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PRICING CONFLICT RISK: EVIDENCE FROM SOVEREIGN BONDS

Jonah M. Rexer, Ethan B. Kapstein and Andrés F. Rivera

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ABSTRACT

What do sovereign bond investors know about the risks and costs of violent conflict? Do they rationally incorporate available information, or are they overly-optimistic – or pessimistic – about the economic effects of political violence? To answer these questions, we estimate event-studies using daily sovereign bond trading prices and information on violent armed conflicts over the past two decades. We show that bond prices fall by an average of 0.7 points after the onset of state-involved conflict. Using reduced-form parameters, we calibrate a bond pricing model which implies both under-reaction and investor learning: the share of the shock that is priced in rises from 14% initially to 75% after 15 days. Consistent with the model, effects are larger where priors are optimistic because of recent peacefulness. Prices also respond more to severe outbreaks of violence near the capital city, and to center-seeking conflicts where rebels threaten the state. The magnitudes of these heterogeneous responses imply accurate beliefs about the process of conflict damages. The results suggest that bondholders have a nuanced understanding of the underlying political and spatial dimensions of conflict.

Keywords: sovereign debt, bonds, conflict, behavioral finance

JEL Classifications: C63, G12, P34, H63, H87

Contact information for author(s)
Jonah M. Rexer, Postdoctoral Associate, Empirical Studies of Conflict Project, Princeton University: jrexer@princeton.edu
Ethan B. Kapstein, Co-director, Empirical Studies of Conflict Project, Princeton University: kapstein@princeton.edu
Andrés F. Rivera, Assistant Professor, Universidad Javeriana-Cali: andres.rivera@javerianacali.edu.co

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1 Introduction

Armed conflict is costly for societies. It disrupts productive activities (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] In some cases, 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 the month following Russia's February 2022 invasion of Ukraine, for example, the price of Ukrainian bonds plummeted to 22 cents on the dollar, at which point some argued that the debt was underpriced.[2] What determines bond market responses to conflict, and do investors efficiently price available conflict-specific information? This article combines daily sovereign bond prices with information on armed conflict over the past two decades to answer these questions.

Despite the salience of conflict risk, bond market reactions to outbreaks of political violence are little studied, notwithstanding a voluminous literature on other forms of sovereign risk. Existing evidence linking financial market behavior to war and peace primarily comprises a handful of individual country case studies.[3] Moreover, the magnitude of investor responses also matters from a policy perspective, because secondary market yields determine future financing costs. Ukraine must now finance its war against Russia through bond sales at elevated interest rates even as its economy contracts from wartime damage.

Financial markets may be particularly prone to mispricing violent conflict, where information is often unreliable and uncertainty is high. Limited or inaccurate information, the "fog of war," implies that individuals update from priors as signals are sent over time, and so may not immediately internalize the cost of a conflict event. But biased beliefs can also cause mispricing. Investors may believe that foreign conflict is more prevalent than it really is because of home bias (Strong and Xu, 2003), or availability bias (Schraeder, 2016), leading to weaker reactions than are warranted by the circumstances on the ground. Biased beliefs

[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] "Now is the right time to start buying," The Wall Street Journal reported a trader as saying in March 2022.

[3] See e.g. Guidolin and La Ferrara (2007); Chaney (2008); Arin et al. (2008) and Castañeda and Vargas (2012).

about costs can also drive underreaction. Over-optimism has, throughout history, led markets to underestimate the costs of war, leading to initial under-reactions, infamously in the early weeks of the First World War.[4] Conversely, if investors mistakenly believe violence is highly unlikely,[5] they will exhibit excessive surprise when it *does* occur. At the same time, undue pessimism about the cost of conflict may also lead markets to overreact when violence eventually occurs.

We begin by combining daily price data on 1731 international sovereign bond issues with media-based event data on the onset of 262 armed conflicts from 2004-2020. Using a bond-day panel, we estimate the dynamic effects of conflict onset on bond prices and yields using event-study regressions. We define "conflict episode onset" as the first violent incident observed between a unique actor pair in a given country, or a subsequent violent incident in that country-actor pair after a prolonged period of peace. To estimate the time-varying parameters, we use a "stacked" event study estimator, which allows us to restrict the control group to never-treated countries (so-called "clean controls"). This mitigates the bias of two-way fixed effects estimators in staggered adoption difference-in-differences settings with heterogeneous effects (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). 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.

Our estimates reveal that in the aggregate, conflict has a small and statistically insignificant effect on bond prices. However, disaggregating events by conflict type reveals important heterogeneity in price movements. Bond prices fall by a daily average of 0.7 points (relative to par) after the onset of *state-involved* conflicts. This point estimate rises to 1.2 points 15+ days after a conflict event, 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 market responses to conflict. Investor sentiment does not naively react to bad news; rather, markets only respond in cases where the sovereign's creditworthiness is likely to be affected. We furthe show empirically that this

[4] Ahamed (2009) writes of this period: "as the financiers of Europe watched their continent limp towards Armageddon...they clung to the illusion that global commerce would be disrupted only briefly." Data on bond prices from WW1 in Chadefaux (2017) support this interpretation.

[5] For e.g., because of biased information (Golez and Karapandza, 2022)

pattern is matched by an underlying increase in credit risk; only the onset of state-involved conflict triggers a large, long-run increase in the monthly probability of debt restructuring.

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. 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, resource dependence, and 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, and sample restrictions.

