by the international media – does not on average generate substantial market response. Instead, markets only respond in cases where the sovereign's creditworthiness is affected. We show empirically that the state conflict-specific market response is matched by an underlying increase in credit risk; only the onset of state conflict triggers a large, long-run increase in the monthly probability of debt restructuring. Bond market losses are largest for the countries with low credit ratings that are closest to the default threshold ex-ante, again consistent with the overriding role of credit risk in driving the effects.

The dynamic estimates also demonstrate parallel pre-trends in bond prices between treated and control countries prior to conflict onset, confirmed by several formal tests that account for low power and pre-test bias. This bolsters confidence in our identification assumption of parallel trends, and suggests our conflict onset dates are precise and unanticipated.[4] We also show that the results are robust to controlling for global macro-financial and commodity shocks, and differential trends by country-level institutions, macroeconomic fundamentals,

[3]Importantly, because they lack access to global capital markets, many of the world's most fragile states are excluded from our sample. Our results should therefore be interpreted as externally valid for a sample of middle-income emerging countries, rather than extremely poor fragile states.

[4]Data on news coverage confirm that our conflict onset dates are unanticipated by international media sources.

resource dependence, and various measures of country risk. The results are also robust to interacted bond-specific controls for maturity, currency, loan size, and coupon rate. We also probe robustness to measurement error, outliers, several different sample restrictions, propensity score reweighting, and other event-study estimators.

To benchmark the magnitude of market responses, we build a simple bond pricing model in which investors learn about the likelihood of violent conflict, which enters the value function as a constant haircut to bond payments in years of active violence, equal to the average annual effect of violent conflict on national income.[5] In equilibrium, market responses to conflict onset depend on three factors: i) investors' prior belief on the risk of conflict, ii) the strength of the signal at onset and the extent to which investors update beliefs, and iii) investor beliefs on the cost of conflict.

Calibrating the model at full information with estimates of empirically correct beliefs shows evidence of both initial under-reaction and investor learning: the share of the shock that is priced in rises from only 14% initially to roughly 76% after 15 days. Consistent with model predictions, we show that short maturities are most affected by conflict, whereas no effects are observed for maturities greater than 10 years. This is driven by the fact that conflict follows an AR(1) process: conflict onset today predicts heightened conflict in the short-run, but annual conflict risk eventually reverts to a long-run mean. Therefore, default risk rises for short bonds, whereas long bonds only come due long after risk has stabilized.

The remainder of the paper investigates what investors know about conflict; that is, whether bond market responses are consistent with reasonable beliefs about conflict risk and cost. The model delivers three key predictions: rational, well-informed investors should respond more where i) priors are more optimistic, so conflict is a surprise, ii) where conflict is costlier, and iii) where information is more plentiful, so priors update more nearly to full information.

We find that effects are significantly larger where priors are optimistic because conflict risk has historically been low. Quantitatively, the average daily bond price effect rises to 1.5 points for a country with no history of conflict in the past ten years and falls to zero for a country with conflict in all of the past ten years. This effect is larger for more severe and recent conflicts. These patterns suggest a relatively sophisticated understanding of country-specific conflict risk by bondholders, who put more weight on recent, deadlier conflict in forming adaptive

[5] Our setup recalls Barberis et al. (1998) and follows the treatment of restructuring risk in Asonuma et al. (2017).

priors (Haruvy et al., 2007).

Investors also use initial information to form cost expectations. Bond price effects are largest for severe outbreaks of violence, with the daily average effect rising to 2.4 points for conflicts in the top quintile of deadliness.[6] Furthermore, we find that conflict onset in the capital city – where risks of regime collapse are heightened – provokes a 1.95-point sell-off, nearly four times larger than the effect for events outside of the capital.[7] We also find that investors are aware of the underlying political divisions driving conflict, responding primarily to center-seeking conflicts that seek to overthrow the state, rather than separatist or spillover wars. Lastly, we show that each of these conflict characteristics is indeed an important independent predictor of conflict cost. Quantitatively, investors price this information accurately, with only mild evidence of correlation neglect (Enke and Zimmermann, 2017).

Not only do bond markets use initial information to price in the economic cost of conflict at onset, they are also able to predict ultimate conflict outcomes. For a subset of state conflicts, we can observe termination during the sample period. For these conflicts, market responses predict fighting duration: on average, the effect of conflict is just 0.3 for conflicts in the 10th percentile, but rises to 0.9 in the 90th percentile of duration. Furthermore, this gradient is only observed in long bonds, since short bonds shouldn't internalize duration. For 10+ year bonds, the 10 percent longest conflicts see price drops of 2.3 points, more than three times the average effect.

Finally, we study the role of news media in facilitating investor learning. As a proxy for information transmission, we calculate the total number of news articles covering each conflict in the weeks before and after its onset. We show that investors respond substantially more to state conflicts that receive greater media coverage after onset. At the same time, investors respond significantly *less* to conflicts that received more coverage in the weeks *prior* to onset. This suggests investors price in relevant information ex-ante, and continue to learn as information accumulates ex-post, consistent with Bayesian updating. However, this effect *only* occurs in information-sparse environments, where "news pressure" (Eisensee and Strömberg, 2007) does *not* crowd out investor attention on media coverage of conflict events. Instead, when competing high-profile news stories are plentiful, investors do not respond to

[6] We define conflict fatalities as those occurring on the onset date to capture the information available to investors at the time of the onset.

[7] We find no differential effects of attacks on other centers of economic activity.

conflict-specific information flows. This result suggests an important role for limited attention (Peress and Schmidt, 2020; Israeli et al., 2022) in investor responses, which may explain the underpricing we observe in the magnitude of the main effect. However, we cannot rule out that limited attention might also induce strategic behavior in timing of attacks by conflict participants (Durante and Zhuravskaya, 2018).

We contribute to several strands of the literature in political economy, economics, and finance. First, we add to an extensive literature on the economics of conflict and state fragility. It is by now well known conflict and political instability are economically costly (Rohner, 2018; Davenport et al., 2019). What is less clear is how these economic costs are priced in financial markets, and what this market behavior might reveal about the underlying dynamics of conflict. We show that markets exhibit sophisticated knowledge of payoff-relevant conflict characteristics, and can even predict conflict outcomes like duration ex-ante. By aggregating available information in real time, markets may serve as potentially useful tools for difficult conflict prediction problems (Bazzi et al., 2022), as has been the case with political prediction markets (Arrow et al., 2008).[8] Still, the market's reliance on media reports and susceptibility to limited attention poses challenges in interpreting these predictions.

We join a growing literature investigating the relationship between politics and financial markets. Since at least Mauro et al. (2002), it has been well-known that political risk influences sovereign spreads. More recently, a set of related recent studies assess the impact of coups (Balima, 2020), democratization (Dasgupta and Ziblatt, 2022), and other political transitions (Girardi, 2020; Ouedraogo et al., 2022) on financial markets. Nonetheless, our understanding of market responses to violent conflict is less developed, coming primarily from single country cases like Iraq (Greenstone, 2007; Chaney, 2008), Colombia (Castañeda and Vargas, 2012), Mexico (Kapstein and Tantravahi, 2021), and Angola (Guidolin and La Ferrara, 2007), as well as historical studies of major conflicts like the American Civil War (Brown and Burdekin, 2000) or World War II (Frey and Kucher, 2001; Barro, 2006; Waldenström and Frey, 2008). An emerging literature studies how derivatives such as credit default swaps (CDS) on sovereign debt react to conflict and political uncertainty (Shen et al., 2024; Pan et al., 2024). We argue

[8] Mitchener et al. (2015) study sovereign bonds as prediction markets in historical conflicts with clear winners and losers, in which bond prices can be interpreted as a bet on the win probability of a given side. In contrast, most contemporary civil wars tend to be protracted, and rarely conclude with dramatic transfer of power or victory. Per UCDP, out of the 3140 state-involved conflict episodes since 1950, only 6.8% have ended with a clear government or rebel victory. In contemporary conflicts after 2000, this is just 3.6%.

that sovereign bond prices provide much more granular information, for a longer time span, over a wider range of countries, and in a more thickly traded market,

We improve on this admittedly large existing literature on the relationship between political risk and sovereign bonds in a number of important ways. While these historical and case studies have the benefit of studying detailed event histories, they are under-theorized and lack external validity. Our large sample approach goes beyond simply showing that "conflict risk matters," allowing us to obtain the first externally valid estimate of market response and leverage conflict-level heterogeneity to study investor beliefs. This heterogeneity allows for the study of how markets respond to country and conflict-specific characteristics such as conflict history, expected cost, duration, and the news environment, providing new insight into belief formation and information diffusion in financial markets in times of war. This represents a marked improvement on existing single-country or historical studies, which lack sufficient heterogeneity to make inferences about underlying mechanisms driving market responses to conflict. In contrast to Mauro et al. (2002) – and to the conventional wisdom of the popular press – we argue that bond market investors pay close attention to specific details of countries' conflict dynamics. [9]

Finally, we also add to the literature on behavioral finance, learning, and asset pricing. A large literature focuses on explaining asset price movements with uncertainty and learning (Pastor and Veronesi, 2009) or by appealing to biased investor beliefs (e.g. overoptimism, as in Reinhart and Rogoff (2009); Gennaioli et al. (2015)), behavioral biases (Barberis and Thaler, 2003; Bordalo et al., 2018; Gennaioli and Shleifer, 2018), and non-Bayesian learning (Schraeder, 2016). We extend this work by investigating market responses in the case of violent conflict, where both information frictions and behavioral biases are likely to be pronounced. Nevertheless, despite some evidence of limited attention, we find evidence of substantial learning and extensive conflict-specific market knowledge.

[9] Only a few papers study the impacts of conflict on financial markets in large samples. Guidolin and La Ferrara (2010) find *positive* effects of conflict on stock markets, though the mechanism for this counter-intuitive result is not specified. The most closely related paper is Chadefaux (2017), which studies bond market reactions to historical wars, finding yield spikes after onset and interpreting negative market reactions to conflict as evidence that markets underestimate conflict risk. Our results demonstrate instead that a negative effect is consistent with rational behavior, and that a fully-specified model of beliefs is required in order to make inferences about the direction and degree of mispricing. Like Jha et al. (2022), we emphasize the role of investor beliefs in understanding conflict price responses.

2 Context and data

2.1 Conflict data

To identify the onset date of armed conflicts, we use data from the Uppsala Conflict Data Program (UCDP) of the Peace Research Institute at Oslo (PRIO). This data is one of the standard sources of conflict events in the literature on the economics of conflict. The UCDP Georeferenced Event Dataset (GED), Version 21.1 contains information on 261,864 violent incidents of armed conflict from 1989-2021. The UCDP-GED dataset uses local and international media sources to identify, geolocate, and describe unique incidents of violence that are part of larger armed conflicts. We define a *conflict* as a violent relationship between two actors (a dyad) in a given country. For example "Turkey: Government of Turkey vs. Islamic State." If the same dyad confronts each other in two different countries, these are considered two distinct armed conflicts. We call these "spillover conflicts" when they occur outside of a given state's territory. Within a conflict, there may be multiple discrete periods of fighting – what we call conflict *events* – each with a unique *onset* date.

There are two types of conflict events in our data. *First events* of a given conflict are dated from the first time an country-actor-pair enters the UCDP-GED dataset. *Subsequent events*, instead, are those that occur after a period of relative peacefulness and cause significant damage, representing a revival of an existing conflict for a country-actor-pair. Two conditions must be met in order for particular violent incident to be classified as the onset date for a subsequent conflict event. First, the number of peaceful days between violent incidents in that conflict is above the 95th percentile of the incident-level empirical distribution within that armed conflict. Second, the number of fatalities that occur during that incident is greater than the 95th percentile of its incident-day-level empirical distribution within that specific armed conflict. In short, both the period of peace and the subsequent spike are defined relative to the distribution of event-days *within that conflict*. The onset dates of these conflict events, combined with their country of occurrence determine the timing and location of treatment exposure to conflict.

We also collect data on conflict characteristics, including the participation of state forces, non-state armed actors, or civilians, as well as onset event characteristics such as location, distance to capital, the number of fatalities, and the ultimate duration in the event of conflict

termination. We then merge our conflict episode onset dates with data on daily sovereign bond prices (described below). Our final sample – events for which bond price data exists within +/- 30 days of the onset date – contains 313 conflict events linked to 262 armed conflicts affecting 44 countries from 2004-2020. Table A1 contains a list of all conflicts in our data, while A2 shows the number of unique events, conflicts, countries, and bonds by conflict type.

In our study, we adhere to the UCDP-GED coding conventions and focus on three mutually exclusive types of conflicts: state conflicts, non-state conflicts, and one-sided violence. State conflicts are characterized by engagements between state forces and either other state forces or non-state armed actors. The requirement is only that *one side of the conflict is a state actor*. Non-state conflicts involve confrontations between two non-state armed groups, neither of which is a state actor. Finally, one-sided violence occurs when an armed actor, whether state or non-state, targets and engages against the civilian population.

2.2 Economic and financial data

We use global bond market data from Cbonds, a bond trading platform. We obtain daily bond price quotes for a sample of 2347 international (foreign currency) bonds issued by 122 countries from 2003-2022. We also add bond-level data on the issue date, maturity, bond currency, coupon rate, loan size, and issue yield, among other characteristics. Our primary outcome is the daily bond price relative to par (100).

We complement these data with a large set of country characteristics constructed from several different sources. We obtain macroeconomic fundamentals from the World Bank and International Monetary Fund. We measure country institutional quality using data from the World Governance Indicators. We also obtain a host of country risk ratings from the International Country Risk Guide (ICRG), including economic, financial, and political risk assessments. All country characteristics are measured in the year of the bond issue. We obtain daily data on commodity prices and global equity and bond indices from the World Bank and the Federal Reserve Bank of St. Louis (FRED).

We generate our final estimation sample by merging Cbonds and UCDP-GED event dates. We construct 313 event-specific datasets as follows: for each event, we obtain all bond-day price observations for the conflict-affected country within a symmetric 61 day estimation window around the onset date. To this we append the set of "clean" control bond-days within the

same time window; these are securities issued by countries that have had no conflict events at all during our sample period. Within each event dataset, we center the price series around its event-date. We then stack these event datasets and generate a single treatment indicator and relative-time dummies in order to estimate an average effect across all events.

2.3 News media data

To collect data on news coverage for our sample of 313 conflict events, we search each conflict using Lexis Nexis, an online news database. All resulting news articles must include the name of each actor in the dyad, as well as the country where the event occurred. To efficiently extract news articles from the Lexis Nexis API, we build an ETL (Extract, Transform, Load) pipeline contained in the R package *rln* (Tantravahi, 2022). The package is designed to extract news articles from Lexis Nexis, transform them into an analysis-ready tabulated format, and load them into a database for analysis. We identify and remove duplicate articles by calculating Levenshtein edit distances in headlines and body text. For each conflict event, we calculate the total number of unique articles published each day within our 61-day estimation window to assess trends in media coverage by conflict characteristics. In all, we are able to obtain comprehensive news coverage data on 298 of 313 conflict events.[10] Our news dataset covers not just major international and English-language news sources but many translated local news outlets, which may contain more timely information on lesser-known conflicts.

To measure the overall information environment in which investors operate, we calculate the "daily news pressure index" from Eisensee and Strömberg (2007). This is measured as the median number of minutes devoted to the top three news segments across US television news broadcasts in a given day, where the main news networks are ABC, CBS, NBC, and CNN. The index captures the extent to large, newsworthy stories – like the Olypmic Games, the 9/11 attacks, or the OJ Simpson trial – might "crowd out" other media coverage, and has been shown to meaningfully influence investor behavior (Israeli et al., 2022). The data is available on a daily basis from 1968-2022 from David Stromberg's website.

[10] The 15 missing events primarily comprise conflicts that are not mentioned in Lexis Nexis news sources. Events in the UCDP-GED dataset are identified from a wide array of sources, and while the majority originate from well-known international news sources, this is not always the case.

2.4 Trading prices trends

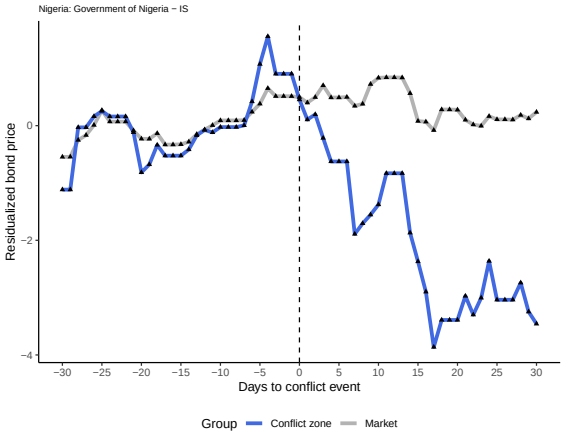
We begin by reporting on the dynamics of bond prices for a selection of armed conflict events in our data. We select these events as case-study illustrations of systematic empirical patterns that we demonstrate in Sections 4 and 5. In order to normalize the scales of the treatment (conflict zone) and control (market) price series', the average prices are residualized by the pre-event mean daily price difference. In particular, we run a linear regression of average bond prices on a conflict zone indicator during the pre-event window ($t < 0$), and then subtract the prediction of this regression from the price series, yielding the residualized bond price.

We examine the trends in bond prices for state-involved, non-state, and one-sided conflict episodes in Figure 1 (a) and (b). We hypothesize that markets are likely to respond to conflicts involving government forces, which strain government budgets and threaten political instability. In panel a), we use the conflict between the Nigerian state and the local affiliate of the Islamic State (IS), a splinter of the militant group Boko Haram. Islamist conflict in Nigeria has been a source of significant territorial loss and military investment for the state (Rexer, 2022). In contrast, conflicts between rival cartels (non-state actors in panel b) in the Mexican Drug War may be deadly but nonetheless do not pose a fundamental threat to the Mexican state. Lastly, violence against civilians – while it may generate negative media coverage – is unlikely to affect state creditworthiness. We consider violence against civilians in Colombia by FARC dissidents in panel c).[11] Figure 1 illustrates a steep drop in bond prices for the conflict zone relative to the market average after a conflict onset in the Nigerian case, but not for the others.

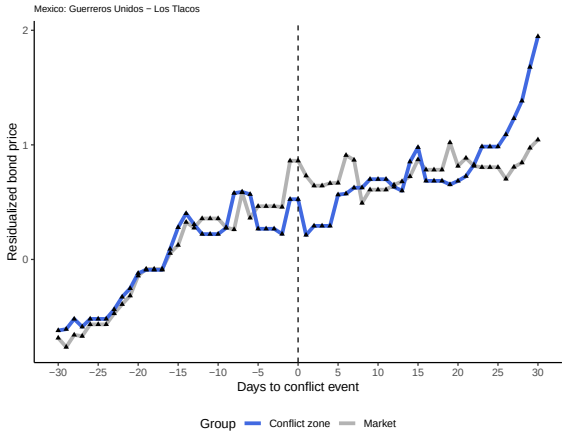
Next, we argue that rational investors should learn about the probability of conflict from past experience, so that new onsets should be more surprising than recurring episodes. We examine this hypothesis in Figure 1 (c) and (d) in the context of the conflict between the Turkish government and IS. Beginning in 2015, IS began attacks on both civilian and government targets within Turkey, representing a substantial increase in conflict risk. However, the first such instance of this conflict is likely to be the most surprising to investors. We observe a substantial drop in Turkish bond prices after the onset of this conflict. However, no changes

[11] This is a faction of the FARC that has not accepted a 2016 peace agreement ending the long-running civil war.

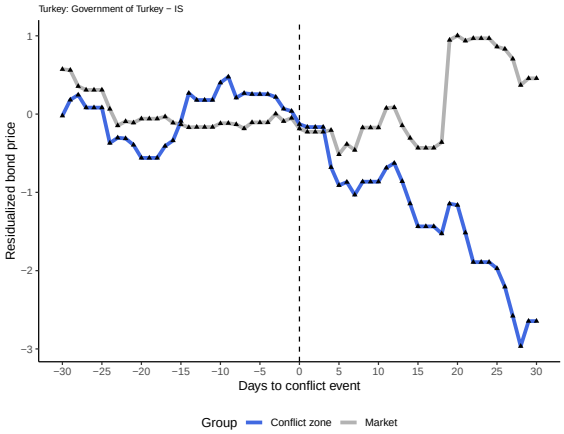
Figure 1: Conflict onset and bond prices by type of conflict



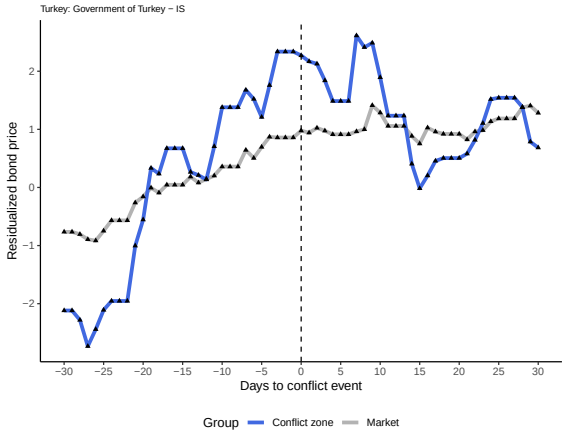
(a) State-involved



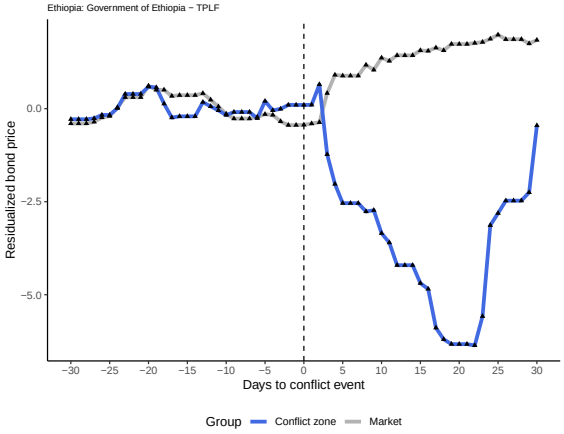
(b) Non-state



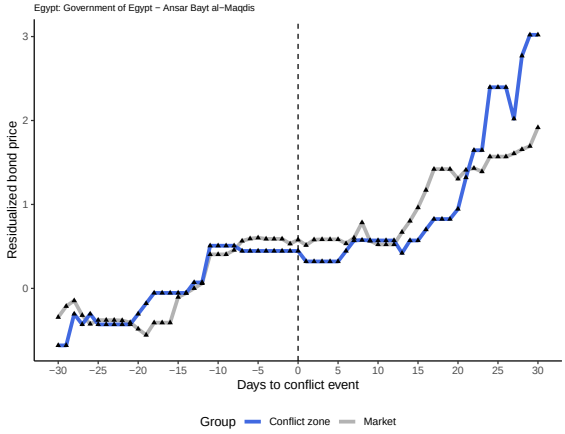
(c) Conflict onset



(d) Subsequent episode



(e) Most violent event



(f) Least violent event

in bond prices obtain for subsequent military engagements between the Turkish government and IS.

Finally, events representing a larger shock in terms of expected conflict losses should provoke larger market responses. Figure 1 (e) and (f) contrast the trajectory of bond prices among the most violent (top quintile) and least violent (bottom quintile) state-involved conflict onset episodes. One of the most violent cases in the data is the ongoing Tigray War between the Ethiopian government and the Tigrayan Peoples Liberation Front (TPLF), a cataclysmic conflict that has claimed up to 500,000 lives in just two years. Bond prices fall dramatically after the onset of the Tigray War. For one of the least violent onset episodes – Egypt's low-intensity conflict against a jihadist group in the Sinai Peninsula – there is no indication of such a pattern.

3 Empirical strategy

3.1 Estimation

We use stacked event-study regressions to estimate the dynamic short-run effects of conflict onset on bond trading prices. We begin by taking our sample of bonds, indexed b, and obtain their daily trading prices, indexed t. For each conflict event e, as defined in Section 2, we form the episode-specific dataset by combining all treated bond-days within the 61-day event window with the sample of "clean" control bond-days. Lastly, we stack each episode into a single dataset and center event-times around k_e, the onset event date. We then apply the "stacked" event-study model (Baker et al., 2021; Dube et al., 2022) to our daily financial markets data. We estimate the following specification for bond b at calendar day t issued by country c in event (stack) e, for $t \in [k_e - 30, k_e + 30]$:

$$y_{btce} = \alpha + \sum_{k \neq -1} \tau_k Treat_{ce} \times 1(k = t - k_e) + \delta_{be} + \delta_{te} + \xi' X_b \times \gamma_{te} + v_{btce} \tag{1}$$

Where y is the trading price of bond b on day t. $Treat_{ce}$ indicates whether country c is the treated country of stack e, of which there is exactly one per event. τ_k are coefficients for leads and lags of the onset date, with $k = -1$ omitted. When y is measured as a (log) trading price, τ_k gives the (percentage) return to a bondholder from holding the bond of a conflict-affected

country purchased at onset for k days, relative to the market (control group) average.[12]

The stacked estimator is akin to a two-way fixed effects event-study regression; the estimates τ_k are variance-weighted averages of the event-wise dynamic effects. The fully saturated model requires event-specific unit and time effects δ_{be} and δ_{te}. Because countries and events may differ in the maturity composition of their bonds, our main specifications also includes initial bond maturity fixed effects X_b interacted with time effects γ_{te}. This is particularly important because, as shown in Section 4.4, we expect the effects to vary substantially by maturity. We also run specifications collapsing the dynamic effects into a single post-event indicator to obtain the average daily effect.

Note that bond-days may repeat in our stacked dataset, since the same country may serve as controls in multiple different events. Similarly, one country may be treated multiply by different conflict events whose windows overlap chronologically. As such, standard errors must be clustered at the country level to account for serial correlation induced both by the underlying panel structure and mechanically by the stacked data. Country-level clustering allows for unrestricted covariance in errors across all bonds, time periods, and events within a given country, which is the unit of treatment assignment (Abadie et al., 2022). Lastly, we test hypotheses about investor responses to conflict by conditioning on event-specific characteristics v_e, such as the presence of state forces or the number of fatalities. In practice, we may split the sample if v_e is binary, or interact v_e with our treatment variable if it is continuous. We include all control variables, at bond or country level, as interactions with the δ_{te} fixed effects.

3.2 Identification

The first key identifying assumption in our event study set-up is parallel trends. Bonds issued by conflict-affected countries may be trending differently prior to conflict. For example, if social conflict is sparked by a long period of economic or financial crisis, this may be reflected in turmoil in bond markets prior to the conflict event. As such, our estimates of conflict onset effects may simply capture preexisting, correlated market trends. To probe the

[12]Note that this is similar to the cumulative abnormal return (CAR) from the finance literature on event-studies (Mackinlay, 1997), with different counterfactual modeling choices. In a finance event-study, the counterfactual is modeled using the prediction from a market model estimated in some pre-event period, while in the applied economics approach that we employ, it is modeled using the contemporaneous trends of control group, the market. We consider this estimator in Section 6.

identifying assumption, we estimate dynamic pre-period coefficients (k <0) in our primary specification, accounting for low power and pre-test bias (Roth, 2022). These pre-trend estimates contain valuable insight into the economic responses of bondholders to conflict situations. For example, anticipation effects prior to onset dates may represent responses to prior information about a conflict. Therefore, pre-trends are not just a test of identifying assumption for causal effects, but also a test for pre-period information.

A related identification issue is the presence of simultaneous shocks. If conflicts are correlated with macroeconomic shocks, and these shocks have differential effects in conflict-affected markets, then the estimated effects may be spurious. We account for correlated aggregate shocks by interacting the treatment indicator with global market conditions – the S&P, VIX, EMBI, and commodity indices. In addition, we allow for differential trends in outcomes by interacting year-by-event fixed effects with a host of country-level controls, including macroeconomic conditions, institutional characteristics, and country risk ratings. We also focus on a relatively narrow event window in order to minimize exposure to other confounding shocks, which likely operate on a longer time horizon.

It is now well-known that TWFE estimates of difference-in-difference treatment effects may be biased in the presence of effect heterogeneity (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). The fundamental challenge with standard TWFE estimators is their use of already-treated and not-yet-treated controls, since these may be on different trends in the presence of dynamic effects. In order to obtain a clean set of controls, we use only never-treated bonds issued by countries that never experience a conflict onset event in our data. These observations are not "contaminated" by either pre or post-treatment dynamic heterogeneity. Our stacked event-study estimator is similar to the Callaway and Sant'Anna (2021) estimator for staggered difference-in-differences in that it excludes contaminated controls, using event-wise estimation and aggregation of treatment effects. While their estimator is nonparametric, our linear functional form is nevertheless easier to interpret and straightforwardly accommodates interaction effects.[13]

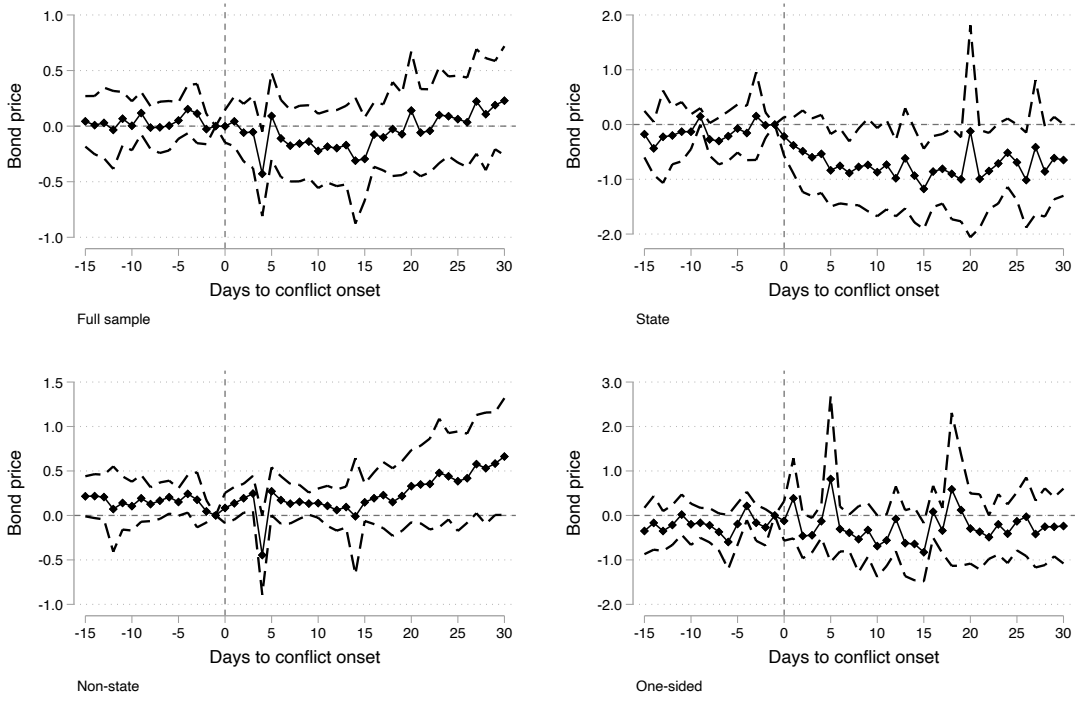
[13]Furthermore, Callaway and Sant'Anna (2021) is computationally infeasible in our context due to a lack of common support on event dates. Because our data is temporally disaggregated, any given event-day is likely to have only a small number of treated units. Our relative-time specification eliminates this issue.

4 Main results

4.1 Conflict onset and bond prices

We begin by considering the aggregate impact of conflict on bond prices. Because we expect substantial heterogeneity in price responses, we also estimate equation 1 for three mutually exclusive subsamples of armed conflict events: state-involved, non-state conflicts, and violence against civilians. Figure 2 shows the dynamic event-study coefficients for each of these groups. The top-left panel shows that in aggregate, there is no effect of conflict onset on bond prices. The plot exhibits clear parallel trends, with the post-event coefficients, all small in magnitude and statistically insignificant.

Figure 2: Event-study: conflict groups



Note: Figure shows coefficients from stacked event-study regressions described in Section 3 on daily bond data for four different samples of conflicts, indicated in each subfigure footer. Standard errors are clustered at the country level. Outcome is the daily bond trading price averaged across all available trading exchanges, indexed to 100 (par). Specifications include interacted event-specific two-way fixed effects as well as interacted bond maturity fixed effects.

However, this muted aggregate pattern masks substantial heterogeneity. We reason that

only conflicts involving the state are likely to affect investors' perceptions about the state's creditworthiness. Non-state conflicts are less likely to divert state resources or threaten regime change. Finally, violence against civilians, while perhaps damaging for a nation's international image, is unlikely to alter a rational investor's perception of credit risk.

The remaining panels of Figure 2 support this hypothesis. In the top-right, we consider only state-involved conflicts. Here, we observe a parallel pre-trend, followed by a clear negative trend in bond prices after onset. The result is a 1.2-point reduction in bond prices after just 15 days, which persists for the remainder of the estimation window.[14] The post-event coefficients are nearly all negative and significant while the pre-event estimates are zero. The bottom panel shows the event-study plots for non-state and one-sided conflicts, respectively. While both plots present clearly parallel trends, there is no evidence of significant effects.

These parallel trends may seem surprising at first glance, since we might expect investors to be price in new information as it is revealed in the lead up to the outbreak of violence. However, this information may not yet be readily available to the market. Figure A2 plots average daily news articles across conflicts for each conflict type. There is minimal pre-event news coverage of the conflicts in our sample, and a large jump in coverage on the onset date and then ultimately reverts to it's pre-event mean after roughly 30 days. The patterns suggest that conflicts are not fully priced-in ex-ante because they are a surprising to the news media and therefore to investors. This evidence suggests investors are responding rationally given the information available. As more information comes out over time, investors learn and price in a larger effect. We discuss role of news in the learning process in substantially more detail in Section 5.5.

Another potential explanation instead of learning is that the underlying default risk is actually changing day-to-day as conflict develops. This is unlikely for two reasons. First, as we show in Section 4.2, credit risk responds to conflict on the order of several months, while markets anticipate this and respond on the order of just a few days. Second, Figure A3 shows that violence in conflicts tends to spike irregularly over time, rather than unfurl gradually. In the thirty day post-event window, only the onset date experiences meaningful amounts

[14] In Figure A1, we consider an alternative specification, the interrupted time-series (Hausman and Rapson, 2018). Using all state-involved events, we estimate separate quadratic time trends for treated vs. control bonds before vs. after $k = 0$. We find a statistically significant drop in bond prices at the onset day in treated but not control bonds, which grows over the post-event period, consistent with the dynamic path in Figure 2.

of violence in our sample. It is therefore unlikely that conflict severity – and default risk – changes materially in just the first two weeks of conflict.

Table 1 provides the corresponding daily average estimates for these four specifications, collapsing the relative time variables into a single post-treatment indicator. Conflict onset has no significant effect on bond prices in aggregate in column (1). Though the point estimate is negative, the magnitude is very small, corresponding to a 0.1% average daily reduction in trading prices. This effect grows substantially, however, when we restrict to conflicts involving state actors in column (2). State conflict onset leads to a 0.7% average daily reduction in trading prices relative to par, significant at the 1% level. Columns (3) and (4) show that the average effects for non-state and civilian conflicts are small and statistically insignificant.

Table 1: Conflict onset and bond prices

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
	(1)	(2)	(3)	(4)
Post × Treated	-0.096	-0.701***	0.076	0.138
	(0.166)	(0.224)	(0.230)	(0.302)
Bond × Event FE	Yes	Yes	Yes	Yes
Day × Event × Maturity FE	Yes	Yes	Yes	Yes
Events	313	91	159	63
Conflicts	262	78	128	56
Countries	120	106	95	105
Observations	4,396,362	1,282,145	2,218,918	895,299

Note: Standard errors in parentheses clustered at the country level. Sample is daily bond panel in stacked event-specific datasets. Outcome variable is the daily bond trading price averaged across all available exchanges, indexed to 100 (par). Each column provides estimates of treatment effects for a different sample of conflicts, indicated in the table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

One concern is that the number of treated countries in Table A2 may be too small for standard cluster-robust inference (Cameron et al., 2008). To address this, we use wild cluster bootstrapping inference in Table A3. The estimate in column (2) retains its significance, while all others remain insignificant. To test whether investors respond to incidents that may negatively tarnish a state's international reputation but are unlikely to be payoff-relevant, we restrict the sample of one-sided conflict events to those involving only state actors and civilians in Table A4; the average effect is negative but small and statistically insignificant.

Tests of pre-trends such as those in Figure 2 may be underpowered, so that large violations of parallel trends may not be detected as significant pre-trends (Roth, 2022). In Figure A4, we consider the sensitivity of our results to linear pre-trends at different levels of statistical power. Linear trends detected with 80% power still yield a substantial effect size for τ_{15}, the maximal event-study coefficient, even after accounting for pre-test bias. We also conduct non-inferiority tests (Dette and Schumann, 2020) that are able to reject the null of pre-trends for all violations greater than 0.4. We discuss more extensive robustness tests in Section 6.

4.2 Conflict onset and credit risk

We interpret the reduction in bond prices after state conflict onset as reflecting investors' perceptions of an increase in underlying credit risk. Plausibly, state-involved conflict both destroys output and thus tax revenue, and also increases government expenditures, hampering timely repayment of debt. In Figure 3, we test whether debt restructurings rise after conflict onset. We estimate equation (1) using monthly data on sovereign defaults and restructurings from Asonuma et al. (2017). Our outcome variable is a restructuring indicator[15] and the stacked model includes country-event and month-event fixed effects. Figure 3 plots the event-study coefficients. In the full sample, conflict onset makes a restructuring significantly more likely, driven by state conflicts. In the top-right panel, conflict increases the probability of a restructuring by an average of 4 percentage points 15 months after onset. This effect is economically meaningful, representing 8 times the average monthly incidence across our sample (0.5 p.p.).[16] In the remaining panels, we observe null effects for other types of conflict. Only conflicts involving state forces substantially reduce government creditworthiness.[17]

Table A5 provides additional evidence that the bond price effects are driven by investors' perception of credit risk. Here, we test whether conflict-induced capital flight is more pronounced for countries with higher credit risk, as measured by the most recent sovereign credit rating at the time of the conflict.[18] For the sample of state-involved conflicts, we interact the

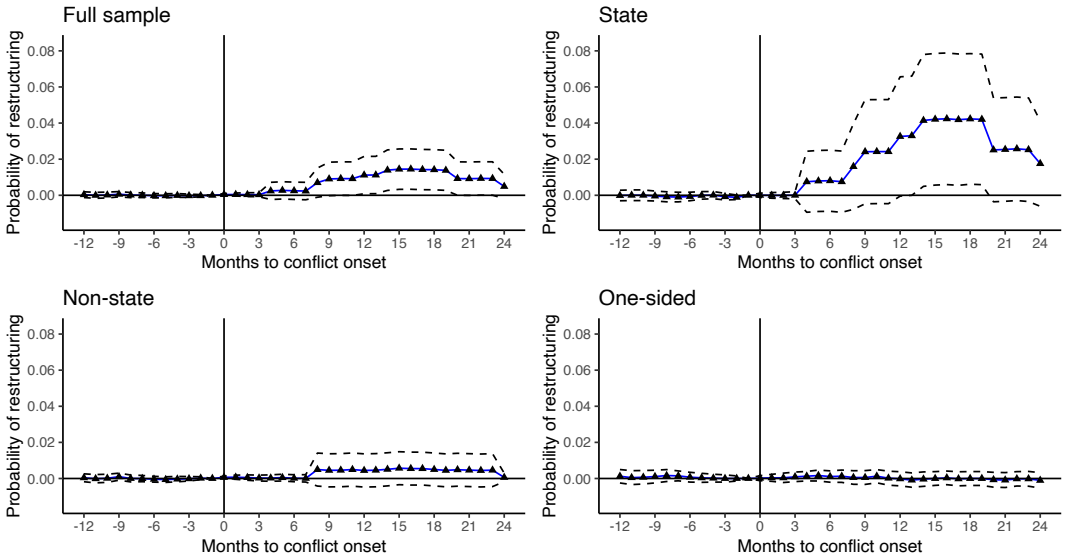
[15] As in Asonuma et al. (2017), we define restructurings as missed payments after negotiations with creditors.

[16] In examining the underlying macroeconomic mechanisms for this increase in credit risk, Tables A6 and A8 show positive impacts of conflict onset on military spending and inflation, respectively, while Table A7 shows no effect on non-military spending. These models use the same stacked estimation method as the main regression, except at the country-year level.

[17] We also tested the effect on defaults *or* restructurings. The results somewhat weaker, since the main effect is driven only by restructurings and not defaults.

[18] We take an average of the ratings across the three major rating agencies: Moody's, Fitch, and S&P. Evidence

Figure 3: Event-study: debt restructurings



Note: Figure shows coefficients from stacked event-study regressions described in Section 3 on monthly weak preemptive restructurings as defined by Asonuma et al. (2017) for four different samples of conflicts, indicated in each subfigure title. Standard errors are clustered at the country level. Outcome is the monthly dummy indicator of a weak restructuring. Specifications include interacted event-specific two-way fixed effects.

treatment indicator of our main specification with either the linear rating value, ranging from 1 to 21, (columns 1-2) or an indicator variable if the sovereign is rated "investment grade" (columns 3-4).[19]

In the linear ratings specification of column (1), affected bonds in the lowest possible non-default rating class (2) trade at a 1.4 point discount after conflict onset, over four times larger than the discount of just 0.34 for the highest-rated bonds (21). Similar results obtain for the investment grade bond comparison in column (3): conflict impacts are -0.89 for non speculative grade bonds, but just 0.16 and not significantly different from zero for investment grade. The logic is straightforward: poorly rated bonds are ex-ante closer to the default threshold, and so investors believe that conflict onset might push them over. Better rated bonds, instead, have more slack to weather the storm. Outbreaks of violent conflict appear to produce similar flight to quality responses as in other rare macroeconomic crises (Gatev and Strahan, 2006;

suggests that credit ratings exert an important effect on the pricing of sovereign bonds (Kapstein et al., 2024).

[19]We use the standard definition for investment grade, which is all sovereigns rated triple-B or better.

Caballero and Krishnamurthy, 2008).

4.3 Benchmarking the effect size

The estimate in Table 1 reveals that state-involved conflict reduces daily bond prices by 0.7 points on average. But is this effect quantitatively meaningful? We investigate this question using a simple bond pricing model in Appendix A. Here, we provide only the key parameters and predictions of the model. In the model, inspired by the set-up in Barberis et al. (1998), risk-neutral investors hold a fixed-income security with a given term structure. In any given period t, with probability ζ_t the sovereign issuing this bond is affected by conflict, incurring an expected loss γ, the economic cost of conflict.

The model provides a simple expression for the impact of conflict onset on bond prices

$$\tau = (\tilde{\zeta}_0 - \zeta_0)[EV(1) - EV(0)] \tag{2}$$

Where ζ_0 is the market's prior belief on the probability of conflict while $\tilde{\zeta}_0$ is the belief after receiving a signal – i.e., observing news reports on the initiation of fighting in the bond-issuing country. $EV(1) - EV(0) < 0$ is the difference in net present expected value of the bond in the conflict and non-conflict states; its absolute value is increasing in γ.

All else equal, the market response is decreasing in ζ_0, the prior belief. This gives a formal definition for the notion that conflict is "priced in". If the market already holds a high prior belief on the likelihood of conflict, it will not be surprised when it occurs. Conversely, for given priors, the response is increasing in $\tilde{\zeta}_0$, the posterior belief after conflict onset. This is driven by the strength (and precision) of information about the conflict after its onset, with $\tilde{\zeta}_0 = 1$ representing the full-information case. If markets instead only observe weak signals that conflict has begun, then $\tilde{\zeta}_0$ is close to ζ_0 and there is a muted response. Finally, the magnitude of the market response depends positively on γ, which determines the gap in payoffs $EV(1) - EV(0)$. If conflict is expected to be costly, markets should respond more.

The model facilitates two useful exercises. First, it provides empirically testable predictions on conflict-specific heterogeneous effects. Rational, well-informed traders should respond more to conflicts $i)$ with greater economic cost (γ), $ii)$ to conflicts that were unexpected (ζ_0), and $iii)$ to conflicts where the flow of information is greater, moving $\tilde{\zeta}_0$ toward 1. Put an-

other way, if markets *do* respond in these ways, it suggests investors are forming reasonable beliefs about these parameters. We test these hypotheses in Section 5.

Second, the model allows us to quantitatively benchmark the empirical effect size. In Appendix A we estimate the key parameters of the model using data on the frequency and economic impact of state-involved conflict. With these parameters in hand, we then simulate the effects implied by equation 2 in the first-best case of the model, where markets have full information ($\tilde{\zeta}_0 = 1$) and empirically correct beliefs ($\gamma = \hat{\gamma}, \zeta_0 = \hat{\zeta}_0$). We simulate that, at its maximal point in Figure 2 at day 15, the empirical effect size is only 76% of the net present cost of conflict. As the model suggests, this could be driven by weak information and incomplete updating, underestimation of the cost of conflict, or overly pessimistic prior beliefs, though these forces cannot directly separated through the model. Of course, these are by no means the only explanations for this "underpricing" result. For example, if conflict onset generates a large increase in the variance of returns and information will be revealed relatively quickly, there may be option value to holding conflict bonds. As such, we are cautious not to over-interpret the model, but rather use it as a benchmark case, and use its simple and intuitive predictions to guide the empirical exercises of Section 5.

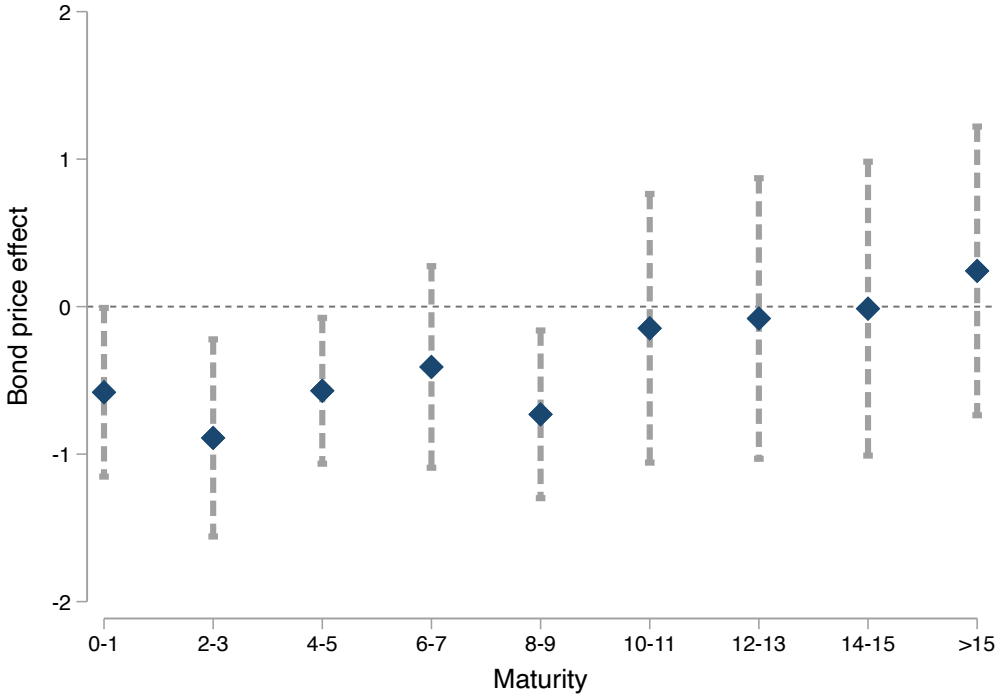
4.4 Time to maturity effects

One insight of the bond pricing model is that the impact of conflict onset depends on bond maturity. Figure A5 shows the dynamics of rational beliefs over annual conflict probability if conflict follows a stationary AR(1) process.[20] Observing conflict today is strongly predictive of conflict over the short term, but holds minimal predictive value for conflict far into the future. Therefore, investors holding long maturity bonds should respond less to conflict onset, since the cost of the shock relative to the lifetime value of the bond is lower; the face value will be repaid well after the likelihood of conflict has reverted to its long-run mean. Instead, short bonds that come due soon after conflict onset face an increased probability of losses on the face value as default risk spikes. This long-short heterogeneity arises directly from the AR(1) process and is therefore an implicit test of the conflict lag structure.

We test this hypothesis non-parametrically by estimating the main event-study regression, adding interactions between the time-varying treatment variable and indicators for bond

[20] We estimate conflict autocorrelation in Table A10, column (5).

Figure 4: Heterogeneous effects by bond maturity



Note: Figure shows average effects of from an augmented specification of equation 1 where the treatment variable is interacted with indicators for each maturity bin. Caps show 95% confidence intervals on each bin-specific estimate. Sample is all state-involved conflicts. Maturity is measured as the (rounded) number of years between the current trading date and the bond's maturity date, with negative values bottom-coded to zero.

time-to-maturity bins. Maturity bins are formed from 0-15 in two-year intervals, with all bonds longer than 15 years included in the final category. The results for the sample of state-involved conflicts are in 4. The estimates reveal that, as expected, short bonds respond the most to conflict – particularly those under ten years. Below this ten year threshold, the effects are robustly negative and significant, while above this threshold they converge to and are not statistically distinguishable from zero. Quantitatively, the average daily effect of conflict for 2-3 year bonds is 0.89, significant at 5%, rising 0.24 and not statistically significant for bonds with maturities greater than 15 years.

Corresponding estimates for each of the four types of conflicts are in Table A12. The results now reveal that among the shortest maturities, the negative effect of conflict on prices

becomes significant in columns (1)-(3) – the full sample, state, and non-state conflicts, respectively – though it remains zero in the one-sided conflicts of column (4). For each of columns (1)-(3), the size of the interaction terms increases in bond maturity, though these estimates of the differential effects are not always statistically significant.

5 Heterogeneity: investor beliefs

Markets should respond more to conflicts that are surprising, costly, and where signals are more informative. In this section, we interrogate whether investors respond to observable conflict and news media characteristics in ways that are consistent with reasonable beliefs about ζ_0, γ, and $\tilde{\zeta}_0$.

5.1 Beliefs about conflict risk

We use conflict history to study how investors form country-specific conflict risk priors. As a first pass, we split the data by initial and subsequent conflict events. The initial episode of a conflict may encounter investors with priors closer to average, provoking more updating and a larger price response. Future episodes of that conflict should encounter more pessimistic priors, and are as a result already priced in. Table A13 estimates the state conflict effect in each event subsample and finds strong evidence for these patterns. Initial episodes cause bonds to lose 0.94% of their value relative to par on average daily, significant at 1%. Subsequent events do not elicit the same investor behavior, with point estimates near zero.

More systematically, we estimate conflict event-specific priors by calculating the share of the $T \in [5, 10, 15, 20]$ years prior to the event-date in which the treated country experienced state-involved conflict. Figure A7 estimates the event study equation, splitting the sample by events occurring in countries with a ten-year prior above (left panel) or equal to zero (right panel). Dynamic estimates are negative and significant for events in countries without recent conflict history, but absent for countries with a history of violence. Holding a bond for 20 days following a surprising conflict event results in nearly a 2% loss in value, relative to par.

Table 2 estimates the average daily effects, using continuous variables of prior conflict probability. We further disaggregate country conflict history into major and minor conflicts.[21]

[21] Per UCDP-GED, minor conflict is defined as 25-999 battle-related deaths in a given year, while major conflicts

Column headers give the choices for T. Across specifications, the results indicate that events in countries with no conflict history reduce bond prices by 1.34-1.65%, relative to par, roughly 91-135% larger than the average effects in Table 1 column (2). Differential effects are stronger in both magnitude and significance for recent, severe conflicts. In particular, the largest effect is the 10-year probability of a major conflict in column (4). Moving from a probability of 0 to 1 in major conflict over the past ten years offsets the entire reduction in bond prices among non-conflict countries. For countries with the most troubled recent conflict history, the implied effect is very near zero, so that conflict onset is fully priced in. The results imply that investors form sophisticated event-specific priors using countries' conflict history, upweighting larger and more recent wars, consistent with adaptive belief formation (Haruvy et al., 2007).

Table 2: Conflict onset and bond prices: conflict history

Dependent variable	Bond price							
Conflict index	5-year		10-year		15-year		20-year	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	-1.647**	-1.632**	-1.481**	-1.506**	-1.369*	-1.440**	-1.337*	-1.448**
	(0.687)	(0.675)	(0.701)	(0.688)	(0.710)	(0.699)	(0.707)	(0.697)
Post × Treated × Conflict index	1.112		0.858		0.699		0.666	
	(0.762)		(0.805)		(0.832)		(0.855)	
Post × Treated × Minor conflict index		0.920		0.702		0.550		0.847
		(0.742)		(0.803)		(0.926)		(0.836)
Post × Treated × Major conflict index		1.357***		1.632**		1.889		0.557
		(0.459)		(0.685)		(1.677)		(1.816)
Bond × event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day × event × maturity FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	935,938	935,938	935,938	935,938	935,938	935,938	935,938	935,938

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is the first event of all conflicts involving state forces. Conflict index is the share of years in the previous T years in which the country experienced a government-involved conflict. Major conflict defined as more than 1000 battle-related deaths in a given year; minor exceeds 25 deaths. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.2 Beliefs about conflict costs

The market response to a conflict event increases in the perceived cost of conflict γ. In this section, we ask how investors form expectations of γ given available information. We test for heterogeneous effects by several initially observable conflict characteristics relevant for costs: i) fatalities on the day of the onset event, ii) distance to the capital, and iii) the stated political aims of non-state actors. The logic behind i is straightforward – larger fatality counts suggest a more severe outbreak of conflict. For ii and iii, we argue that geographically remote

have least 1,000 battle-related deaths.

and/or separatist (non center-seeking) conflicts do not pose a direct threat to the stability of the current regime, and therefore portend smaller expected losses for creditors.

Table 3: Conflict onset and bond prices: fatalities

Dependent variable	Bond price					
Fatality quintile		1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	-0.208	-0.701***	-0.629**	-0.950***	-1.204***	-2.462***
	(0.196)	(0.224)	(0.275)	(0.257)	(0.375)	(0.452)
Post × Treated × Fatalities	-0.013***					
	(0.001)					
Bond × event FE	Yes	Yes	Yes	Yes	Yes	Yes
Day × event × maturity FE	Yes	Yes	Yes	Yes	Yes	Yes
Events	91	91	66	52	36	18
Countries	106	106	103	95	87	79
Observations	1,282,145	1,282,145	948,083	747,255	517,003	271,493

Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict events involving state forces. Column (1) uses the full sample, while columns (2)-(6) use subsamples of all events with fatalities greater than quintiles. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3 tests heterogeneity in deadliness. Column (1) interacts the treatment indicator with the number of fatalities recorded on the day of the event. Zero-fatality events have no significant effect on bond prices, but each additional event fatality is associated with a 0.013-point decrease in trading prices. Using these linear effects, a conflict onset event 1SD more deadly than the mean state-involved event predicts a 1.07-point decrease in bond prices. Columns (2)-(6) estimate the model the model for events where the number of fatalities is greater than or equal to the quintile indicated in the table header. Though coefficients are monotonically increasing in fatalities, the effects appear convex rather than linear. The linear predicted effect of the average in the 5th quintile is 1.70, while the observed effect in column (6) is 2.46. Figure A8 shows the corresponding event-studies; pre-trends are broadly parallel for all quintiles, but higher fatality events exhibit a sharper initial drop in bond prices.

Next, we identify the location of each conflict onset event and calculate its distance to the country capital. We hypothesize that remote events are considered less payoff-relevant by investors, while proximate events may directly threaten regime stability and are associated with high conflict costs. We split the sample by thresholds of distance to the capital – 25, 50, 75, and 100 miles – and estimate treatment effects above and below these thresholds. Table

4 contains the results of this analysis. In column (1), the treatment effect is 1.95 points for events within 25 miles of the capital and only 0.53 for events further away, both significant at 5%.[22] However, these gaps narrow as the threshold rises. By 75 miles (3), the effects above and below the threshold both stabilize around the full-sample effect of 0.7. This suggests a nonlinear pattern where events very close to the capital have outsize effects, while those further away converge rapidly to the full sample estimate.[23] This pattern is consistent with expected costs that are convex in distance, with investors pricing in a discrete jump in the likelihood of state collapse for the most proximate events.

Table 4: Conflict onset and bond prices: distance to capital

Dependent variable	Bond price			
Cutoff (miles)	25	50	75	100
	(1)	(2)	(3)	(4)
Below cutoff	-1.953**	-1.751**	-0.716	-0.675
	(0.874)	(0.812)	(0.705)	(0.649)
Above cutoff	-0.526**	-0.544**	-0.697***	-0.709***
	(0.216)	(0.216)	(0.203)	(0.199)
Difference	-1.426	-1.207	-0.019	0.034
p-value	0.087	0.118	0.979	0.959
Observations	1,282,145	1,282,145	1,282,145	1,282,145
Bond × Event FE	Yes	Yes	Yes	Yes
Day × Event × Maturity FE	Yes	Yes	Yes	Yes

Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflicts involving state forces. Estimates provide effects of conflict onset above and below different thresholds of distance to capital, as indicated in table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Finally, we consider whether investors understand and respond to the underlying political dynamics of conflict. We classify state conflict events into three groups: i) conflicts fought between a government and rebel over the state (center-seeking), ii) conflicts fought between a government and rebel over a subnational territory (regional), and iii) conflicts of any political motivation in which another country's government or territory is contested (spillover).[24]

[22] The differential effect, 1.43, is significant at 10%.

[23] Table A14 shows that interaction effects using the log of distance, while positive, are not significant. Table A15 shows the interaction effect using the log of a population-weighted average distance to the next four major cities after the capital. The estimate is positive but much smaller, and not statistically significant.

[24] For example, incidents between Boko Haram and the Nigerian government that take place in Cameroon, or interventions by e.g. the government of Turkey against jihadist forces in Iraq.

Table 5: Conflict onset and bond prices: conflict type

Dependent variable	Bond price				
Event-type	Spillover	Own			All
Conflict-type	All	All	Center	Separatist	All
	(1)	(2)	(3)	(4)	(5)
Post × Treated	-0.261	-0.880***	-1.967**	-0.534**	-1.967**
	(0.246)	(0.260)	(0.858)	(0.245)	(0.857)
Post × Treated × Regional					1.433
					(0.889)
Post × Treated × Spillover					1.706**
					(0.858)
Bond × event FE	Yes	Yes	Yes	Yes	Yes
Day × event × maturity FE	Yes	Yes	Yes	Yes	Yes
Observations	375979	906166	239940	666226	1282145

Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflicts involving state forces. Estimates provide effects of conflict onset for event subsamples, as indicated in table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5 examines heterogeneous effects by conflict type. In column (1), average daily effects for spillover conflicts are small and statistically insignificant, indicating that investors are at the very least aware of the primary parties to a conflict and do not expect outside conflicts to substantially affect a country's creditworthiness. Column (2) restricts to own-country state conflicts, and finds that the average effect rises to 0.88, a 25% increase relative to the full sample. Columns (3) and (4) maintain the own-country restriction, but further split the sample into center-seeking and separatist conflicts. Investors are aware of the underlying political divisions driving conflicts; investors react primarily to center-seeking civil wars, with the effect rising to 1.97. Local conflicts, while economically costly, may not pose the same risk of state collapse, and therefore see smaller, though still significant, investor responses, at 0.53.

5.3 Benchmarking cost beliefs

Bond investors price information signaling that a conflict is likely to be economically costly. In this section, we conduct another benchmarking exercise to understand quantitatively how large these responses should be in the benchmark full-information model in Appendix A, and what investor responses reveal about their beliefs. We can allow linear

heterogeneity in the cost parameter γ, such that it depends on conflict characteristics:

$$\gamma_{it} = \gamma_0 + \gamma_1 f_{it} + \gamma_2 c_{it} + \gamma_3 d_{it}^{50} \tag{3}$$

Where f_{it} is the average number of fatalities per conflict-day, c_{it} is an indicator for center-seeking conflicts, and d_{it}^{50} is an indicator for any attacks within 50 miles of the capital.

We compare investor responses to model-simulated responses as follows. First we estimate an interacted conflict cost model to obtain the the γ_j – the impact of each conflict cost factor j on GDP. We then plug the estimated γ_j coefficients into the pricing model to predict benchmark full-information market responses along each dimension of conflict heterogeneity. Lastly, we estimate our bond market event-study with the same interactions and compare these observed responses with the predicted heterogeneous effects τ_j. However, we restrict the post-event sample here to $k \geq 15$ to capture only the long-run bond market effect.

Table 6 presents results for heterogeneity in conflict cost (Panel A) and in the bond market (Panel B). In columns (1)-(3), we include each heterogeneity variable separately, while column (4) includes all simultaneously to account for correlation in the cost information received by investors. The results suggest that investor beliefs are at least directionally correct. In Panel A, deadlier conflict, clashes near the capital, and center-seeking rebellions are all associated with significantly greater economic costs, relative to baseline conflicts lacking these characteristics. Importantly, the information is correlated; the magnitude of each coefficient falls in the conditional specification in (5), most substantially for center-seeking indicator, which loses significance. Conflicts that threaten the capital reduce income by 6.8-7.8% relative to baseline, suggestive of large nonlinear costs of state collapse in civil war.

The results in Panel B largely reprise what we learned Section 5.2, but it is informative to compare magnitudes with Panel A. Across all specifications, the magnitude of market responses are ordered identically to the cost effects in Panel A – the most payoff-relevant information provokes the largest response. Furthermore, column (4) suggests that investors partially account for correlation across cost signals. In the conditional model, the center-seeking response falls, while the response to fatalities does not change, exactly as in Panel A. However, the response to battles near the capital grows, even as its association with income falls after accounting for correlated information. This pattern suggests that investors exhibit

Table 6: Heterogeneous effects

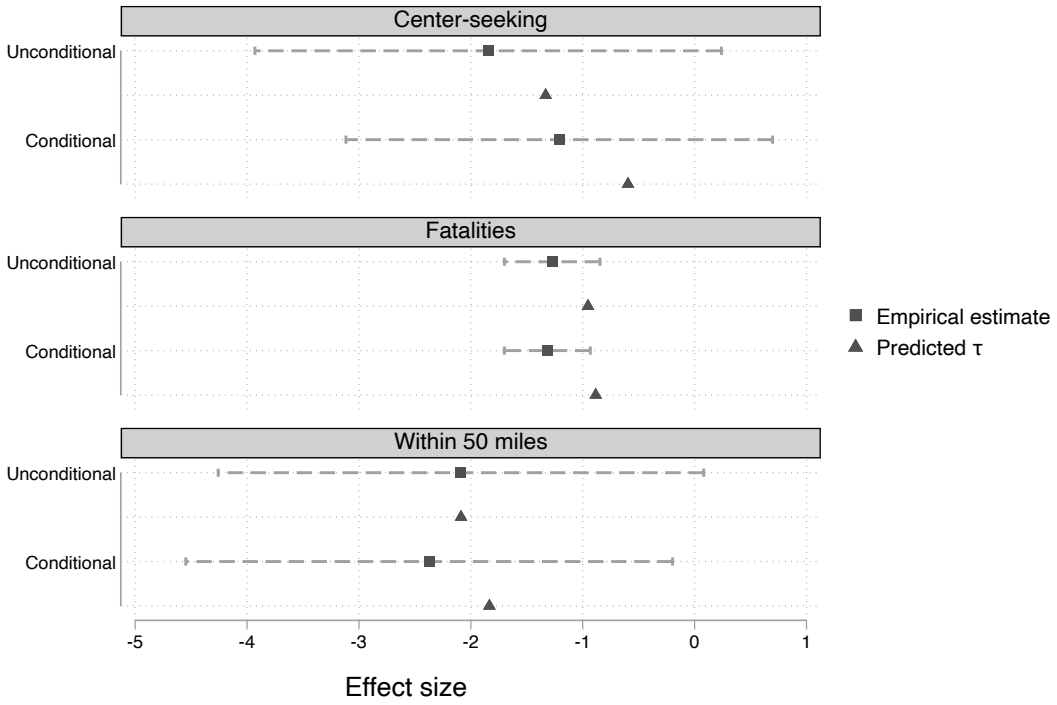
	(1)	(2)	(3)	(4)
Panel A: Conflict cost	$\log(GDP)$			
Conflict	-0.036	-0.009	-0.023	0.002
	(0.027)	(0.027)	(0.024)	(0.025)
Conflict × Fatalities	-0.035**			-0.033**
	(0.017)			(0.016)
Conflict × Within 50 miles		-0.078***		-0.068**
		(0.028)		(0.028)
Conflict × Center-seeking			-0.050*	-0.022
			(0.030)	(0.031)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5928	5928	5928	5928
Panel B: Bond Market	Bond price			
Post × Treated	-0.251	-0.749**	-0.491	0.272
	(0.318)	(0.316)	(0.301)	(0.425)
Post × Treated × Fatalities	-1.273***			-1.316***
	(0.218)			(0.196)
Post × Treated × Within 50 miles		-2.089*		-2.372**
		(1.107)		(1.110)
Post × Treated × Center-seeking			-1.845*	-1.210
			(1.064)	(0.973)
Bond × event FE	Yes	Yes	Yes	Yes
Day × event × maturity FE	Yes	Yes	Yes	Yes
Observations	966222	966222	966222	966222

Note: Standard errors in parentheses clustered at the country level. Outcome variable is either $\log(GDP)$ (A) or the daily bond price (B). Event sample is all conflicts involving state forces. Sample is either the country-year panel (A) or the stacked bond panel (B). Fatalities are measured in hundreds. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

correlation neglect on this dimension of belief formation (Enke and Zimmermann, 2017).

But what do these magnitudes say about the accuracy of investors' beliefs? Figure 5 plots the empirical bond price effects in Panel B alongside the model-implied differential treatment effects from the bond pricing model for each γ_j estimated in Panel A. Across the board, predicted responses are very close to the estimates ones, with none excluded from the confidence intervals. However, the results suggest that investors slightly over-respond to center-seeking and deadlier conflicts. Remarkably, the unconditional effect of capital conflict is nearly iden-

Figure 5: Pricing conflict cost information



Note: Figure plots estimated heterogeneous effects of conflict onset on daily bond prices from stacked event-study regressions described in Section 3 for three different conflict characteristics, indicated in subfigure headers. All specifications estimate long-run average effects ($k \geq 15$) and include interacted bond maturity effects. Standard errors are clustered at the country level. Figure also plots the benchmark model-predicted heterogeneous treatment effect τ implied by the empirical marginal effect of characteristic j on conflict cost, γ_j in Table 6. Effects are simulated assuming full information and accurate priors. Conditional specifications include all conflict features simultaneously while unconditional include each heterogeneous variable separately.

tical to its predicted value. However, consistent with mild correlation neglect, the gap grows when other variables are conditioned on. Taken together, the results show that heterogeneous market responses are both directionally correct and imply reasonably accurate beliefs about conflict costs, especially considering the difficulties of information aggregation and inference in highly uncertain wartime environments.

5.4 Conflict duration effects

Clearly, investors should respond more if they expect conflict to last longer. But are investors able to identify which conflicts will ultimately be long-lasting? In other words, can

markets predict the ultimate outcomes of conflict? To investigate this question, we assemble data from UCDP on conflict termination dates. Only 35 state-involved conflicts out of the total of 78 have defined termination dates, while the remainder are ongoing, as of writing. For this sub-sample, then, we calculate the total conflict duration in years and interact this with the treatment in our event-study specification. We also control for interactions with the conflict cost variables included in Table 6, since these characteristics might be correlated with duration.

The results are in Table 7, column (1). On average, the market responds more to long-lasting conflicts. For each additional year in conflict, the market prices in an additional 0.16 point discount on the bond. Quantitatively, the effect at the 10th percentile of conflict duration is just -0.3, while at the 90th percentile it rises nearly threefold to -0.9. Directionally, the market is able to predict the duration of conflict well before its conclusion, as these effects are estimated only within the first 30 days of fighting. Furthermore, since the overall sample is composed of relatively recent conflicts, we only observe the subset of events that terminated the fastest. With a longer time horizon, the contrast in responses to long or short conflicts is likely to be much larger.

Table 7: Conflict onset and bond prices: conflict duration

Dependent variable	Bond price				
Bond maturity	All	0-4	5-9	10-14	15+
	(1)	(2)	(3)	(4)	(5)
Post × Treated	0.930**	0.190	1.324*	2.094***	2.203***
	(0.442)	(0.442)	(0.713)	(0.682)	(0.551)
Post × Treated × Conflict duration	-0.160*	-0.094	-0.118	-1.055***	-0.712***
	(0.082)	(0.115)	(0.111)	(0.317)	(0.203)
Bond × event FE	Yes	Yes	Yes	Yes	Yes
Day × event FE	Yes	Yes	Yes	Yes	Yes
Observations	594407	272972	185983	53373	81958
R^2	0.976	0.969	0.972	0.969	0.988

Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict events involving state forces. Conflict duration is measured in years. All models control for the interactions between treatment and total fatalities as well as an indicator for violent events in the capital city. Column (1) uses the full sample, while columns (2)-(5) use subsamples of all bonds with maturity of the level indicated in table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In addition, Table 7 reveals a stark maturity profile to the market's prediction of ultimate

conflict duration. Intuitively, only long bonds should respond to future conflict duration, since short maturities will be exposed to the conflict risk regardless of its duration. We test for this differential duration response in columns (2)-(5). Indeed, markets do not significantly respond to conflict duration in the short maturities of columns (2) and (3). However, for bonds with over 10 years remaining to maturity, the duration effects are much more pronounced and significant at 5%: an additional year of conflict magnifies the effect by 0.71-1.06 points. For bonds of over ten years, the market response to conflict at the 90th percentile of duration rises to 2.3 points daily. This maturity-dependent response also suggests that it is not bond market responses *causing* longer conflicts by generating higher yield spreads, since there is no reason to believe that effect would be limited to long term bonds.

5.5 News coverage and information dissemination

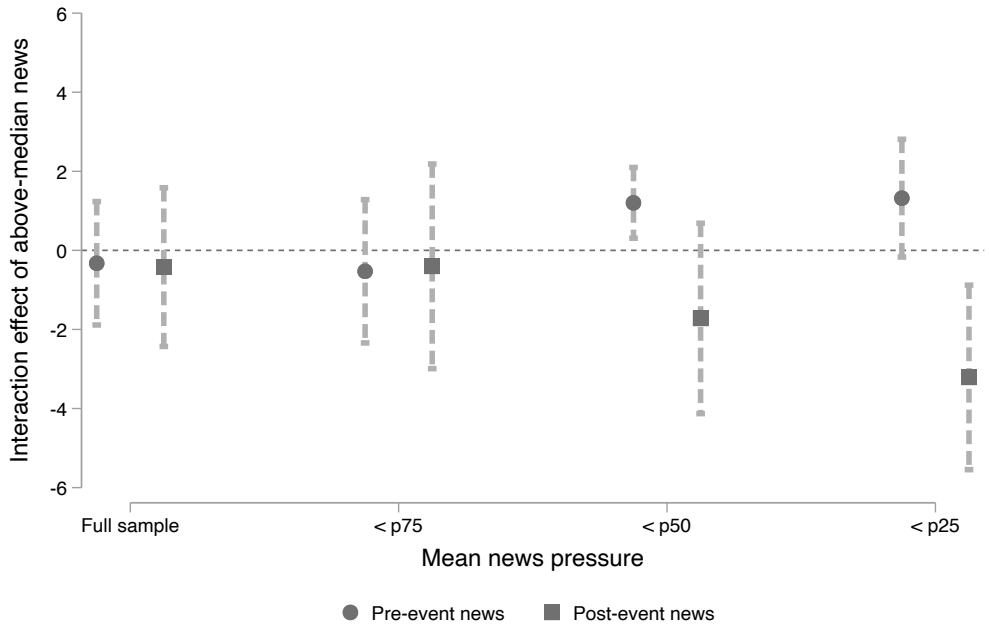
The evidence presented in Section 5 suggest that investors form reasonably accurate, country-specific beliefs about both the cost of conflict γ and its prior probability ζ_0 by observing conflict characteristics and country histories. But how do investors obtain information about the state of the world ζ? Does the quantity of conflict-specific media coverage that the market is exposed to affect the extent of belief updating in the short-run?

Under rational Bayesian learning, $\zeta_0 \to 1$ as the market receives signals that conflict is imminent. We therefore expect that the overall quantity of *pre-event* news media coverage should *reduce* the effect of conflict onset on prices by contributing to the updating of ζ_0 over time. In other words, better-covered conflicts will have more of the expected conflict loss priced in ex-ante. Instead, news coverage *after* the conflict event moves $\tilde{\zeta}_0 \to 1$, the full-information benchmark, as investors receive signals that conflict has arrived. Therefore, holding pre-event coverage constant, conflicts with more *post-event* coverage should observe *larger* price effects.

Moreover, these conflict-specific news flows interact with the broader media environment. A substantial literature shows that high profile events can crowd out attention to other events, for example, natural disasters (Eisensee and Strömberg, 2007) and violent conflict (Durante and Zhuravskaya, 2018). This has implications for asset pricing when traders exhibit limited attention (Peress and Schmidt, 2020; Israeli et al., 2022). We argue that this crowd out effect mediates investor responses to conflict news. Where investor attention is monopolized by a news-heavy environment, the impacts of conflict news on pricing are likely to be muted.

Instead, conflicts that occur during "news droughts" are more likely to exhibit Bayesian responses to information flows. Following the literature, we measure the overall quantity of news using "daily news pressure" – the median number of minutes devoted to the top three news segments across news broadcasts in a given day (Eisensee and Strömberg, 2007).

Figure 6: Impact of news on market response by news pressure



Note: Figure shows interaction estimates from event-study regressions where time-varying treatment is interacted with indicators of above-median pre-event ($\tau \in [-30, 0)$) and post-event ($\tau \in [0, 15]$) news coverage. Estimated models include only data for days $\tau < 0$ and $\tau > 15$. These models are estimated on four different conflict subsamples, indicated on the categorical axis: i) the full sample, ii) below the third quartile of news pressure, iii) below median news pressure, and iv) bottom quartile news pressure. News pressure is taken from Eisensee and Strömberg (2007) and averaged daily by conflict over the whole event window.

Figure 6 investigates how the media environment shapes investor learning about conflict. We classify all conflicts as above or below median across two dimensions: the number of news articles in the 30 days before the onset date (pre-onset coverage), and the number of articles in the 15 days after (post-onset coverage). We then interact these variables with the treatment in the main specification fixed effects specification. Finally, we calculate the average daily news pressure by conflict for the entire event window, and then split the sample in columns (2)-(4)

based on quartiles of average daily news pressure.[25]

In the full sample, there is no significant relationship between news coverage and market responses to conflict. However, once we restrict the sample to conflicts that happen to occur in periods of low news pressure, evidence of investor learning emerges. When we restrict sample to conflicts with average daily news pressure below the median and bottom quartile, respectively, we see statistically significant positive coefficients on the interaction with the pre-onset news indicator, and statistically significant negative coefficients for post-onset news, of similar magnitude.

To summarize, in state-involved conflicts where news coverage is *not* crowded out, investors rationally update beliefs on pre- and post- onset media information flows. However, on average, these responses are more muted, suggesting an important role for limited attention in pricing conflict risk. Since the preceding analysis suggests that investors are able to form reasonable expectations of γ and ζ_0, it may be that this limited attention effect is responsible for weak updating, explaining the aggregate market under-reaction to conflict.

The corresponding estimates are available in Table A16. Additionally, Table A17 controls for interactions between treatment and conflict cost factors considered in Table 6, since news coverage of a conflict may well be correlated with its costliness. The patterns of investor learning by news pressure environment remain robust.

6 Identification threats and robustness

6.1 Endogeneity

The identifying assumption for equation (1) to deliver a causal effect is that counterfactual trends in conflict-affected bonds evolve in parallel to unaffected ones in the absence of conflict. We interrogate this assumption with the event-study specification, which shows evidence of strongly parallel pre-trends. Still, there are numerous sources of bias that might contaminate the results and yet remain consistent with insignificant pre-event coefficients.

Bond characteristics: If conflict-affected countries issue a different mix of bonds to control countries, this may bias our estimates. Appendix Table A18 includes various combinations of

[25] To account for the fact that the post-event coverage covers the first 15 days after onset, we estimate only the long-run equilibrium price response, that is for the sub-sample $\tau < 0$ or $\tau \geq 15$.

bond-level variables interacted with the δ_{te}: maturity FE, currency FE, maturity $\times$ currency FE, loan size, and coupon rate. The results vary between 0.5-1 across the specifications, and remain significant at 5% or lower in nearly all specifications.

Country characteristics: Conflict-affected countries are poorer, more resource-dependent, and more corrupt. If bond prices display differential trends according to these characteristics over the estimation window, this may bias our effect away from zero. We consider robustness to 10 different country macroeconomic fundamentals in Table A19, including obvious potential confounders such as per-capita income and population, interacting each control variable individually with δ_{te}. Table A20 controls for institutional differences by including interactions with country-level World Bank Governance Indicators, while Table A21 measures country risk with ICRG scores. Finally, Table A22 controls for measures of oil, mineral, and total natural resource rents as a share of GDP. Across all country-level confounders tested, the results are broadly unchanged and remain significant in 38 out of 40 specifications.

Propensity score reweighting: Root causes of conflict are complex and can stem from a combination of social, economic, political, and historical factors. As such, the underlying differences between countries facing conflict and those that don't may be substantial. We account for these potential differences by re-weighting our data with a propensity score (Hirano et al., 2003). We estimate the score using a logistic regression of a conflict indicator on country characteristics, including the logarithm of population, GDP per capita, natural resources rents as percentage of GDP, human development index, and an index of electoral democracy and political rights. After reweighting for the probability of treatment conditional on these characteristics, treated and control groups become more comparable (see Figure A9). Table A23 shows that results are similar to the main estimates.

Macroeconomic shocks: Our specification controls for the impact of common macro shocks on bond prices by using event-by-time fixed effects. However, aggregate shocks, such as changes in global risk appetite or interest rates, are likely to have differential effects across countries (Ahmed et al., 2017). We control for this in Table A24 by including interactions between several global macro indices and our treatment indicator. We consider US equity indices such as the DJIA, NASDAQ, and S&P 500, as well as the VIX to capture market volatility. We also include emerging market equity and bond indices, including the EMBI. We include several major commodity price indices in Table A25. The results are broadly unchanged.

Specification choices: Our stacked event study specification may seem unfamiliar to some readers, as it differs from the standard finance event studies (Mackinlay, 1997) as well as two-way fixed effects event studies (Goodman-Bacon, 2021). However, the method is well studied and delivers a consistent variance-weighted average of event-specific dynamic treatment effects. The stacked estimator is equivalent to a local projections estimator under inverse variance weighting (Dube et al., 2022). The stacked estimator typically outperforms TWFE because it allows for the selection of "clean" controls, solving the negative weights problem (Goodman-Bacon, 2021). It also provides much more transparent identification assumptions than finance event studies on the counterfactual trends of assets unaffected by conflict.

Nonetheless, we confirm out main results for state-involved conflict with five additional empirical strategies. First, we use interrupted time series (Baicker and Svoronos, 2019) in Figure A1. The figure reveals a sharp drop in bond value for conflict countries at $\tau = 0$, with no corresponding change for the treated. Second, we also use propensity score re-weighting on a variety of country characteristics, as decribed above, in Figure A9. Third, we use standard empirical asset pricing techniques to estimate the cumulative abnormal returns of holding a conflict-affected bond in the 20 days after conflict onset. The results, in Figure A10, show a remarkably similar quantitative effect of a roughly 1% short-run loss in bond value from conflict. Fourth, in our stacked specification there may be overlap in event windows within a country, so that a given bond-day shows up as treated for multiple events. To address this issue, we re-estimate the main results in Table A26 using the sample non-overlapping of conflict events. Fifth, we also estimate event-specific event-study coefficients and plot their distribution in Figure A11. The unweighted average is -0.41, while the inverse variance weighted average is -0.83, with a modal estimate of -1. Though there is a wide dispersion, the majority of the coefficients are negative; of these 72% are statistically significant at the 5% level.[26]

6.2 Measurement and sample selection

Several arbitrary measurement assumptions and sample-selection criteria may also affect the results. We consider each of these in turn. In Figure A12 we estimate the main

[26]Some estimates in the right tail of the distribution are positive and significant – more than would be expected from random sampling variation alone. It is not difficult to imagine scenarios that would make this possible; if, for example, conflict settles uncertainty and sovereign bonds feature a risk premium; or if the outbreak of violence signals the strength of the state against rebel threats.

difference-in-differences estimate across all possible event windows contained in +/- 30 interval, and plot a histogram of the estimated coefficients. All estimates across all possible event-windows are negative and significant at the 5% level. In Figure A13, we consider robustness of the main results to different percentile thresholds and measures of event severity for subsequent episodes, including the sum of fatalities in t and $t+1$ or over the entire episode period. Though these definitions substantially change the number and composition of events in the data, the results are remarkably similar across specifications.

Our event timing also aligns well with changes in external political risk ratings, such as those compiled by the International Country Risk Guide (ICRG). In Table A27 and Figure A14 we look at the evolution of monthly ICRG ratings for civil war, internal conflict, economic risk, and the overall composite risk in the wake of our UCDP-defined conflict events. Figure A14 plots monthly event-study coefficients, revealing clear changes in risk for civil war and internal conflict before and after our event dates. Interestingly, Table A27 reveals that the correlation between our conflict events and ICRG conflict risk ratings is reasonably strong and constant across state-involved and non-state conflicts (columns 1-2). However, the economic risk rating (column 3) is *only* correlated with the onset of state conflicts. This evidence is consistent with the argument presented throughout this paper that state conflicts are uniquely costly, prompting greater bond market response.

Table A28 includes region-by-day fixed-effects. Table A29 assesses the role of measurement error induced by imprecision in the UCDP-GED data, finding no evidence that the main effect varies systematically with the level of data precision. To address the role of outliers, in Table A30 we winsorize or drop bond prices at the 5th/95th or 1st/99th percentiles. Outlier trimming generally increases both the magnitude and significance of the results. Given gaps in the Cbonds data and varying reporting frequencies, our sample composition may change over time within any given event. In Table A31 and Figure A15, we replicate the main results using the subsample of bonds for which a fully balanced panel is available within a given estimation window. The results are broadly similar.

Lastly, we consider the effect of state-involved conflict on bond yields in Table A32. Panel A uses yields (bp) as the outcome, while Panel B uses log yields to express the effects as percent change in yield. Columns (1)-(5) split by fatality quintiles, column (6) interacts with log distance to the capital, and columns (7)-(10) interacts with the 5-year conflict prior. Conflict

increases bond spreads by an average of 5.3 basis points, or 0.7%, rising to 18.2 for the most violent conflicts, or 2.5%. This effect rises to 20.8 near the capital, and 6.4 for first events, and between 7.6-12.4 for countries with no recent conflict history. All interaction terms are of similar sign and significance as the main results using prices.

7 Conclusion

As recent wars in Ukraine, Israel/Palestine, Syria, Ethiopia, and elsewhere vividly demonstrate, violent conflict can cast a pall over global trade and finance. Globally, many other conflicts receive considerably less media coverage, though their local economic impacts may be quite severe. We use daily bond price data to test responses of sovereign bond markets to political violence across several hundred recent incidents of armed conflict. Event study regressions show that bond markets react swiftly and negatively to conflicts in which the state battles an organized rebel or terrorist group, but not to violence between non-state actors or against civilians. State-involved conflicts produce large increases in default risk, while other outbreaks of violence do not. Price responses are largest in risky, short-maturity bonds that are highly exposed to heightened default risk. These results suggest that investors react to relevant information about state solvency, rather than naively to bad news.

To benchmark effect sizes, we build a simple asset pricing model where market responses to conflict depend on beliefs about the likelihood and the cost of conflict. Combining the model with reduced form estimates of the dynamic treatment effects, we find that while investors initially underreact, they price in up to 75% of the shock by day 15, relative to a full-information context with empirically correct beliefs.

We then investigate the role of market beliefs in more detail. Consistent with model predictions for rational investors, we show that markets respond more to surprising events where priors are optimistic, and to severe conflicts where expected costs are high and rebels explicitly threaten the state. Markets are able to predict the duration of conflict ex-ante, and learn about conflict from international news media. However, this learning occurs *only* when the overall information environment is sufficiently sparse that conflict-related news is not crowded out. Despite a market replete with information asymmetries and plausible behavioral biases, investors form data-driven, conflict-specific priors and cost expectations that in-

corporate both historical knowledge and initial signals on the spatial and political characteristics of war. However, evidence of limited attention and news crowd out may in part explain why conflict appears to be slightly underpriced in equilibrium.

The findings provide the most externally valid, well-identified estimate of the bond market effects of conflict, and provide the first systematic tests of what drives investors' responses to violence. Still, several questions remain unanswered. This study focuses exclusively on short-run price behavior – how much do investors really understand about long-run conflict dynamics, especially as many civil wars can drag on for many years? Do markets respond more to conflicts that affect major government revenue sources, like productive regions or natural resources? How are cross-border spillovers priced? Do local bond markets, often dominated by insiders, respond differently than investors in international bond issues? How does the local corporate and banking sector react to conflict in terms of investment decisions and the deployment of capital and personnel? We view these as important avenues for future research.

References

Abadie, A., Athey, S., Imbens, G. W., and Wooldridge, J. M. (2022). When Should You Adjust Standard Errors for Clustering? *The Quarterly Journal of Economics*.

Abadie, A. and Gardeazabal, J. (2003). The Economic Costs of Conflict: A Case Study of the Basque Country. *American Economic Review*, 93(1):113–132.

Aguiar, M. and Amador, M. (2014). Chapter 11 - sovereign debt. In Gopinath, G., Helpman, E., and Rogoff, K., editors, *Handbook of International Economics*, volume 4 of *Handbook of International Economics*, pages 647–687. Elsevier.

Aguiar, M. and Amador, M. (2021). *The Economics of Sovereign Debt and Default*. CREI Lectures in Macroeconomics. Princeton University Press.

Ahmed, S., Coulibaly, B., and Zlate, A. (2017). International financial spillovers to emerging market economies: How important are economic fundamentals? *Journal of International Money and Finance*, 76:133–152.

Akresh, R., Bhalotra, S., Leone, M., and Osili, U. O. (2012). War and Stature: Growing Up during the Nigerian Civil War. *American Economic Review*, 102(3):273–277.

Arin, K. P., Ciferri, D., and Spagnolo, N. (2008). The price of terror: The effects of terrorism on stock market returns and volatility. *Economics Letters*, 101(3):164–167.

Arrow, K. J., Forsythe, R., Gorham, M., Hahn, R., Hanson, R., Ledyard, J. O., Levmore, S., Litan, R., Milgrom, P., Nelson, F. D., Neumann, G. R., Ottaviani, M., Schelling, T. C., Shiller, R. J., Smith, V. L., Snowberg, E., Sunstein, C. R., Tetlock, P. C., Tetlock, P. E., Varian, H. R., Wolfers, J., and Zitzewitz, E. (2008). The promise of prediction markets. *Science*, 320(5878):877–878.

Asonuma, T., Niepelt, D., and Rancière, R. (2017). Sovereign Bond Prices, Haircuts and Maturity. *NBER Working Paper 23864*, page 43.

Baicker, K. and Svoronos, T. (2019). Testing the validity of the single interrupted time series design. Working Paper 26080, National Bureau of Economic Research.

Baker, A. C., Larcker, D. F., and Wang, C. C. (2021). How much should we trust staggered difference-in-differences estimates? *Harvard Business School Working Paper 21-112*.

Balima, H. W. (2020). Coups d'état and the cost of debt. *Journal of Comparative Economics*, 48(3):509–528.

Barberis, N., Shleifer, A., and Vishny, R. (1998). A model of investor sentiment. *Journal of Financial Economics*, page 37.

Barberis, N. and Thaler, R. (2003). A Survey of Behavioral Finance. *Handbook of the Economics of Finance*, pages 1052–1121.

Barro, R. J. (2006). Rare disasters and asset markets in the twentieth century*. *The Quarterly Journal of Economics*, 121(3):823–866.

Bazzi, S., Blair, R. A., Blattman, C., Dube, O., Gudgeon, M., and Peck, R. (2022). The promise and pitfalls of conflict prediction: Evidence from colombia and indonesia. *The Review of Economics and Statistics*, 104(4):764–779.

Blair, G., Christensen, D., and Wirtschafter, V. (2022). How does armed conflict shape investment? Evidence from the mining sector. *Journal of Politics*, 84(1):117–133.

Bordalo, P., Gennaioli, N., and Shleifer, A. (2018). Diagnostic expectations and credit cycles. *The Journal of Finance*, 73(1):199–227.

Brown, W. O. and Burdekin, R. C. K. (2000). Turning points in the u.s. civil war: A british perspective. *The Journal of Economic History*, 60(1):216–231.

Caballero, R. J. and Krishnamurthy, A. (2008). Collective risk management in a flight to quality episode. *The Journal of Finance*, 63(5):2195–2230.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230. Themed Issue: Treatment Effect 1.

Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2008). Bootstrap-Based Improvements for Inference with Clustered Errors. *The Review of Economics and Statistics*, 90(3):414–427.

Castañeda, A. and Vargas, J. F. (2012). Sovereign risk and armed conflict: an event-study for colombia. *Defence and Peace Economics*, 23(2):185–201.

Cerra, V. and Saxena, S. C. (2008). Growth Dynamics: The Myth of Economic Recovery. *American Economic Review*, 98(1):439–457.

Chadefaux, T. (2017). Market anticipations of conflict onsets. *Journal of Peace Research*, 54(2):313–327.

Chaney, E. (2008). Assessing pacification policy in Iraq: Evidence from Iraqi financial markets. *Journal of Comparative Economics*, 36(1):1–16.

Chesney, M., Reshetar, G., and Karaman, M. (2011). The impact of terrorism on financial markets: An empirical study. *Journal of Banking & Finance*, 35(2):253–267.

Collier, P. (1999). On the economic consequences of civil war. *Oxford Economic Papers*, 51(1):168–183.

Dasgupta, A. and Ziblatt, D. (2022). Capital Meets Democracy: The Impact of Franchise Extension on Sovereign Bond Markets. *American Journal of Political Science*, 66(3):630–647.

Davenport, C., Mokleiv Nygård, H., Fjelde, H., and Armstrong, D. (2019). The Consequences of Contention: Understanding the Aftereffects of Political Conflict and Violence. *Annual Review of Political Science*, 22(1):361–377.

Dette, H. and Schumann, M. (2020). Difference-in-differences estimation under non-parallel trends. *Working Paper*.

Dube, A., Girardi, D., and Taylor, A. M. (2022). A Local Projections Approach to Difference-in-Differences Event Studies. *Working Paper*, page 46.

Durante, R. and Zhuravskaya, E. (2018). Attack when the world is not watching? us news and the israeli-palestinian conflict. *Journal of Political Economy*, 126(3):1085–1133.

Eisensee, T. and Strömberg, D. (2007). News droughts, news floods, and u. s. disaster relief. *The Quarterly Journal of Economics*, 122(2):693–728.

Enke, B. and Zimmermann, F. (2017). Correlation Neglect in Belief Formation. *The Review of Economic Studies*, 86(1):313–332.

Frey, B. and Kucher, M. (2001). Wars and markets: How bond values reflect the second world war. *Economica*, 68(271):317–333.

Gatev, E. and Strahan, P. E. (2006). Banks' advantage in hedging liquidity risk: Theory and evidence from the commercial paper market. *The Journal of Finance*, 61(2):867–892.

Gennaioli, N. and Shleifer, A. (2018). *A Crisis of Beliefs: Investor Psychology and Financial Fragility*. Princeton University Press.

Gennaioli, N., Shleifer, A., and Vishny, R. (2015). Neglected risks: The psychology of financial crises. *American Economic Review*, 105(5):310–14.

Girardi, D. (2020). Partisan Shocks and Financial Markets: Evidence from Close National Elections. *American Economic Journal: Applied Economics*, 12(4):224–252.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Greenstone, M. (2007). Is the "surge" working? Some new facts. *NBER Working Paper 13458*.

Guidolin, M. and La Ferrara, E. (2007). Diamonds are forever, wars are not: Is conflict bad for private firms? *American Economic Review*, 97(5):1978–1993.

Guidolin, M. and La Ferrara, E. (2010). The economic effects of violent conflict: Evidence from asset market reactions. *Journal of Peace Research*, 47(6):671–684.

Haruvy, E., Lahav, Y., and Noussair, C. N. (2007). Traders' expectations in asset markets: Experimental evidence. *American Economic Review*, 97(5):1901–1920.

Hausman, C. and Rapson, D. S. (2018). Regression discontinuity in time: Considerations for empirical applications. *Annual Review of Resource Economics*, 10:533–552.

Hirano, K., Imbens, G. W., and Ridder, G. (2003). Efficient estimation of average treatment effects using the estimated propensity score. *Econometrica*, 71(4):1161–1189.

Israeli, D., Kasznik, R., and Sridharan, S. A. (2022). Unexpected distractions and investor attention to corporate announcements. *Review of Accounting Studies*, 27(2):477–518.

Jha, S., Koudijs, P., and Salgado, M. (2022). Markets under Siege: How Differences in Political Beliefs can Move Financial Markets. *SSRN Electronic Journal*.

Kapstein, E., Rexer, J., Tantravahi, A., and Yi, F. (2024). Penalties and premiums in sovereign credit ratings. *Working Paper*.

Kapstein, E. B. and Tantravahi, A. (2021). The price of violence: Interest rates and homicides in mexico. *ESOC Working Paper 26*.

Ksoll, C., Macchiavello, R., and Morjaria, A. (2022). Electoral Violence and Supply Chain Disruptions in Kenya's Floriculture Industry. *The Review of Economics and Statistics*, (Forthcoming):1–45.

Leon, G. (2012). Civil Conflict and Human Capital Accumulation: The long-term effects of political violence in Perú. *Journal of Human Resources*, 47(4):991–1022.

Mackinlay, A. C. (1997). Event Studies in Economics and Finance. *Journal of Economic Perspectives*, 35:13–39.

Mauro, P., Sussman, N., and Yafeh, Y. (2002). Emerging market spreads: Then versus now. *The Quarterly Journal of Economics*, 117(2):695–733.

Meyer, J., Reinhart, C. M., and Trebesch, C. (2022). Sovereign Bonds Since Waterloo. *The Quarterly Journal of Economics*, 137(3):1615–1680.

Mitchener, K. J., Oosterlinck, K., Weidenmier, M. D., and Haber, S. (2015). Victory or repudiation? predicting winners in civil wars using international financial markets. *Journal of Banking Finance*, 60:310–319.

Novta, N. and Pugacheva, E. (2020). The Macroeconomic Costs of Conflict. *IMF Working Paper*.

Ouedraogo, R., Mlachila, M., Sourouema, W. S., and Compaoré, A. (2022). The impact of conflict and political instability on banking crises in developing countries. *The World Economy*, 45(6):1937–1977.

Pan, W.-F., Wang, X., Xiao, Y., Xu, W., and Zhang, J. (2024). The effect of economic and political uncertainty on sovereign cds spreads. *International Review of Economics Finance*, 89:143–155.

Pastor, L. and Veronesi, P. (2009). Learning in Financial Markets. *Annual Review of Financial Economics*, 1(1):361–381.

Peress, J. and Schmidt, D. (2020). Glued to the tv: Distracted noise traders and stock market liquidity. *The Journal of Finance*, 75(2):1083–1133.

Qureshi, M. S. (2013). Trade and thy neighbor's war. *Journal of Development Economics*, 105:178–195.

Reinhart, C. and Rogoff, K. (2009). *This Time Is Different: Eight Centuries of Financial Folly*. Princeton University Press.

Rexer, J. M. (2022). The Brides of Boko Haram: Economic Shocks, Marriage Practices, and Insurgency in Nigeria. *The Economic Journal*, 132(645):1927–1977.

Rohner, D. (2018). The economics of conflict: A literature review and practitioner's guide for the foreign aid community. 26(4):21.

Roth, J. (2022). Pretest with caution: Event-study estimates after testing for parallel trends. *American Economic Review: Insights*, 4(3):305–22.

Schraeder, S. (2016). Information Processing and Non-Bayesian Learning in Financial Markets. *Review of Finance*, 20(2):823–853.

Shen, Y., Feng, Q., and Sun, X. (2024). Stability and risk contagion in the global sovereign cds market under russia-ukraine conflict. *The North American Journal of Economics and Finance*, 74:102204.

Tantravahi, A. (2022). rln (version 1).

Utar, H. (2020). Firms and labor in times of violence: Evidence from the mexican drug war. *CESifo Working Papers*.

Waldenström, D. and Frey, B. S. (2008). Did nordic countries recognize the gathering storm of world war ii? evidence from the bond markets. *Explorations in Economic History*, 45(2):107–126.

ONLINE APPENDIX

A Benchmarking effect sizes: quantitative framework

In this section, we build a quantitative framework to assess whether investors accurately price conflict events. We then estimate the parameters of the model using reduced-form regressions and simulate counterfactual price responses. We first compare the observed price effect over time with that predicted by empirically correct priors to quantify the extent of market mispricing. Next, we use the model to identify prior beliefs on the probability of conflict and/or its costliness that are consistent with the observed price effect.

A.1 Model

We consider a risk-neutral investor holding a representative bond with the following discounted expected present value as of $t = 0$

$$EV_0 = \sum_{t=0}^{T} \frac{C_t}{(1+r)^t}(1 - \zeta_t\gamma) + \frac{F}{(1+r)^T}(1 - \zeta_T\gamma) \tag{4}$$

Where r is the return on a risk-free asset, F is the face value of the bond, $C_t = C = i \times F$ is the constant coupon payment at rate i, and T is the terminal period in which the face value is returned. In each period, the investor faces a potential negative cash flow shock due to conflict. Conflict occurs with probability $\zeta_t = pr(z_t = 1)$, where z_t is the state variable indicating active conflict in a given period. In the conflict state, the bond experiences a time-constant haircut of $1 - \gamma$.[27,28] The no-arbitrage condition implies that $EV_t = p_t$ for all t.

As in Barberis et al. (1998) and Pastor and Veronesi (2009), investors learn about cash flows. In our case, they learn about ζ_t based on realizations of conflict. Investors know that

[27] We assume that conflict causes a share of the payment to be lost. This could be interpreted as a change in the default probability, a haircut, or late/missed payments. The set up is similar to the analysis of haircuts in Asonuma et al. (2017). Given risk neutrality, we need specify only the expectation rather than the entire distribution of losses.

[28] Note that other types of non-conflict default risk, e.g., macroeconomic risk, are not modelled here. This is unproblematic assuming additive separability from the conflict shock, since our treatment effect is differenced, and therefore does not depend on price levels.

conflict will follow an AR(1) process going forward

$$z_t = \alpha + \rho z_{t-1} + \epsilon_t \tag{5}$$

However, investors do not know what state of the world they are currently in. The expected payoffs after observing each state of the world as of $t = 0$ can be written recursively as a function of the realization of z_0.

$$EV_0(1) = (1-\gamma)C_0 + EV_1(1) \qquad EV_0(0) = C_0 + EV_1(0)$$

Since the AR(1) process is Markov, the continuation values are updated using the probabilities of the implied transition matrix of equation (5) beginning at z_0.[29] The ex-ante expected value can be written as an explicit function of the $t = 0$ prior belief on conflict probability ζ_0

$$EV_0 = \zeta_0 EV_0(1) + (1-\zeta_0)EV_0(0) \tag{6}$$

After observing the realization $z_0 = 1$ but before subsequent periods, market participants may update their priors from ζ_0 to $\tilde{\zeta}_0$.[30] The ex-post expected value $\widetilde{EV}_0$ will be as in (6), except with the updated probabilities. As such, the treatment effect τ will be the difference between the ex-ante and ex-post expected values after a conflict signal:

$$\tau = \widetilde{EV}_0 - EV_0 = (\tilde{\zeta}_0 - \zeta_0)[EV_0(1) - EV_0(0)] \tag{7}$$

In the full-information case, the investor knows that the signal $z_0 = 1$ has been observed with certainty, and a conflict episode has begun. In this case, there is full updating and $\tilde{\zeta}_0 = z_0 = 1$. This is the base case of our model, which we use to benchmark the price response.

A few things are immediately obvious from equation (7). First, since $\tilde{\zeta}_0 \geq \zeta_0$ in any reasonable learning process, τ is negative as long as conflict is costly and autocorrelated, or $\gamma > 0$ and $\rho \geq 0$. Second, the magnitude of this effect is increasing in γ, the cost of conflict, and falling in the prior ζ_0. This coheres to the classic intuition from asset pricing learning

[29] The implied transition matrix is given by the conditional probabilities: $pr(z_t = 1|z_{t-1} = 0) = \alpha$, $pr(z_t = 0|z_{t-1} = 0) = 1-\alpha$, $pr(z_t = 1|z_{t-1} = 1) = \alpha + \rho$, $pr(z_t = 0|z_{t-1} = 1) = 1 - (\alpha + \rho)$

[30] This within-period updating makes sense when we consider that bonds are paid out annually, while the price response occurs within days.

models that events must be surprising and payoff-relevant to move markets. Finally, we observe r, i, T in the bond-level data, and we can further estimate γ, α, ρ, and ζ_0 from the conflict data. We can therefore simulate the full-information, unbiased treatment effect τ.

A.2 Estimation and simulation

To simulate the predicted market response τ, we need estimates of γ, the average cost of conflict, and α and ρ, the parameters of the AR(1) process. To estimate γ, we add another equation to the model. We assume income follows a log-linear two-way fixed-effects process, varying around its long-run level in response to conflict shocks. For country i in year t, we have:

$$\log(y_{it}) = \alpha_0 + \gamma z_{it} + \delta_i + \delta_t + u_{it} \tag{8}$$

The results of the estimation of this equation are in Table A10, columns (1)-(4). After conditioning on aggregate shocks δ_t and location-specific characteristics δ_i in column (3), γ represents the annual effect of conflict on aggregate output. The results indicate a $\hat{\gamma} = 0.058$, or a 5.8% average annual loss in output for each year that a conflict is active.[31] Our estimates are similar in magnitude to those in Novta and Pugacheva (2020). Next, we estimate the AR(1) parameters from equation (5) using OLS on the same country-year panel.[32] The results are in Table A10, column (5). We estimate $\hat{\rho} = 0.8$ and $\hat{\alpha} = 0.028$. We take the remaining parameter values from the data. We focus on ten-year bonds, $T = 10$, the median maturity in our sample. We set $r = 0.029$, the average 10-year T-bill rate over the sample period, and $i = 0.055$, the average bond-level coupon rate. $\zeta_0 = 0.140$, the average country-year prevalence of conflict from 1990-2020. A summary of parameter values for the simulation is in Table A11.

The simulation proceeds as follows: using the AR(1) transition matrix, we simulate forward the Markov process for z_t from $t = 0$ to T, giving us a path of beliefs on ζ_t for initial conditions $z_0 = 0$ and $z_0 = 1$. Here, we maintain the assumption of full updating, so that $\zeta_0 = z_0$ after z_0 is revealed. Figure A5 shows the path of beliefs on conflict probability after

[31] In column (4), we include conflict variables z^H and z^L for major and minor conflicts, respectively. The output effect for major conflicts is 15.7%, while for minor it is only 4.8%. We use the composite effect for our simulations.

[32] We combine data from the World Bank WDI with UCDP-GED for 1990-2022. We use this time span rather than the sample period alone (2004-2020) to capture earlier conflict events that might affect investors' expectations.

the state is revealed at $t = 0$. If $z_0 = 1$, the probability of conflict is immediately updated to unity. It then falls thereafter, reflecting the persistence of conflict. Since the AR(1) process is stationary, beliefs converge to the accurate prior in the long-run. The AR(1) process produces an intuitive path for beliefs: conflict today should predict a high likelihood of conflict tomorrow, but does not reveal much information about the risk conflict 20 years hence. Using the sequence of ζ_t in Figure A5, we calculate the net present value of payments under each state, $EV_0(1)$ and $EV_0(0)$. Then, we take the ex-ante expectation EV_0 over a grid of ζ_0 ranging from 0 to 1. Finally, we take the ex-ante and ex-post differences to obtain a linear function $\tau(\zeta_0)$.

A.3 Results

A.3.1 Full-information benchmark

We begin by benchmarking the observed effect relative to the full-information, perfectly unbiased case in which $\zeta_0 = \hat{\zeta}_0$, $\gamma = \hat{\gamma}$ and $\tilde{\zeta}_0 = 1$, comparing the simulated τ under these true parameter values with our time-varying post-event coefficients $\hat{\tau}_k$.

The results are in Figure A6, top-left panel. We plot each $\frac{\hat{\tau}_k}{\tau}$ as well as overlay a local polynomial fit to smooth day-to-day noise in the coefficients. We estimate that in the perfect information case with accurate beliefs, investors' long-run downward revision on the value of the average bond should be 1.55 points. The results show that initially, there is limited market response; markets price in only 14% of this benchmark effect on the first day. However, there is evidence of rapid market correction over time. The share of the shock priced in rises to 54% on day 5, and 76% by day 15, its maximal value.[33] The evidence suggests that while investors learn rapidly about conflict probability, they also underreact even at the largest post-event effect. Nonetheless, the confidence intervals in Figure 2 make it clear that we cannot rule out that the market fully internalizes the conflict shock for many of the estimated τ_k.

A.3.2 Implied biases

In our simple framework, there are three reasons that investors might underreact to news of a new conflict: *i*) biases about ζ_0, *ii*) biases about γ, and *iii*) information asymmetries leading to incomplete updating of $\tilde{\zeta}_0$

[33] The local polynomial suggests convergence to a steady state in which around 60% of the shock priced in.

First, investors may be overly pessimistic about the probability of conflict, and have a higher belief ζ_0 than is warranted by the data, leading to too-little surprise after a shock. The top-right panel of Figure A6 plots $\tau(\zeta_0)$ in the full information case holding γ fixed. Dashed lines indicate the location of the empirically estimated maximum treatment effect $\hat{\tau}_{15}$, along the vertical axis, and the correct prior probability of conflict (the empirical average), along the horizontal.[34]

The solid line plots the full-information $\tau(\zeta_0)$ schedule for $\gamma = \hat{\gamma}$, the empirical cost estimate. The point at which this line intersects $\hat{\tau}_{15}$ gives the implied prior ζ_0 that rationalizes the estimated treatment effect under an accurate expectation of γ. Given full information and accurate cost beliefs, the observed treatment effect implies a prior belief on the probability of conflict of 34.6%, much larger than the empirical likelihood of 14%.

Second, investors may be wrong about the expected costs of conflict, underestimating γ. If investors believe conflict is not costly, they may not respond sufficiently to the shock. In the same figure, we plot the full-information $\tau(\zeta_0)$ functions for different values of γ. It is clear from equation (7) that this rotates the predicted treatment effect line around $(1, 0)$. As γ falls, the line rotates upward so that a smaller treatment effect is predicted for each level of ζ_0.

Assuming that $\zeta_0 = \hat{\zeta}_0$, we can invert τ to identify the γ such that $\hat{\tau}_{15} = \tau(\hat{\zeta}_0)$. We estimate this at approximately 4.38%, plotted in Figure A6, top-right. Assuming full information and accurate priors about conflict probability, the observed treatment effect implies a 1.38 p.p. under-estimation of the cost of conflict. In the bottom-left panel, we trace out the points (γ, ζ_0) at which $\tau(\zeta_0, \gamma) = \hat{\tau}_{15}$. This belief frontier gives all combinations of the two parameters that are consistent with the observed treatment effect under full information. The hyperbolic shape of the function implies that for most reasonable values of ζ_0, the implied γ is relatively close to its empirical value (horizontal line). While we cannot separately identify these two biases with only one treatment effect, market under-reaction is more likely to be driven by small mis-estimations of conflict cost than large biases in conflict probability.

Finally, we have maintained thus far the full-information assumption that $\tilde{\zeta}_0 = 1$ by $k = 15$. The dynamic path of coefficients in Figure 2 suggest substantial updating up to day 15, and leveling off thereafter. However, investor posteriors may not actually converge to 1 if insufficient information is revealed by $k = 15$. Allowing for incomplete information shifts

[34] We fix τ at $\hat{\tau}_{15}$, the maximal treatment effect, in order to obtain conservative bounds on the implied biases.

$\tau(\zeta_0)$ upward, affecting the intercept without changing the slope, as in Figure A6, bottom-right panel. Assuming unbiased priors, the posterior belief that rationalizes the treatment effect on day 15 is roughly 0.8.

A.4 Limitations

The proposed framework makes several simplifying assumptions that impose important limitations on the analysis. First, we assume linearity in the effect of conflict on the cost to the bondholder – a constant haircut in all conflict periods. In reality, default probabilities might be nonlinear in conflict severity, with default occurring only after some threshold is reached. This might well produce different – and potentially more volatile – price dynamics than the simple model. Second, risk neutrality is a strong assumption. Of course, risk averse agents would demand premia for holding assets with default risk. If conflict onset increased uncertainty, then option value might actually prevent sell-offs. On the other hand, if conflict represents the resolution of uncertainty, it might even be conceivable that prices to rise in response to onset, as the risk premium in yield spreads falls. Third, our haircut cost is time-constant, but in reality might vary dynamically in important ways that interact with the term structure of debt. Finally, conflict might follow a non-Markovian processes that complicates the simple recursive representation presented here. These – and others – are all valid points.

Nevertheless, this stylized model is useful for two reasons: i) it provides the simplest possible quantification of the effects of conflict onset against which to benchmark our empirical estimate and ii) it provides a parsimonious set of predictions to test the direction of investor beliefs. Ultimately, the results in Section 5 reveal that markets appear to follow the logic of the model on average, suggesting that the simple model performs well as a first approximation.

B Conflict events included in the sample

Table A1: List of conflicts

Conflict name	Onset	State	Non-state	One-sided
Angola: Government of Angola - FLEC-FAC-TN	2016-01-01	✓		
Armenia: Government of Russia (Soviet Union) - Civilians	2015-01-12			✓
Austria: IS - Civilians	2020-11-02			✓
Bahrain: Government of Bahrain - Civilians	2011-02-14			✓
Belgium: Government of Iraq - IS	2016-03-15	✓		
Belgium: IS - Civilians	2016-03-22			✓
Benin: IS - Civilians	2019-05-03			✓
Brazil: Comando Classe A - Comando Vermelho	2019-07-29		✓	
Brazil: Comando Vermelho - FDN	2020-01-01		✓	
Brazil: Comando Vermelho - GDE	2017-01-01		✓	
Brazil: Comando Vermelho - Terceiro Comando Puro	2004-07-14		✓	
Brazil: FDN - PCC	2016-10-16		✓	
Brazil: PCC - Sindicato RN	2015-06-10		✓	
Cameroon: Ambazonia insurgents - Civilians	2018-02-12			✓
Cameroon: Comités locaux de vigilance - Jama'atu Ahlis Sunna Lidda'awati wal-Jihad	2016-12-22		✓	
Cameroon: Government of Cameroon - Ambazonia insurgents	2017-09-16	✓		

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Cameroon: Jama'atu Ahlis Sunna Lidda'awati wal-Jihad - Civilians	2020-08-01			✓
China: Government of Myanmar (Burma) - MNDAA	2009-08-27	✓		
China: Han Chinese - Uighur	2009-06-26		✓	
Colombia: AUC - Civilians	2005-12-10			✓
Colombia: FARC dissidents - Civilians	2018-01-21			✓
Colombia: Government of Colombia - ELN	2010-08-18	✓		
Colombia: Government of Colombia - FARC dissidents	2017-04-08	✓		
Ecuador: Government of Colombia - FARC dissidents	2019-01-16	✓		
Egypt: Bani Hilal - Nubian Dabudiya	2014-04-03		✓	
Egypt: Government of Egypt - Ansar Bayt al-Maqdis	2014-03-11	✓		
Egypt: Government of Egypt - Harakit Sawa'id Misr	2016-10-08	✓		
Egypt: Government of Egypt - IS	2014-11-16	✓		
Egypt: Government of Egypt - Jama'at Ansar al-Islam	2017-10-20	✓		
Egypt: Government of Israel - Hamas	2010-01-06	✓		
Egypt: IS - Civilians	2015-10-31			✓
Egypt: Opponents of Morsi - Supporters of Morsi	2012-11-25		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Egypt: Supporters of al-Ahly football team - Supporters of al-Masry football team	2012-02-01		✓	
Egypt: Tawhid wal Jihad - Civilians	2004-10-07			✓
Ethiopia: Afar - Oromo	2020-03-12		✓	
Ethiopia: Afar - Somali	2019-10-11		✓	
Ethiopia: Agaw, Amhara - Gumuz	2020-09-25		✓	
Ethiopia: Ale - Konso	2020-07-24		✓	
Ethiopia: Amhara - Gumuz	2019-04-27		✓	
Ethiopia: Amhara - Qimant	2018-10-01		✓	
Ethiopia: Amhara, Guraghe - Oromo	2020-06-29		✓	
Ethiopia: Amhara, Oromo, Shinasha - Gumuz	2020-12-23		✓	
Ethiopia: Government of Eritrea, Government of Ethiopia - Civilians	2020-11-25			✓
Ethiopia: Government of Ethiopia - Military faction (forces of Civilians)	2015-11-01			✓
Ethiopia: Government of Ethiopia - Military faction (forces of Asaminew Tsige)	2019-06-22	✓		
Ethiopia: Government of Ethiopia - OLA	2019-05-27	✓		
Ethiopia: Government of Ethiopia - TPLF	2020-11-01	✓		
Ethiopia: Murle - Nuer	2016-04-15		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Ethiopia: OLA - Civilians	2020-04-09			✓
Ethiopia: Oromo - Dorze, Gamo, Guraghe, Silt'e, Wolayta	2018-09-14		✓	
Ethiopia: Oromo - Gamo, Guraghe	2018-09-15		✓	
Guatemala: Gulf Cartel - Sinaloa Cartel	2008-03-25		✓	
Guatemala: Los Zetas - Civilians	2011-01-01			✓
Indonesia: Government of Indonesia - OPM	2013-02-21	✓		
Iraq: Government of Iran - Government of Israel	2020-01-03	✓		
Iraq: Government of Turkey - IS	2016-04-19	✓		
Iraq: IS - JRTN	2014-06-02		✓	
Iraq: Jaysh al-Mukhtar - MEK	2015-10-29		✓	
Israel: Government of Israel - PIJ	2017-10-30	✓		
Israel: Government of Syria - Syrian insurgents	2014-06-22	✓		
Israel: Hamas - Jund Ansar Allah	2009-08-14		✓	
Ivory Coast: AQIM - Civilians	2016-03-13			✓
Ivory Coast: FRGO - Civilians	2010-12-14			✓
Ivory Coast: Fulani - Koulango, Lobi, Malinké	2016-03-24		✓	
Ivory Coast: Government of Ivory Coast - FDSI-CI	2011-03-01	✓		
Ivory Coast: Guéré - Malinké	2010-11-28		✓	
Jordan: Government of Jordan - IS	2016-03-01	✓		

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Jordan: Government of Syria - Syrian insurgents	2015-06-25	✓		
Jordan: IS - Southern Front	2016-10-16		✓	
Kenya: Chiebiliew Yoal (Lou Nuer) - Cie Bol	2018-12-10		✓	
Kenya: Government of Kenya - Al-Shabaab	2015-03-13	✓		
Lebanon: ADP - Civilians	2013-08-23			✓
Lebanon: Alawite (Lebanon) - Sunni (Lebanon)	2012-02-10		✓	
Lebanon: Brigades of Aisha - Hezbollah	2013-08-15		✓	
Lebanon: Government of Lebanon - IS	2014-06-25	✓		
Lebanon: Government of Syria - Civilians	2011-10-06			✓
Lebanon: Government of Syria - Syrian insurgents	2013-02-24	✓		
Lebanon: Hezbollah - HTS	2017-07-21		✓	
Lebanon: Hezbollah - IS	2014-01-02		✓	
Lebanon: Hezbollah - Jabhat Fateh al-Sham	2013-06-01		✓	
Lebanon: Hezbollah - PSP	2008-05-09		✓	
Lebanon: IS - Civilians	2016-06-27			✓
Lebanon: IS - HTS, Saraya al-Sham	2017-05-27		✓	
Lebanon: IS - Jabhat Fateh al-Sham	2016-01-27		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Malaysia: Government of Malaysia - Sultanate of Sulu	2013-03-01	✓		
Malaysia: Government of Philippines - ASG	2017-12-04	✓		
Mexico: Autodefensas Unidas de Michoacán - Los Caballeros Templarios	2013-04-28		✓	
Mexico: Beltrán Leyva Cartel - Valdez Villareal faction	2010-01-01		✓	
Mexico: Beltrán Leyva Cartel - Sinaloa Cartel	2008-01-01		✓	
Mexico: CRAC-PF - Los Ardillos	2019-01-01		✓	
Mexico: Caborca Cartel, Sinaloa Cartel - Los Mayos faction - Sinaloa Cartel - Chapitos faction	2020-01-01		✓	
Mexico: Cartel Independiente de Acapulco - La Barredora	2011-01-07		✓	
Mexico: Cartel Independiente de Acapulco - Los Virus	2019-01-03		✓	
Mexico: Fuerza Anti Unión - La Union de Tepito	2018-01-25		✓	
Mexico: Guerreros Unidos - Los Rojos	2012-09-08		✓	
Mexico: Guerreros Unidos - Los Tlacos	2018-07-31		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Mexico: Gulf Cartel - Dragones de Sheyla faction - Gulf Cartel - Los Fresitas faction	2014-01-01		✓	
Mexico: Gulf Cartel - Los Zetas	2010-01-01		✓	
Mexico: Gulf Cartel - faction - Ramirez Treviño Gulf Cartel - Villareal faction	2013-03-10		✓	
Mexico: Gulf Cartel - Sinaloa Cartel	2005-02-16		✓	
Mexico: Jalisco Cartel New Generation - La Familia	2018-01-01		✓	
Mexico: Jalisco Cartel New Generation - La Nueva Familia	2016-12-25		✓	
Mexico: Jalisco Cartel New Generation - La Resistencia	2011-02-12		✓	
Mexico: Jalisco Cartel New Generation - Los Caballeros Templarios	2012-02-12		✓	
Mexico: Jalisco Cartel New Generation - Los Rojos	2019-01-11		✓	
Mexico: Jalisco Cartel New Generation - Los Zetas	2011-09-20		✓	
Mexico: Jalisco Cartel New Generation - Nueva Plaza Cartel	2018-01-01		✓	
Mexico: Jalisco Cartel New Generation - Santa Rosa de Lima Cartel	2018-01-01		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Mexico: Jalisco Cartel New Generation - Sinaloa Cartel	2015-01-01		✓	
Mexico: Juarez Cartel - Sinaloa Cartel	2012-01-01		✓	
Mexico: La Familia - Los Caballeros Templarios	2011-01-16		✓	
Mexico: La Mochomera - Sinaloa Cartel	2013-04-19		✓	
Mexico: Los Ardillos - Los Rojos	2014-07-08		✓	
Mexico: Los Aztecas - La Linea	2018-01-01		✓	
Mexico: Los Zetas - Civilians	2010-02-18			✓
Mexico: Los Zetas - Cártel del Noreste faction - Los Zetas - Old School Zetas faction	2016-02-22		✓	
Mexico: Los Zetas - Jorge Ivan Hernandez Cantu faction - Los Zetas - Juan Pedro Salvador Saldivar Farias faction	2016-02-10		✓	
Mexico: Los Zetas - Los Zetas - Velazquez Caballero faction	2012-06-11		✓	
Mexico: Los Zetas - Sinaloa Cartel	2010-03-28		✓	
Mexico: Mara 18 (Guatemala) - Salvatrucha Mara (Guatemala)	2004-09-23		✓	
Mexico: Santiago Xochiltepec - Santo Domingo Teojomulco	2002-05-30		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Mexico: Sinaloa Cartel - Chapitos faction - Sinaloa Cartel - Los Mayos faction	2019-12-12		✓	
Mexico: Sinaloa Cartel - El Pepillo faction - Sinaloa Cartel - Los 28 faction	2015-01-01		✓	
Mexico: Sinaloa Cartel - Forces of Damaso	2014-10-31		✓	
Mexico: Sinaloa Cartel - Los Chavez Matamoros faction - Sinaloa Cartel - Los Salgueiro faction	2014-02-05		✓	
Mexico: Sinaloa Cartel - Los Memos faction - Sinaloa Cartel - Los Salazar faction	2015-03-01		✓	
Mexico: Sinaloa Cartel - Tijuana Cartel	2006-01-01		✓	
Mexico: Tijuana Cartel - Tijuana Cartel - El Teo faction	2008-02-01		✓	
Mozambique: Ansar al-Sunnah - Civilians	2017-11-27			✓
Mozambique: Government of Mozambique - Ansar al-Sunnah	2018-01-13	✓		
Mozambique: Government of Mozambique - IS	2019-06-02	✓		
Mozambique: IS - Civilians	2019-07-23			✓
Netherlands: Government of Iran - MEK	2015-12-15	✓		

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Nigeria: Abugbe (Agatu) - Agbaduma (Agatu)	2019-06-30		✓	
Nigeria: Agatu - Fulani	2013-04-20		✓	
Nigeria: Agatu - Tiv	2013-06-04		✓	
Nigeria: Atakar - Fulani	2013-03-19		✓	
Nigeria: Atyap - Fulani	2020-01-17		✓	
Nigeria: Chabo - Hausa	2020-05-14		✓	
Nigeria: Deebam - NDV	2016-01-04		✓	
Nigeria: Eggon - Gwandere	2014-04-26		✓	
Nigeria: Fulani - Jukun	2014-04-15		✓	
Nigeria: Fulani - Kadara	2017-07-13		✓	
Nigeria: Fulani - Mambila	2017-06-16		✓	
Nigeria: Fulani - Yandang	2018-07-04		✓	
Nigeria: Government of Cameroon - Ambazonia insurgents	2018-02-23	✓		
Nigeria: Government of Cameroon - IS	2015-12-20	✓		
Nigeria: Government of Nigeria - IS	2015-03-15	✓		
Nigeria: Hausa - Jukun	2013-02-22		✓	
Nigeria: Hausa - Kadara	2018-10-18		✓	
Nigeria: IS - Civilians	2015-03-14			✓
Nigeria: IS - Yan Gora	2015-04-03		✓	
Nigeria: Ichen - Tiv	2020-04-30		✓	
Nigeria: Ikot-Offiong community - Oku Iboku community	2016-12-27		✓	
Nigeria: Ikurav (Tiv) - Shitile (Tiv)	2019-01-01		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Nigeria: Jama'atu Ahlis Sunna Lidda'awati wal-Jihad - Civilians	2019-01-28			✓
Nigeria: Jama'atu Ahlis Sunna Lidda'awati wal-Jihad - Yan Gora	2013-07-25		✓	
Nigeria: Jole - Shomo	2020-04-13		✓	
Nigeria: Kuteb - Tiv	2013-04-09		✓	
Nigeria: Lunguda - Waja	2018-10-23		✓	
Nigeria: Supporters of APC - Supporters of PDP	2014-06-20		✓	
Nigeria: Wanhihem community - Wanikade community	2017-06-28		✓	
Nigeria: Yan Sakai - Civilians	2018-01-01			✓
Pakistan: Ansaar ul-Islam - TTP	2013-01-25		✓	
Pakistan: BLA - Civilians	2004-11-25			✓
Pakistan: BLA - UBA	2015-06-30		✓	
Pakistan: Bangesh - Mangal	2010-09-02		✓	
Pakistan: Forces of Momin Afridi - TTP - TA	2010-08-29		✓	
Pakistan: Government of Afghanistan - Civilians	2012-11-11			✓
Pakistan: Government of Afghanistan - Taleban	2005-07-15	✓		
Pakistan: Government of Iran - Jaish al-Adl	2017-05-27	✓		
Pakistan: Government of Iran - Jondullah	2005-08-22	✓		
Pakistan: Government of Iraq - IS	2017-07-03	✓		

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Pakistan: Government of Pakistan - BLA	2004-06-25	✓		
Pakistan: Government of Pakistan - BLF	2012-01-11	✓		
Pakistan: Government of Pakistan - BRA	2014-06-05	✓		
Pakistan: Government of Pakistan - BRAS	2019-02-17	✓		
Pakistan: Government of Pakistan - IMU	2013-05-12	✓		
Pakistan: Government of Pakistan - IS	2016-02-12	✓		
Pakistan: Government of Pakistan - Jamaat-ul-Ahrar	2014-03-03	✓		
Pakistan: Government of Pakistan - TTP - TA	2012-06-02	✓		
Pakistan: Government of Pakistan - UBA	2012-10-08	✓		
Pakistan: IS - Civilians	2015-01-30			✓
Pakistan: IS - Taleban	2017-04-29		✓	
Pakistan: Jaish-ul-Islam - Civilians	2014-01-01			✓
Pakistan: Jondullah - Civilians	2012-02-28			✓
Pakistan: Lashkar of Kukikhel clan - TTP	2011-08-16		✓	
Pakistan: Lashkar of Mohmand tribe - TTP	2009-07-04		✓	
Pakistan: Lashkar of Orakzai tribe - TTP	2009-08-18		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Pakistan: Lashkar-e-Islam - Lashkar of Akakhel tribe	2012-06-04		✓	
Pakistan: Lashkar-e-Islam - Lashkar of Zakakhel tribe	2011-03-20		✓	
Pakistan: Lashkar-e-Islam - TTP	2010-02-18		✓	
Pakistan: Lashkar-e-Islam - TTP - TA	2011-01-20		✓	
Pakistan: Lashkar-e-Islam - Tawheed ul-Islam	2012-03-02		✓	
Pakistan: Laskhar of Masozai Qaumi tribe - TTP	2011-01-06		✓	
Pakistan: TTP - MR - TTP - MT	2010-02-17		✓	
Pakistan: TTP-Islahi - Civilians	2012-02-17			✓
Pakistan: TTP-KM - TTP-SM	2014-04-06		✓	
Pakistan: Taleban - Civilians	2016-07-31			✓
Pakistan: Taleban - High Council of Afghanistan Islamic Emirate	2019-08-16		✓	
Pakistan: UBA - Civilians	2012-09-13			✓
Pakistan: al-Qaida - Civilians	2005-05-31			✓
Papua New Guinea: Oi Kuru - Libe Koi	2019-06-01		✓	
Peru: Government of Colombia - FARC dissidents	2020-02-13	✓		
Philippines: Al-Khobar - Civilians	2007-06-01			✓
Philippines: BIFM - MILF	2011-08-09		✓	
Philippines: CPP - Civilians	2006-01-28			✓
Philippines: Forces of Jing Caludtiag - Forces of Randy Karon	2009-05-28		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Philippines: Government of Malaysia - Sultanate of Sulu	2013-04-24	✓		
Philippines: Government of Philippines - ASG	2014-04-11	✓		
Philippines: Government of Philippines - BIFM	2012-08-05	✓		
Philippines: Government of Philippines - BIFM-K	2016-07-26	✓		
Philippines: Government of Philippines - CPP	2018-01-01	✓		
Philippines: Government of Philippines - Civilians	2019-03-30			✓
Philippines: Government of Philippines - IS	2016-05-26	✓		
Philippines: Government of Philippines - MILF	2015-01-25	✓		
Philippines: Government of Philippines - MNLF - HM	2007-04-13	✓		
Philippines: Government of Philippines - MNLF - NM	2013-09-09	✓		
Philippines: Government of Philippines - Maute group	2016-02-20	✓		
Philippines: Government of Philippines - al-Harakat al-Islamiyah	2016-01-19	✓		
Philippines: IS - Civilians	2016-09-02			✓
Philippines: Lumad - Moro	2006-06-01		✓	

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Russia (Soviet Union): Forces of the Caucasus Emirate - Civilians	2007-11-22			✓
Russia (Soviet Union): Gazotan Murdash - Civilians	2004-02-06			✓
Russia (Soviet Union): Government of Russia (Soviet Union) - Forces of the Caucasus Emirate	2007-10-07	✓		
Russia (Soviet Union): Government of Russia (Soviet Union) - IS	2015-06-25	✓		
Russia (Soviet Union): Government of Syria - Syrian insurgents	2020-10-15	✓		
Russia (Soviet Union): Government of Ukraine - LPR	2014-07-13	✓		
Russia (Soviet Union): IS - Civilians	2015-09-30			✓
Rwanda: Government of Rwanda - FDLR-RUD	2019-10-04	✓		
Saudi Arabia: Government of Yemen - (North Yemen) - Civilians	2019-06-23			✓
Spain: IS - Civilians	2017-08-17			✓
Sri Lanka: IS - Civilians	2019-04-21			✓
Sweden: IS - Civilians	2017-04-07			✓
Tajikistan: IS - Civilians	2018-07-29			✓
Tanzania: Government of Burundi - Civilians	2017-01-17			✓
Tanzania: IS - Civilians	2019-11-12			✓
Thailand: Government of Myanmar (Burma) - KNPP	2005-01-25	✓		

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Thailand: Patani insurgents - Civilians	2013-05-01			✓
Tunisia: AQIM - Civilians	2015-10-11			✓
Tunisia: Government of Tunisia - IS	2015-03-18	✓		
Tunisia: IS - Civilians	2015-03-18			✓
Turkey: 23rd Division, Ahrar al-Sham, Jaysh al Nasr, Jaysh al-Nukhba, Nour al-Din Zenki, Sham Legion, SNA - SDF	2018-02-02		✓	
Turkey: Government of Russia (Soviet Union) - Forces of the Caucasus Emirate	2011-09-12	✓		
Turkey: Government of Syria - Union) - Syrian insurgents	2012-10-03	✓		
Turkey: Government of Turkey - IS	2015-07-23	✓		
Turkey: Government of Turkey - MKP	2005-01-15	✓		
Turkey: Government of Turkey - TAK	2005-04-30	✓		
Turkey: Government of Turkey - Yurtta Sulh Konseyi	2016-07-15	✓		
Turkey: IS - Civilians	2015-06-05			✓
Turkey: IS, Jabhat Fateh al-Sham - PYD	2013-07-26		✓	
Turkey: SNA - SDF	2019-10-10		✓	
Turkey: TAK - Civilians	2004-08-10			✓
Ukraine: Government of Ukraine - DPR	2014-04-12	✓		
Ukraine: Government of Ukraine - LPR	2014-05-04	✓		
Ukraine: Government of Ukraine - Maidan	2014-01-22	✓		
Ukraine: Government of Ukraine - United Armed Forces of Novorossiya	2014-09-17	✓		

Continued on next page

Table A1 – continued from previous page

Conflict name	Onset	State	Non-state	One-sided
Ukraine: Supporters of independence for Eastern Ukraine - Supporters of Ukrainian unity	2014-05-02		✓	
United Kingdom: Government of Iraq - IS	2019-11-29	✓		
United Kingdom: IS - Civilians	2019-11-29			✓
Venezuela: Government of Colombia - FARC	2007-04-01	✓		
Venezuela: Government of Venezuela - Civilians	1998-05-24			✓

Appendix tables

Table A2: Sample sizes by conflict type

Group	Non-state (1)	One-sided (2)	State (3)	Total (4)
All				
Conflicts	128	56	78	262
Episodes	159	63	91	313
Countries	95	105	106	120
Bonds	1,374	1,406	1,420	1,731
Bond-days	868,491	592,518	719,778	1,082,119
Treated				
Countries	19	29	30	44
Bonds	318	364	377	667
Bond-days	61,779	19,603	32,348	108,093
Control				
Countries	76	76	76	76
Bonds	1,056	1,042	1,043	1,064
Bond-days	806,712	572,915	687,430	974,026

Table shows counts of unique conflicts, episodes, countries, bonds, and bond-days for the full sample, as well as treatment vs. control, by conflict type.

Table A3: Conflict onset and bond prices: wild cluster bootstrap

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
p-value	0.752	0.018	0.852	0.778
95% CI	[-1.065, 0.469]	[-1.343, -0.112]	[-1.86, 1.777]	[-0.759, 1.272]

Table shows p-values and confidence sets for the estimates in Table 1 from a wild cluster bootstrap procedure (Cameron et al., 2008), with clustering at the country-level. Sample is daily bond panel in stacked event-specific datasets. Outcome variable is the daily bond trading price averaged across all available exchanges, indexed to 100 (par). Each column provides estimates of treatment effects for a different sample of conflicts, indicated in the table header.

Table A4: Conflict onset and bond prices: one-sided state-violence

Dependent variable	Bond price (1)
State forces, all episodes	
Post × Treated	-0.344
	(0.242)
Bond FE × Event FE	Yes
Date FE× Event FE× Maturity	Yes
Observations	654,968

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all one-sided conflict involving state forces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Conflict onset and bond prices: credit ratings

Dependent variable	Bond price			
	(1)	(2)	(3)	(4)
Post × Treated	-1.632**	-1.973**	-0.899**	-0.691**
	(0.627)	(0.757)	(0.236)	(0.282)
Post × Treated × Rating	0.094**	0.165		
	(0.047)	(0.107)		
Post × Treated × Investment grade			1.055***	1.562***
			(0.290)	(0.554)
Post × Treated × GDP per capita	No	Yes	No	Yes
Bond FE × Event FE	Yes	Yes	Yes	Yes
Date FE× Event FE× Maturity	Yes	Yes	Yes	Yes
Observations	1,305,052	1,304,083	1,305,052	1,304,083

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict involving state forces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Conflict onset and military spending

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
	(1)	(2)	(3)	(4)
Post × Treated	0.025**	0.033	0.021	0.021
	(0.012)	(0.025)	(0.015)	(0.024)
Country × Event FE	Yes	Yes	Yes	Yes
Date × Event FE	Yes	Yes	Yes	Yes
Events	313	91	159	63
Conflicts	262	78	128	56
Countries	108	94	83	93
Observations	152,796	45,788	77,451	29,557

Note: Standard errors in parentheses clustered at the country level. Sample is yearly military spending panel in stacked event-specific datasets. Outcome variable is the logarithm of military spending. Each column provides estimates of treatment effects for a different sample of conflicts, indicated in the table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7: Conflict onset and non-military spending

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
	(1)	(2)	(3)	(4)
Post × Treated	0.020	0.028	0.009	0.033
	(0.013)	(0.027)	(0.018)	(0.028)
Country × Event FE	Yes	Yes	Yes	Yes
Date × Event FE	Yes	Yes	Yes	Yes
Events	313	91	159	63
Conflicts	262	78	128	56
Countries	108	94	83	93
Observations	116,666	34,284	59,502	22,880

Note: Standard errors in parentheses clustered at the country level. Sample is yearly military spending panel in stacked event-specific datasets. Outcome variable is the logarithm of non-military spending. Each column provides estimates of treatment effects for a different sample of conflicts, indicated in the table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: Conflict onset and inflation rate

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
	(1)	(2)	(3)	(4)
Post × Treated	1.281***	1.545***	1.563***	-0.025
	(0.320)	(0.607)	(0.433)	(0.720)
Country × Event FE	Yes	Yes	Yes	Yes
Date × Event FE	Yes	Yes	Yes	Yes
Events	313	91	159	63
Conflicts	262	78	128	56
Countries	119	108	97	107
Observations	181,178	54,691	92,473	34,014

Note: Standard errors in parentheses clustered at the country level. Sample is yearly military spending panel in stacked event-specific datasets. Outcome variable is the annual inflation rate. Each column provides estimates of treatment effects for a different sample of conflicts, indicated in the table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: Fatalities across types of conflict

Dependent variable	Number of fatalities							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State	-31.243				-1.102			
	(20.365)				(7.314)			
Non-state		48.877*		46.940*		2.950		2.600
		(24.402)		(25.244)		(6.520)		(7.776)
One-sided			-33.801*	-4.635			-2.091	-0.851
			(18.731)	(11.647)			(7.575)	(8.944)
Observations	262	262	262	262	262	262	262	262
Country FE	No	No	No	No	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the number of fatalities during conflict onset. Event sample is all conflicts. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10: Parameter estimates for conflict cost and autocorrelation

Outcome	$\log(y_{it})$				z_{it}
	(1)	(2)	(3)	(4)	(5)
γ	1.104***	-0.091*	-0.058**		
	(0.317)	(0.054)	(0.027)		
γ^H				-0.157***	
				(0.040)	
γ^L				-0.048*	
				(0.026)	
ρ					0.801***
					(0.022)
α					0.028***
					(0.004)
Country FE	No	Yes	Yes	Yes	No
Year FE	Yes	No	Yes	Yes	No
Observations	5,932	5,928	5,928	5,925	6,758

Note: Standard errors in parentheses clustered at the country level. Outcome variable is either the log of constant-dollar GDP or a conflict dummy, as indicated in the table header. Parameters refer to those indicated in Section A. Sample is all country-years for which data is available from 1990-2000 *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A11: Simulation parameter list

Parameter	Description	Value
γ	Annual cost of conflict	0.058
α	AR(1) intercept	0.028
ρ	AR(1) autoregressive term	0.801
ζ_0	Prior probability of conflict	0.140
$\tilde{\zeta}_0$	Posterior probability of conflict	1
r	Risk-free rate	0.029
i	Coupon rate	0.055
T	Maturity	10

Table shows estimated values and descriptions for each parameter of the simulation exercise in Section A.

Table A12: Conflict onset and bond prices by maturity

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
	(1)	(2)	(3)	(4)
Post × Treated	-0.470***	-0.581**	-0.460**	-0.295
	(0.130)	(0.292)	(0.189)	(0.216)
Maturity bin=2 × Post × Treated	0.019	-0.310	0.154	0.019
	(0.137)	(0.450)	(0.125)	(0.254)
Maturity bin=4 × Post × Treated	0.257*	0.010	0.347**	0.231
	(0.138)	(0.234)	(0.161)	(0.344)
Maturity bin=6 × Post × Treated	0.307*	0.172	0.295	0.669*
	(0.182)	(0.357)	(0.219)	(0.350)
Maturity bin=8 × Post × Treated	0.314*	-0.150	0.326**	1.017**
	(0.184)	(0.511)	(0.148)	(0.410)
Maturity bin=10 × Post × Treated	0.797**	0.434	1.002*	0.547
	(0.370)	(0.609)	(0.532)	(0.569)
Maturity bin=12 × Post × Treated	0.973***	0.500	0.983***	1.400**
	(0.287)	(0.655)	(0.316)	(0.563)
Maturity bin=14 × Post × Treated	2.181*	0.567	3.366***	-0.216
	(1.202)	(0.659)	(1.127)	(0.507)
Maturity bin=15 × Post × Treated	0.689***	0.823	0.675***	0.507
	(0.240)	(0.640)	(0.184)	(0.623)
Bond × event FE	Yes	Yes	Yes	Yes
Day × event FE	Yes	Yes	Yes	Yes
Events	313	91	159	63
Conflicts	262	78	128	56
Countries	120	106	95	105
Observations	4478557	1304579	2262806	911172
R^2	0.979	0.976	0.980	0.980

Note: Standard errors in parentheses clustered at the country level. Event sample is all conflict involving state forces. Outcome variable is the daily bond trading price averaged across all available exchanges, indexed to 100 (par). Maturity bins are $2 = [2,3], 4 = [4,5], 6 = [6,7], 8 = [8,9], 10 = [10,11], 12 = [12,13], 14 = [14,15], 15 => 15$. Omitted bin is $0 = [0,1]$. Maturity is measured as the rounded number of years from the trading date to full repayment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13: Conflict onset and bond prices: first episodes

Dependent variable	Bond price							
Episodes	First		Subsequent			All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	-0.665*	-0.942***	0.025	-0.192	-0.925	0.025	-0.192	0.404
	(0.345)	(0.356)	(0.466)	(0.499)	(0.651)	(0.466)	(0.499)	(0.452)
Post × Treated × First episode						-0.690	-0.750	-1.347**
						(0.725)	(0.737)	(0.658)
Bond × Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day × Event FE	Yes	No	Yes	No	No	Yes	No	No
Day × Event × maturity FE	No	Yes	No	Yes	Yes	No	Yes	Yes
Onset					No			Yes
Events	68	68	23	23	11	91	91	80
Conflicts	67	67	11	11	10	78	78	68
Countries	102	102	85	85	76	106	106	104
Observations	952,489	935,938	352,090	346,207	146,811	1,304,579	1,282,145	1,135,334

Standard errors in parentheses clustered at the country level. Sample is daily bond panel in stacked event-specific datasets. Event sample is all conflict involving state forces. Outcome variable is the daily bond trading price averaged across all available exchanges, indexed to 100 (par). Columns provide estimates of treatment effects initial, subsequent, or all events, as indicated in the table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A14: Conflict onset and bond prices: distance to capital

Dependent variable	Bond price			
Episodes	First		All	
	(1)	(2)	(3)	(4)
Post × Treated	-1.181*	-1.551**	-0.920	-1.318*
	(0.597)	(0.769)	(0.562)	(0.751)
Post × Treated × Log distance to capital	0.111	0.135	0.097	0.126
	(0.089)	(0.117)	(0.090)	(0.119)
Bond × event FE	Yes	Yes	Yes	Yes
Day × event	Yes	No	Yes	No
Day × event × maturity FE	No	Yes	No	Yes
Events	68	68	91	91
Countries	102	102	106	106
Observations	952,489	935,938	1,304,579	1,282,145

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflicts involving state forces. Estimates are for either first events or all events, as indicted in table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A15: Conflict onset and bond prices: distance to other major cities

Dependent variable	Bond price (1)
State forces, all episodes	
Post × Treated	-0.801
	(1.630)
Post × Treated × Log distance Index	0.047
	(0.273)
Bond FE × Event FE	Yes
Date FE× Event FE× Maturity	Yes
Observations	1,364,812

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflicts involving state forces. The log distance index is the logarithm of an average distance from conflict event location to four major cities after the capital weighted by population level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A16: Conflict onset, news pressure, and information flows

Dependent variable	Bond price			
Quartile of news pressure	4	3	2	1
	(1)	(2)	(3)	(4)
Post × Treated	-0.251	-0.269	-0.024	1.699
	(0.609)	(0.462)	(0.548)	(1.328)
Post × Treated × Above median news coverage, pre-onset	-0.326	-0.529	1.202***	1.320*
	(0.796)	(0.924)	(0.457)	(0.760)
Post × Treated × Above median news coverage, post-onset	-0.423	-0.404	-1.722	-3.213***
	(1.024)	(1.320)	(1.228)	(1.190)
Bond × event FE	Yes	Yes	Yes	Yes
Day × event × maturity FE	Yes	Yes	Yes	Yes
Observations	890579	691883	477680	278017
R^2	0.976	0.978	0.986	0.987

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Pre-onset news is measured as the sum of conflict-specific news articles in the 30 days before the conflict onset date. Post-onset news is measured as the sum of conflict-specific news articles in the 15 days after the conflict onset date. Event sample is all conflicts involving state forces. Header indicates the sample is all conflict events with average daily news pressure greater than a given quartile of the event-level distribution. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A17: Conflict onset, news pressure, and information flows: with controls

Dependent variable	Bond price			
Quartile of news pressure	4	3	2	1
	(1)	(2)	(3)	(4)
Post × Treated	1.211*	1.154	1.525	1.804
	(0.621)	(0.801)	(1.023)	(1.493)
Post × Treated × Above median news coverage, pre-onset	-0.565	-0.974	0.741	1.194
	(0.736)	(0.939)	(0.555)	(0.785)
Post × Treated × Above median news coverage, post-onset	-0.895	-0.725	-1.921**	-3.098**
	(0.977)	(1.080)	(0.960)	(1.203)
Post × Treated × Within 50 miles	-2.311**	-3.607**	-3.088***	-0.910
	(1.107)	(1.464)	(1.049)	(1.111)
Post × Treated × Center-seeking	-1.466	-1.213	-2.448**	0.013
	(1.021)	(1.067)	(1.151)	(0.849)
Post × Treated × Fatalities	-1.429***	-1.297***	-1.372***	-0.085
	(0.286)	(0.397)	(0.465)	(0.347)
Bond × event FE	Yes	Yes	Yes	Yes
Day × event × maturity FE	Yes	Yes	Yes	Yes
Observations	890579	691883	477680	278017
R^2	0.976	0.978	0.986	0.987

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Pre-onset news is measured as the sum of conflict-specific news articles in the 30 days before the conflict onset date. Post-onset news is measured as the sum of conflict-specific news articles in the 15 days after the conflict onset date. Event sample is all conflicts involving state forces. Header indicates the sample is all conflict events with average daily news pressure greater than a given quartile of the event-level distribution. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A18: Conflict onset and bond prices: robustness to bond characteristics

Dependent variable	Bond price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: State conflicts, all episodes								
Post × Treated	-0.476**	-0.661***	-0.414*	-0.618***	-0.503**	-0.700***	-0.482**	-0.714***
	(0.225)	(0.240)	(0.209)	(0.218)	(0.232)	(0.239)	(0.241)	(0.250)
Observations	1,248,212	1,228,026	1,241,792	1,142,820	1,240,656	1,220,647	1,199,700	1,174,954
Panel B: State conflicts, first episode								
Post × Treated	-0.681**	-0.946***	-0.694*	-0.944***	-0.729**	-1.005***	-0.626*	-0.928**
	(0.342)	(0.350)	(0.350)	(0.356)	(0.355)	(0.356)	(0.361)	(0.367)
Observations	1,028,811	1,012,058	1,023,315	940,770	1,022,718	1,006,142	988,747	968,296
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE × Event FE	Yes	No	No	No	Yes	No	Yes	No
Day FE × Event FE × Maturity	No	Yes	No	No	No	Yes	No	Yes
Day FE × Event FE × Currency FE	No	No	Yes	No	No	No	No	No
Day FE × Event FE × Maturity × Currency FE	No	No	No	Yes	No	No	No	No
Day FE × Event FE × Loan size	No	No	No	No	Yes	Yes	No	No
Day FE × Event FE × Coupon	No	No	No	No	No	No	Yes	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict involving state forces. Estimates are for either first events or all events, as indicted in table header. Bond characteristics are in table footer. Controls enter either as continuous variables interacted with day-by-event effects or, for categorical variables, as indicators interacted with day-by-event effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A19: Conflict onset and bond prices: robustness to macroeconomic characteristics

Dependent variable	Bond price											
Control for	None		Mil exp	Growth	GDPPC	Population	FDI	BOP	Fin. sector	Unemp	Reserves	CPI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post × Treated	-0.356	-0.667***	-0.646**	-0.657**	-0.712***	-0.335	-0.649***	-0.724***	-0.657***	-0.693***	-0.666***	-0.666***
	(0.228)	(0.239)	(0.252)	(0.327)	(0.254)	(0.270)	(0.239)	(0.249)	(0.235)	(0.244)	(0.214)	(0.241)
Observations	991,175	964,204	964,204	964,204	964,204	964,204	964,204	964,204	964,204	964,204	964,204	964,204
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date FE × Event FE	Yes	No	No	No	No	No	No	No	No	No	No	No
Date FE × Event FE × Maturity	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict involving state forces. Country-level macroeconomic variables are in table header. All controls enter as continuous variables interacted with day-by-event effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A20: Conflict onset and bond prices: robustness to institutional risk factors

Dependent variable	Bond price					
Control for	None		Corruption	State capacity	Stability	All
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	-0.450**	-0.715***	-0.921**	-0.852***	-0.966**	-0.733**
	(0.205)	(0.233)	(0.359)	(0.295)	(0.436)	(0.298)
Observations	1,304,580	1,282,162	1,282,162	1,282,162	1,282,162	1,282,162
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE × Event FE	Yes	No	No	No	No	No
Date FE × Event FE × Maturity	No	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict involving state forces. Institutional risk factors are taken from the World Bank Worldwide Governance Indicators and measured at the country-year level. All controls enter as continuous variables interacted with day-by-event effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A21: Conflict onset and bond prices: robustness to country risk factors

Dependent variable	Bond price													
Control for	None		Composite	Economic	Financial	Payment	Political	Budget	Debt	Exchg rate	Growth	Inflation	Liquidity	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Post × Treated	-0.478**	-0.764***	-0.929***	-0.810***	-0.876***	-0.703***	-1.107***	-0.711***	-0.789***	-0.627***	-0.819***	-0.788***	-0.885***	-0.624**
	(0.209)	(0.244)	(0.327)	(0.265)	(0.303)	(0.255)	(0.420)	(0.240)	(0.249)	(0.229)	(0.276)	(0.274)	(0.312)	(0.282)
Observations	1,186,976	1,161,731	1,161,731	1,161,731	1,161,731	1,161,731	1,161,731	1,187,426	1,161,731	1,161,731	1,161,731	1,161,731	1,161,731	1,161,731
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date FE × Event FE	Yes	No	No	No	No	No	No	No	No	No	No	No	No	No
Date FE × Event FE × Maturity	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict involving state forces. ICRG risk rating controls are in table header. All controls enter as continuous variables interacted with day-by-event effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A22: Conflict onset and bond prices: robustness to resource dependence

Dependent variable	Bond price					
	(1)	(2)	(3)	(4)	(5)	(6)
State forces, all episodes						
Post × Treated	-0.451**	-0.717***	-0.699***	-0.696***	-0.621***	-0.620***
	(0.205)	(0.233)	(0.232)	(0.194)	(0.213)	(0.207)
Observations	1,298,237	1,275,502	1,275,502	1,275,502	1,275,502	1,275,502
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE × Event FE	Yes	No	No	No	No	No
Date FE × Event FE × Maturity	No	Yes	Yes	Yes	Yes	Yes
Date FE × Event FE × Mineral rents	No	No	Yes	No	No	Yes
Date FE × Event FE × Oil rents	No	No	No	Yes	No	Yes
Date FE × Event FE × Natural resources rents	No	No	No	No	Yes	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict involving state forces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A23: Conflict onset and bond prices (propensity score methods)

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
	(1)	(2)	(3)	(4)
Post × Treated	-0.085	-0.558**	0.074	0.054
	(0.114)	(0.231)	(0.137)	(0.296)
Bond × Event FE	Yes	Yes	Yes	Yes
Day × Event × Maturity FE	Yes	Yes	Yes	Yes
Events	313	91	159	63
Conflicts	262	78	128	56
Countries	108	94	83	93
Observations	2,002,964	577,161	1,017,742	408,061

Note: Standard errors in parentheses clustered at the country level. Sample is daily bond panel in stacked event-specific datasets as defined by the weighted sample in Figure A9. Outcome variable is the daily bond trading price averaged across all available exchanges, indexed to 100 (par). Each column provides estimates of treatment effects for a different sample of conflicts, indicated in the table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A24: Conflict onset and bond prices: robustness to global market indices

Dependent variable	Bond price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	-0.751***	-0.727***	-0.748***	-0.714***	-0.740***	-0.721***	-0.680***	-0.611***
	(0.261)	(0.230)	(0.261)	(0.232)	(0.246)	(0.231)	(0.220)	(0.185)
Dow Jones Industrial Average × Treated	0.000							0.002**
	(0.000)							(0.001)
NASDAQ Composite Index × Treated		0.000						0.006***
		(0.001)						(0.002)
S&P 500 × Treated			-0.000					-0.043**
			(0.002)					(0.016)
CBOE Volatility Index: VIX × Treated				-0.017				-0.424
				(0.047)				(0.267)
CBOE Emerging Markets ETF Volatility Index × Treated					-0.000			0.037
					(0.033)			(0.159)
Equity Market-related Economic Uncertainty Index × Treated						0.002		0.021**
						(0.002)		(0.009)
EMBI Global × Treated							-0.001	-0.001
							(0.000)	(0.001)
Observations	1,227,343	1,282,162	1,227,343	1,282,162	1,244,119	1,282,162	1,261,321	1,227,343
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Event FE× Maturity FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Sample is all state-involved conflict events. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A25: Conflict onset and bond prices: robustness to major commodity prices

Dependent variable	Bond price								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post × Treated	-0.691***	-0.694***	-0.694***	-0.732***	-0.710***	-0.695***	-0.719***	-0.719***	-0.665***
	(0.225)	(0.211)	(0.214)	(0.252)	(0.247)	(0.221)	(0.236)	(0.229)	(0.232)
All Commodity Price Index × Treated	0.032								0.581
	(0.050)								(0.545)
Food Price Index × Treated		0.105							0.973
		(0.110)							(0.595)
Agriculture Price Index × Treated			0.098						-1.157
			(0.118)						(0.742)
All Metals Index × Treated				-0.034					-0.132
				(0.027)					(0.151)
Precious Metals Price Index × Treated					-0.094**				-0.153**
					(0.038)				(0.063)
Crude Oil (petroleum) Price index × Treated						0.016			-0.159
						(0.018)			(0.166)
Natural Gas Price Index × Treated							-0.003		-0.072
							(0.041)		(0.069)
Coal Price Index × Treated								0.016	0.012
								(0.039)	(0.026)
Observations	1,282,162	1,282,162	1,282,162	1,282,162	1,282,162	1,282,162	1,282,162	1,282,162	1,282,162
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date FE × Event FE × Maturity FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Sample is all state-involved conflict events. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A26: Conflict onset and bond prices: non-overlapping events

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
	(1)	(2)	(3)	(4)
Post × Treated	-0.259	-0.845**	-0.146	0.500
	(0.161)	(0.354)	(0.178)	(0.331)
Bond × event FE	Yes	Yes	Yes	Yes
Day × event × maturity FE	Yes	Yes	Yes	Yes
Events	147	54	54	39
Conflicts	127	49	45	33
Countries	118	101	92	99
Observations	2027928	757537	700135	570256
R^2	0.983	0.976	0.987	0.987

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Sample is all non-overlapping conflict events. Overlapping events are defined as events that occur within the same event window in the same country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A27: Conflict onset and ICRG risk measures

Dependent variable	ICRG risk rating			
Sub-index	Civil war	Internal conflict	Economic	Composite
	(1)	(2)	(3)	(4)
Post × Treated	-0.061**	-0.107**	0.013	-0.121
	(0.025)	(0.047)	(0.183)	(0.113)
Post × Treated × State conflict	0.001	0.041	-0.916**	-0.434
	(0.073)	(0.104)	(0.404)	(0.390)
Country × event FE	Yes	Yes	Yes	Yes
Day × event FE	Yes	Yes	Yes	Yes
Mean dep var	3.891	9.720	36.081	71.876
Events	313	313	313	313
Conflicts	262	262	262	262
Countries	100	100	100	100
Observations	300118	300118	300118	300118
R^2	0.986	0.950	0.897	0.956

Note: Standard errors in parentheses clustered at the country level. Risk ratings come from ICRG and are normalized such that a lower rating indicates more risk. Sample is all conflict events. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A28: Conflict onset and bond prices: region FE

Dependent variable	Bond price			
Conflicts	All	State	Non-state	One-sided
	(1)	(2)	(3)	(4)
Post × Treated	-0.144	-0.499***	-0.050	-0.009
	(0.195)	(0.235)	(0.283)	(0.280)
Bond × Event FE	Yes	Yes	Yes	Yes
Day × Region × Event FE	Yes	Yes	Yes	Yes
Events	313	91	159	63
Conflicts	262	78	128	56
Countries	108	94	83	93
Regions	5	5	5	5
Observations	4,480,218	1,305,052	2,263,739	911,427

Note: Standard errors in parentheses clustered at the country level. Sample is daily bond panel in stacked event-specific datasets. Outcome variable is the daily bond trading price averaged across all available exchanges, indexed to 100 (par). Each column provides estimates of treatment effects for a different sample of conflicts, indicated in the table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A29: Conflict onset and bond prices: robustness to level of record precision

Dependent variable	Bond price
	(1)
State forces, all episodes	
Post × Treated	-0.798*
	(0.471)
Post × Treated × Precision (quintile 2)	-0.003
	(0.969)
Post × Treated × Precision (quintile 3)	0.784
	(0.641)
Post × Treated × Precision (quintile 4)	0.230
	(0.613)
Post × Treated × Precision (quintile 5)	0.428
	(1.368)
Observations	1,282,162
Bond FE × Event FE	Yes
Date FE× Event FE× Maturity	Yes

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict involving state forces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A30: Conflict onset and bond prices: robustness to outliers

Dependent variable	Bond price							
Events	All				First			
Percentile	5/95		1/99		5/95		1/99	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	-0.716***	-0.806***	-0.709***	-0.703***	-1.010***	-1.075***	-1.006***	-1.013***
	(0.231)	(0.275)	(0.235)	(0.239)	(0.343)	(0.379)	(0.344)	(0.354)
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE× Event FE × Maturity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,228,026	1,109,758	1,228,026	1,204,033	1,012,058	912,081	1,012,058	991,354

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). Event sample is all conflict involving state forces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A31: Conflict onset and bond prices: filled-in and balanced samples

Dependent variable	Bond price	
	Filled-in	Balanced
	(1)	(2)
Post × Treated	-0.385***	-0.798***
	(0.144)	(0.261)
Bond FE × Event FE	Yes	Yes
Day FE × Event FE × Maturity	Yes	Yes
Events	236	68
Conflicts	236	68
Countries	120	102
Observations	2,871,361	1,262,230

Note: Standard errors in parentheses clustered at the country level. Outcome variable is the daily bond price, indexed to 100 (par). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A32: Conflict onset and yield spreads

Events	All						First			
Fatality quintile	1	2	3	4	5		Full sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Yield spreads (basis points)										
Post × Treated	5.292**	6.032**	9.187**	11.976**	18.185***	20.848***	6.444**	12.405**	11.875**	7.454***
	(2.131)	(2.696)	(4.552)	(5.315)	(6.525)	(7.898)	(2.664)	(5.792)	(5.694)	(2.810)
Post × Treated × Log distance to capital						-2.935**				
						(1.179)				
Post × Treated × Conflict index								-9.363		
								(6.219)		
Post × Treated × Minor conflict index									-8.891	
									(6.253)	
Post × Treated × Major conflict index										-10.483**
										(4.322)
Observations	1,174,954	891,311	711,264	514,609	256,364	1,174,954	968,296	968,296	968,296	968,296
Panel B: Yield spreads (log)										
Post × Treated	0.007***	0.007**	0.015**	0.019**	0.025***	0.024***	0.010***	0.020**	0.019**	0.012***
	(0.003)	(0.003)	(0.006)	(0.007)	(0.008)	(0.008)	(0.004)	(0.008)	(0.008)	(0.004)
Post × Treated × Log distance to capital						-0.003**				
						(0.001)				
Post × Treated × Conflict index								-0.015*		
								(0.009)		
Post × Treated × Minor conflict index									-0.014	
									(0.009)	
Post × Treated × Major conflict index										-0.017**
										(0.007)
Observations	1,168,210	886,573	707,531	511,872	255,140	1,168,210	962,771	962,771	962,771	962,771
Bond FE × Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE × Event FE × Maturity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

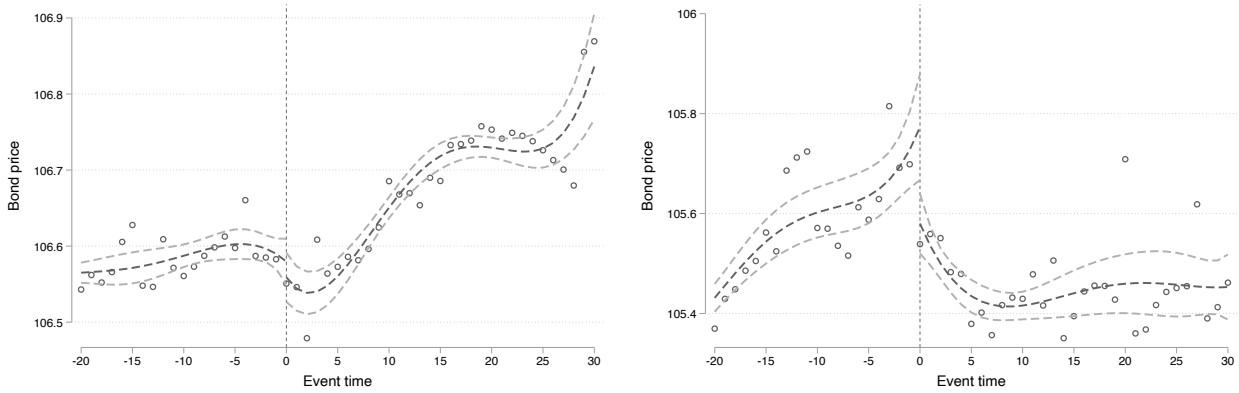
Note: Standard errors in parentheses clustered at the country level. Outcome variable is either daily current yields or log yields, as indicated in the panel headers. Event sample is all conflict involving state forces, with subsamples given in table header. Fatality quintiles indicate samples of events with fatalities greater than or equal to the quintile in the table header. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Figures

Figure A1: Interrupted time series

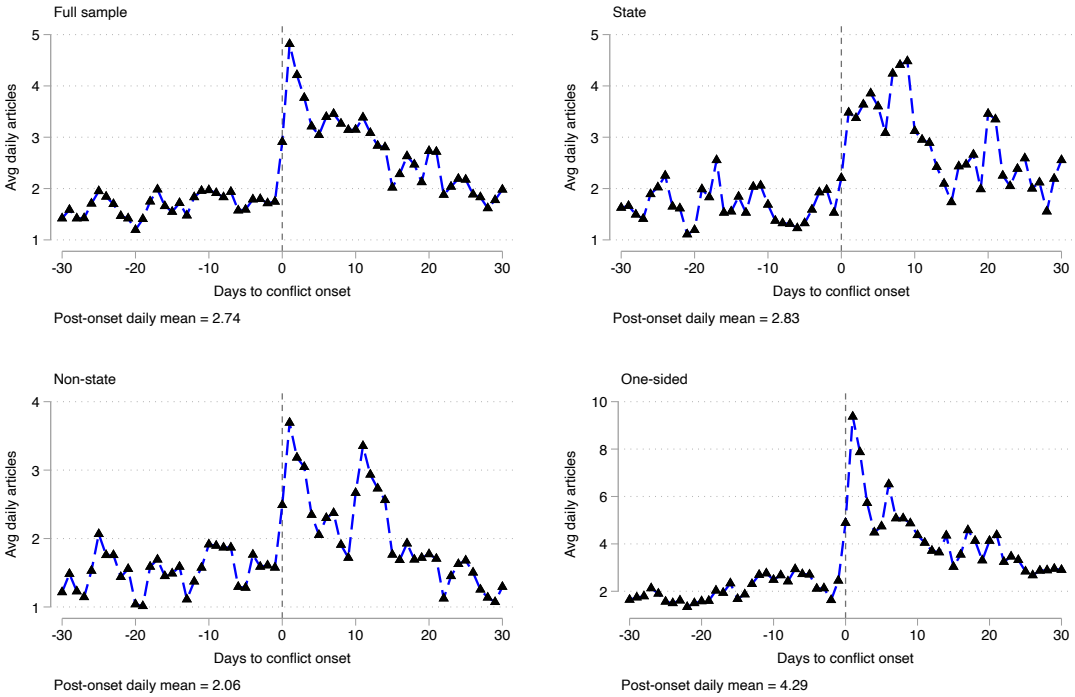
(a) Control bonds

(b) Treated bonds



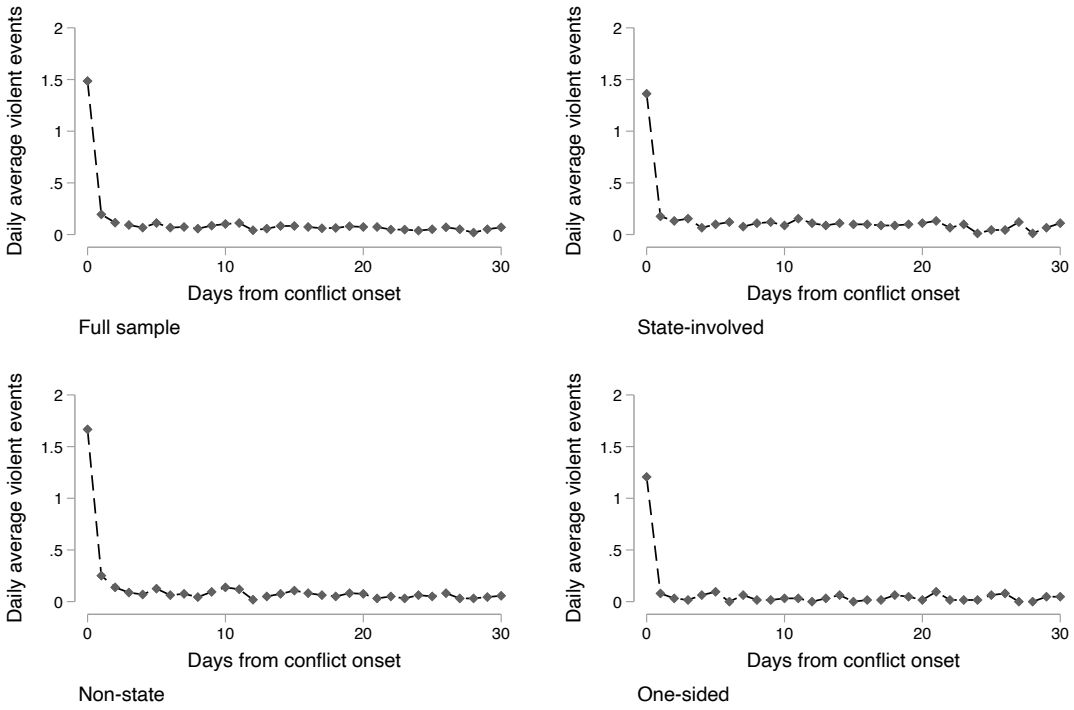
Note: Figure shows results from interrupted time series model of bond prices. Panel (a) and (b) show average bond prices over event-days for control and treated bonds, respectively. Each plot fits a cubic model separately on either side of the event date.

Figure A2: News media coverage by conflict type



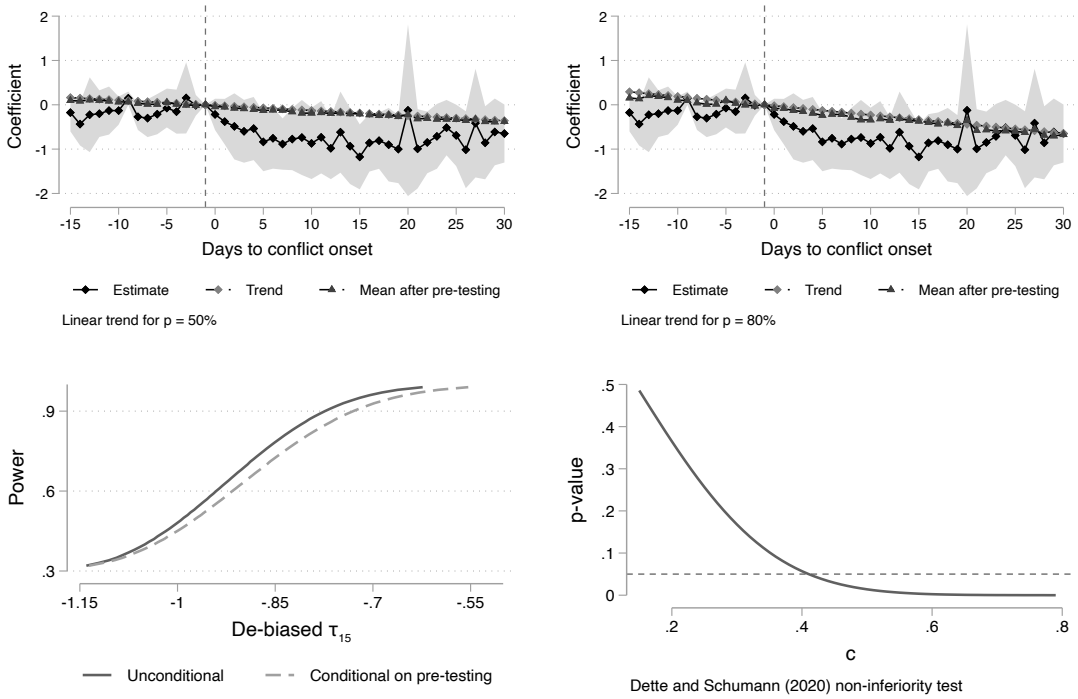
Note: Figure shows average daily number of conflict-related news articles by conflict type from LexisNexis. Each subfigure also includes the average daily number of articles during the 30 days after conflict onset, across conflicts events within conflict groups.

Figure A3: Daily conflict event trends



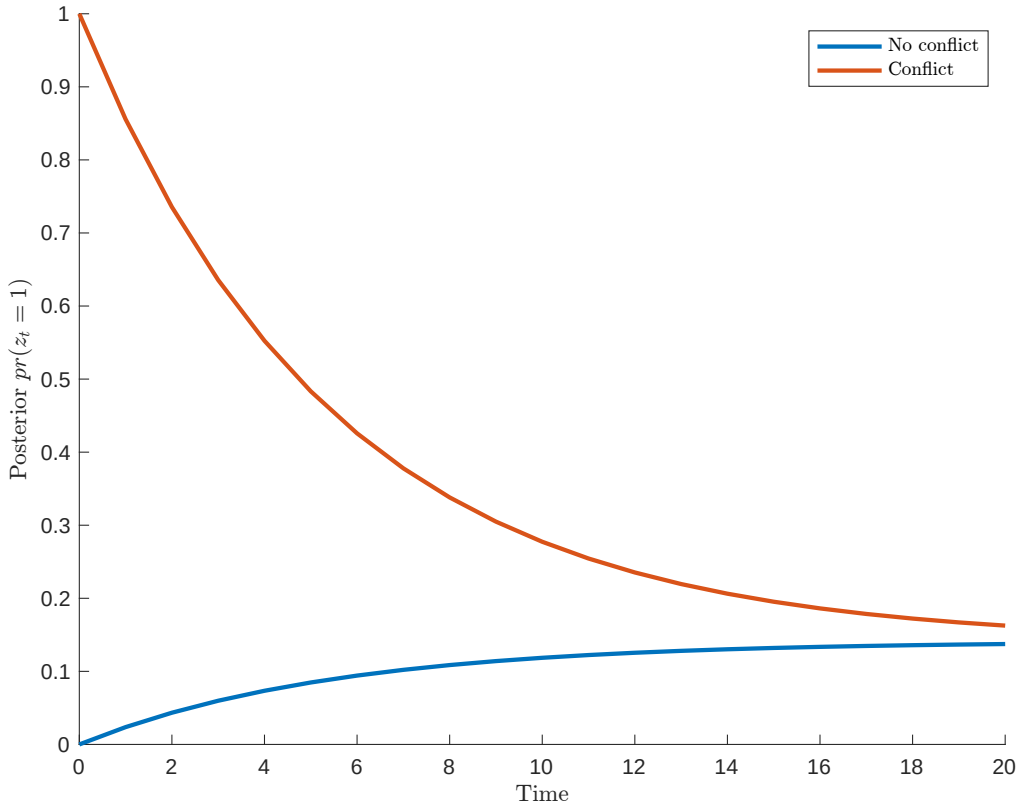
Note: Figure shows average daily number of conflict events following onset date from UCDP, averaged across conflicts events within conflict groups.

Figure A4: Parallel trends power analysis



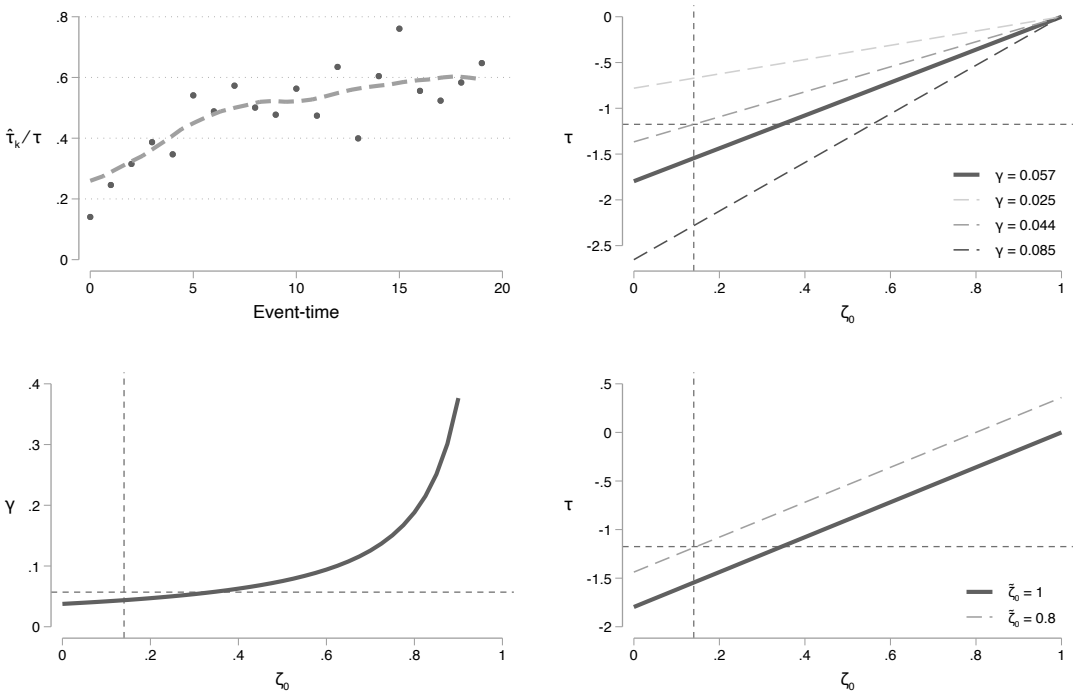
Note: Figure shows pre-test power analysis from Roth (2022). Top panel plots event-study coefficients from Figure 2 for state-involved conflicts. Overlaid are hypothesized linear trends before and after pretesting, with slopes detectable with 0.5 and 0.8 power (top left and right, respectively). Bottom-left panel shows de-biased treatment effect at $t = 15$ under trends detectable with different levels of power, before and after pre-testing. Bottom-right shows parallel trends non-inferiority test from Dette and Schumann (2020), plotting p-values for multiple levels of c.

Figure A5: Path of conflict probability beliefs



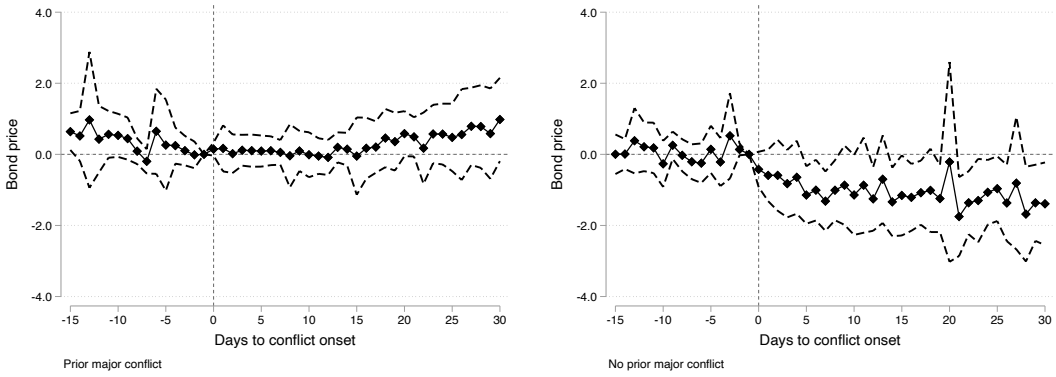
Note: Figure shows the simulated path of beliefs ζ_t on the probability of conflict after observing an event of either conflict or no-conflict. Beliefs are derived from the Markov process of the AR(1) estimates in equation (5).

Figure A6: Prior beliefs, conflict costs, and price responses



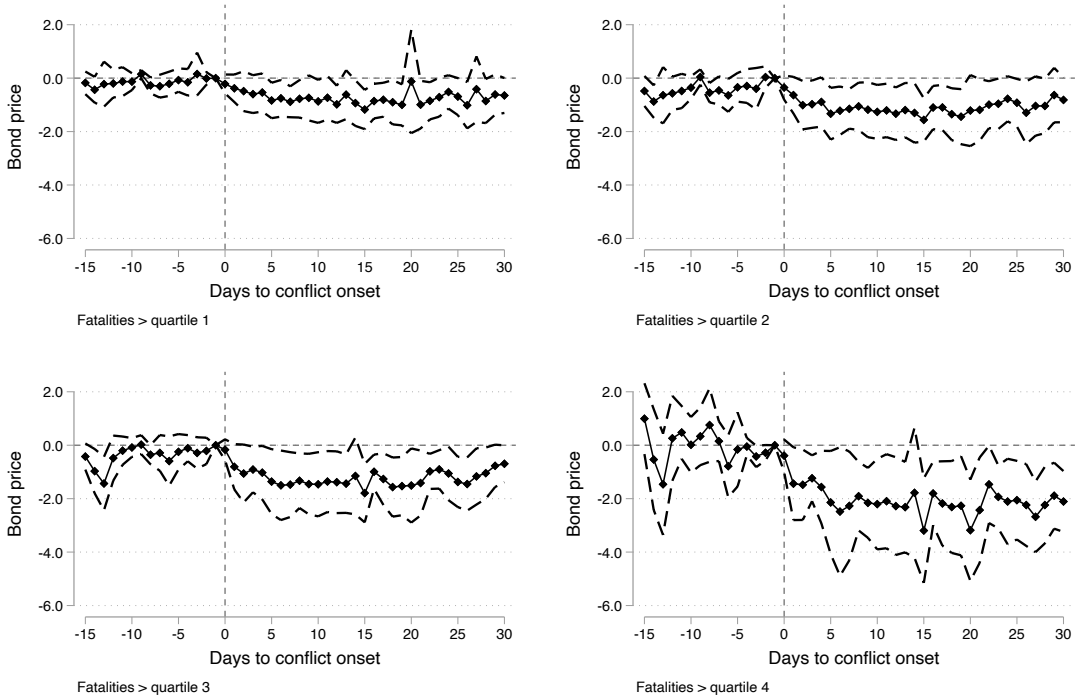
Note: Figure shows results from the simulation of bond prices in the full-information case. Top-left panel compares estimated $\hat{\tau}_k$ from Figure 2 to simulated effect, with overlaid local polynomial smoother. Top-right panel plots $\tau(\zeta_0)$ for various values of γ. Bottom-left panel traces out the belief frontier consistent with $\hat{\tau}_{15}$ under full information. Bottom-right panel plots $\tau(\zeta_0)$ for varying posterior beliefs $\tilde{\zeta}_0$.

Figure A7: Event-study: conflict history



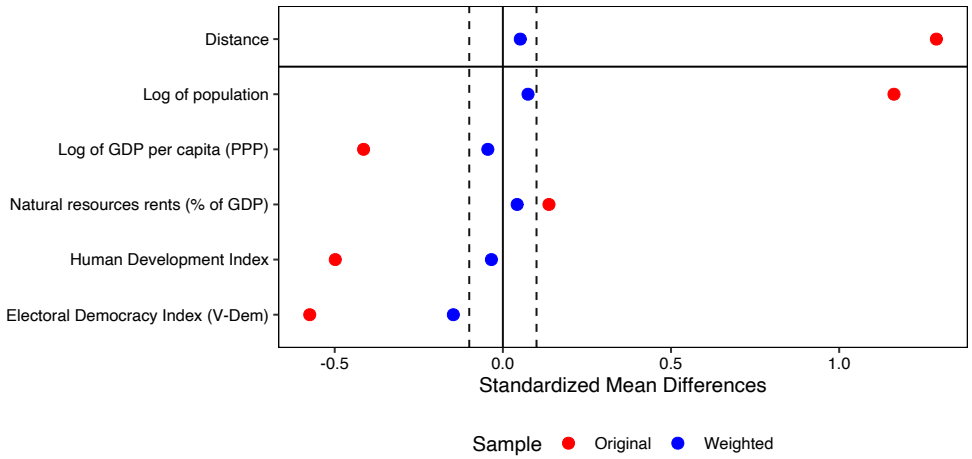
Note: Figure shows coefficients from stacked event-study regressions described in Section 3 on daily bond data for two different subsamples of treated countries, indicated in each subfigure footer. Major conflict is defined as a country experiencing conflict resulting in more than 1000 battle-related deaths in a given year in the five years before the onset date. Standard errors are clustered at the country level. Outcome is the daily bond trading price averaged across all available trading exchanges, indexed to 100 (par). Specifications include interacted event-specific two-way fixed effects as well as interacted bond maturity fixed effects.

Figure A8: Event-study: fatalities



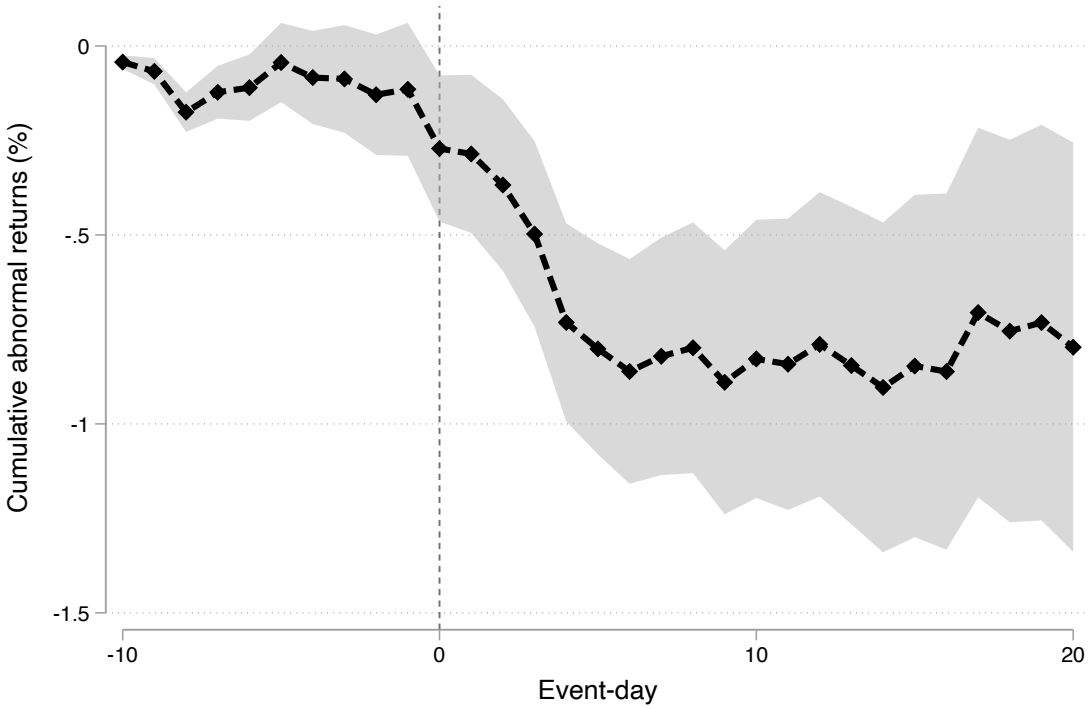
Note: Figure shows coefficients from stacked event-study regressions described in Section 3 on daily bond data for four fatality quartiles, indicated in each subfigure footer. Sample includes only events involving state forces. Standard errors are clustered at the country level. Outcome is the daily bond trading price averaged across all available trading exchanges, indexed to 100 (par). Specifications include interacted event-specific two-way fixed effects as well as interacted bond maturity fixed effects event-study estimates.

Figure A9: Sample balance



Note: Figure shows standardized mean differences between treated and control countries over a host of observable characteristics. The comparison is made for two different samples. Original sample refers to original observations used in main estimates. Weighted sample is the result of weighting the original sample with the propensity score obtained from a logistic regression of a dummy indicator of conflict on country characteristics as the logarithm of population, the logarithm of GDP per capita, natural resources rents as percentage of GDP, human development index, and an index of electoral democracy.

Figure A10: Cumulative abnormal returns to conflict-affected bonds

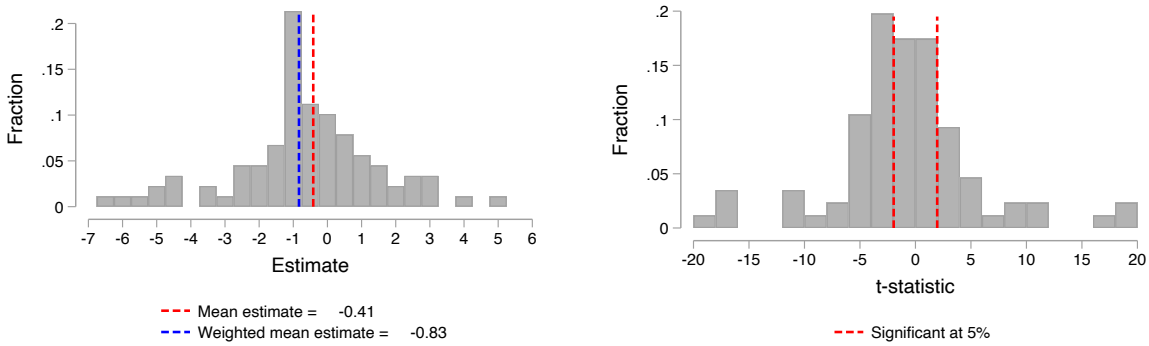


Note: Figure shows cumulative abnormal returns of conflict bonds for the event window $k \in [-10, 20]$. Sample is all state-involved conflict first events. 95% confidence intervals are formed using a clustered bootstrapping procedure with 500 replications and event-by-bond clustering. Daily abnormal returns are defined as the difference between the observed daily return for the conflict affected bond and the predicted return based on the correlation between conflict bonds and the non-affected average daily market return from the estimation window of $k \in [-30, -10)$. Cumulative abnormal returns sum the daily abnormal up to a given k.

Figure A11: Event-specific estimates

(a) Point estimate

(b) t-statistic

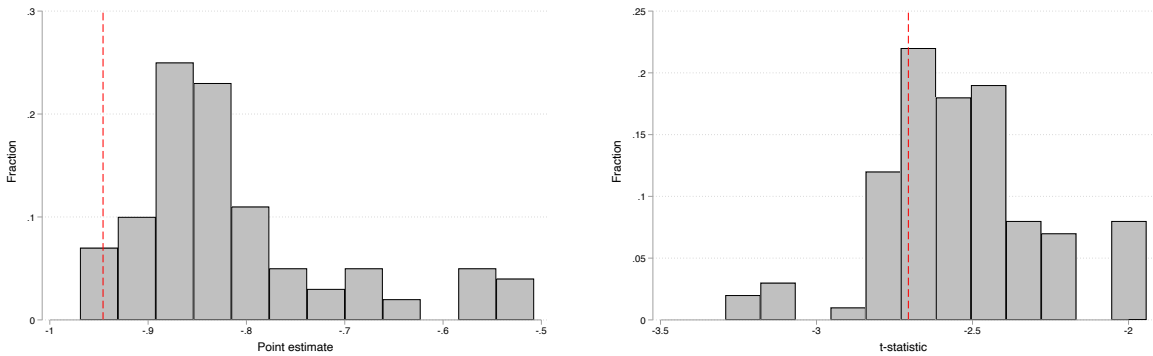


Note: Figure shows the distribution of event-specific estimates (a) and t-statistics (b) from 89 state-involved conflict events. Estimates are obtained from individual event study regressions for each event in the data. The mean estimate is the unweighted average of all point estimates, while the weighted mean is weighted by $1/s_e^2$ where s_e is the estimated standard error on the treatment effect for event e.

Figure A12: Robustness to event-windows

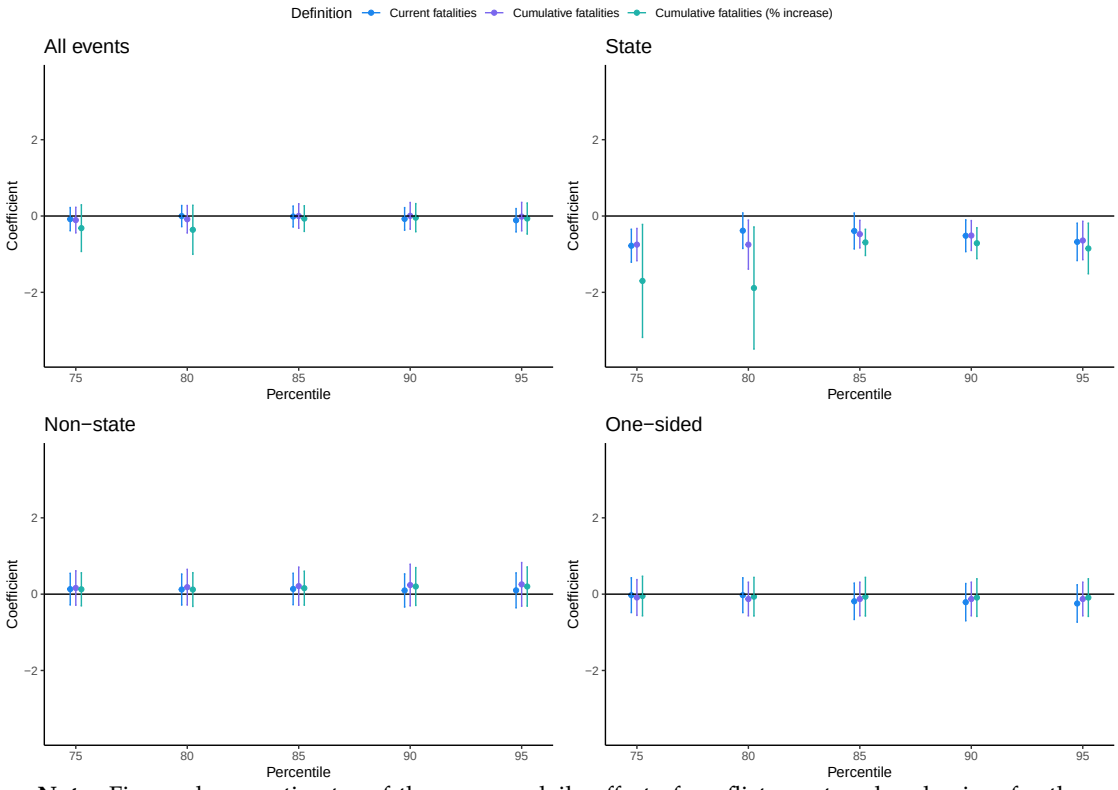
(a) β point estimate

(b) t-statistic



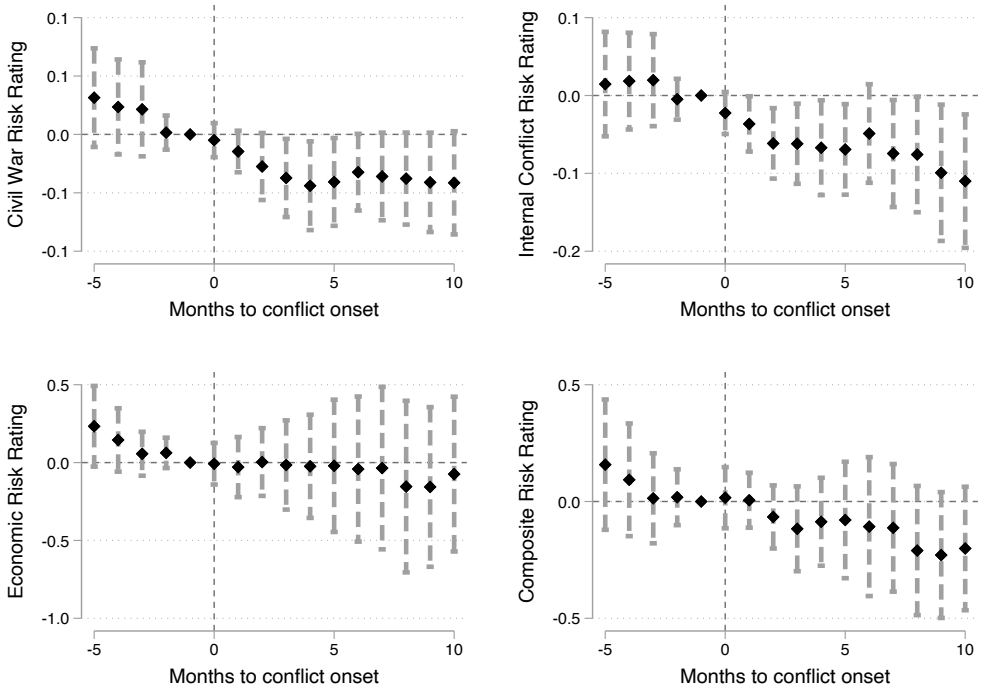
Note: Figure shows the distribution of estimates (a) and t-statistics (b) from all possible event-windows between contained in the +/-30 day interval. Sample is all state-involved conflicts. Regression specification is the stacked estimation in Table 1 column (2).

Figure A13: Robustness to event definition



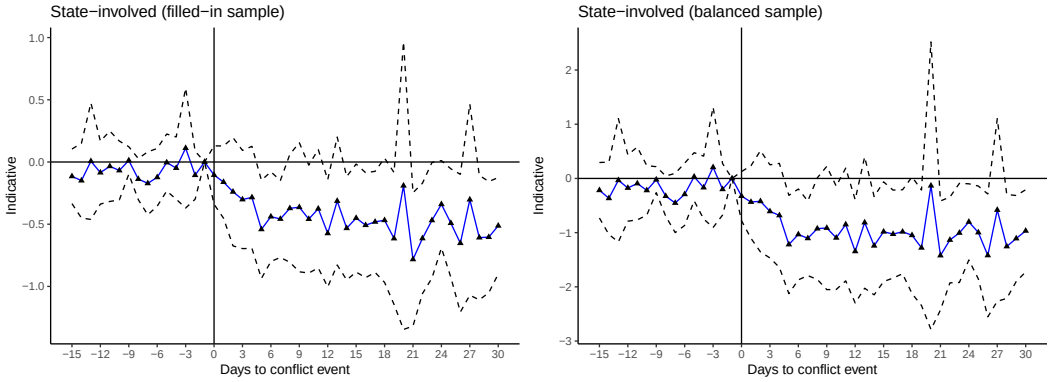
Note: Figure shows estimates of the average daily effect of conflict onset on bond prices for the four event subsamples indicated in the subfigure headers. Each subfigure tests $3 \times 5 = 15$ different definitions of subsequent conflict events, varying the definition of fatalities (3) and the percentile threshold (5). Current fatalities are those on the day-of the event, cumulative are those between event-date k and $k+1$, and cumulative (% increase) is the % change between cumulative fatalities before and after date k_e.

Figure A14: Correlation with ICRG risk measures



Note: Figure shows correlation between ICRG risk country-level risk measures and conflict onset dates from UCDP. Estimates are from stacked event-study models at the country-month level within each event. Sample is all conflicts. ICRG ratings are normalized with a lower rating indicating more risk. Standard errors clustered at the country level.

Figure A15: Event-study: filled-in and balanced samples



Note: Figure shows coefficients from stacked event-study regressions described in Section 3 on daily bond data for state-involved conflicts in two different samples, indicated in each subfigure title. Standard errors are clustered at the country level. Outcome is the daily bond trading price averaged across all available trading exchanges, indexed to 100 (par). Specifications include interacted event-specific two-way fixed effects as well as interacted bond maturity fixed effects.