To calibrate the magnitude of market responses, we build a simple two-state, risk-neutral, fixed-income bond pricing model in which investors learn about the likelihood of violent conflict. Conflict enters the value function as a constant haircut to fixed bond payments in years of active conflict, equal to the average annual effect of violent conflict on national income.[6] Conflict follows an AR(1) process, so that observing the current state allows investors to forecast future probabilities of conflict. We assume that investors know the cost of conflict and the parameters of the process, but have uncertainty over the current state. After observing a conflict event, investors update their beliefs toward the conflict state.

We estimate the parameters of the conflict process using reduced-form regressions and simulate counterfactual price changes after observing a conflict event. To benchmark the market reaction, we compare the estimated price effect with that predicted under full information and empirically correct beliefs. The model shows evidence of both initial underreaction and rapid investor learning: the share of the shock that is priced in rises from only 14% initially to roughly 76% after only 15 days. The speed of learning suggests that the initial market underreaction is caused by incomplete initial updating due to information frictions, which ameliorates rapidly as more information becomes available, rather than by biased beliefs.[7] The persistent under-pricing even after bond prices stabilize is consistent with a 1.3 percentage-point under-estimation of the economic cost of conflict.

[6]Our setup recalls Barberis et al. (1998) and follows the treatment of restructuring risk in Asonuma et al. (2017).
[7]However, our model can't separately identify biased beliefs from incomplete information.

The model also shows that conflict shocks must be sufficiently surprising and economically costly to move markets. We find that effects are significantly larger where priors are optimistic because the country-level annual 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 – an additional 7-11 basis points in credit spread – 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 up-weight recent, deadlier conflict in forming priors, consistent with adaptive belief formation (Haruvy et al., 2007).

Prices also respond to the expected costs of conflict. Bond price effects are largest for more severe outbreaks of violence, with the daily average treatment effect rising to 2.4 points for conflicts in the top quintile of fatalities.[8] Furthermore, investors respond more to outbreaks of violence near the capital city, where risks of regime collapse are heightened. We find that conflict onset in the capital provokes 1.95-point sell off in affected bonds, nearly four times larger than the effect for events outside of the capital. We also find that investors are aware of the underlying political divisions driving conflict, responding primarily to center-seeking conflicts that pose a threat to political stability, rather than separatist or spillover wars. Lastly, we show that each of these conflict characteristics is indeed an important independent predictor of conflict cost, and that investors' heterogeneous responses are quantitatively close to the model-predicted efficient pricing, with only mild evidence of correlation neglect (Enke and Zimmermann, 2017). We conclude that investors effectively use historical data and initial information to form accurate conflict-specific priors and cost expectations.

We contribute to several strands of the literature in finance, political economy, and economics. First, we add to the literature on behavioral finance, learning, and asset pricing. In particular, a large literature focuses on explaining asset price anomalies relative to efficient benchmarks either with uncertainty and learning processes (reviewed in Pastor and Veronesi (2009)) or by appealing to biased beliefs, behavioral biases, and non-Bayesian learning processes (see Barberis and Thaler (2003) for a somewhat dated review). We extend this work by testing market efficiency in the case of violent conflict, where both information frictions

[8]Note that we define fatalities as those occurring on the conflict onset date, to capture the information available to investors at the time of the onset.

and behavioral biases are likely to be pronounced. Despite some under-reaction, we find evidence of substantial learning, extensive conflict-specific market knowledge, and only mild over-optimism by financial markets.

A set of closely related recent studies make use of similar event-study techniques to 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). While these case-studies have the benefit of detail, they are under-theorized and lack external validity. Our large sample approach allows us to both obtain an externally valid estimate of market response and leverage conflict-level heterogeneity to test hypotheses about investor beliefs. In doing so, we contribute to a large literature in both finance and economics on political risk in emerging markets.

Several related papers study the impacts of conflict on financial markets in a larger sample. Guidolin and La Ferrara (2010) find *positive* effects of conflict on national stock markets, though the mechanism for this counter-intuitive result is unclear. The most closely related paper is Chadefaux (2017), which studies bond market reactions to historical episodes of war, finding yield spikes after onset. However, the study lacks a strong empirical design, using only country fixed effects and lagged dependent variables. Our work, in contrast, uses a high-dimensional set of bond-type, country, and time fixed effects in an event-study framework to credibly identify causal effects. More importantly, Chadefaux (2017) interprets negative market reactions to conflict onset as evidence that markets underestimate conflict risk. We show that prior beliefs, conflict dynamics, and the cost of conflict all must be specified to make inferences about the direction and degree of mispricing. Our results demonstrate that a negative effect is actually consistent with under-reaction. Our heterogeneous results further suggest that investors form reasonable, data-driven expectations of conflict likelihood and cost.[9] Like Jha et al. (2022), we emphasize the centrality of investor beliefs in understanding the extent and causes of conflict-related mispricing.

[9]Our work extends research which has examined the effects of terrorism on such economic variables as GDP, interest rates, and investment (Moody's Investor Services, 2013).

2 Context and data

2.1 Conflict data

To identify the onset date of armed conflict "episodes" that are likely to be relevant to bond markets, we use Version 21.1 of the UCDP Georeferenced Event Dataset (GED), [10] which contains information on 261,864 armed conflict events from 1989-2021. The UCDP-GED data uses local and international media sources to identify, geolocate, and describe unique incidents of violence that are part of larger armed conflicts. We define three separate concepts. An *event* is an individual incident of violence between two actors (a dyad) in a given geo-tagged location, while a *conflict* is the collection of all such events between the actor-pair in a given country.[11] Finally, an *episode* is a subset of events within a conflict, with a start and end date, initiated by an *onset event*. The dates of these onset events combined with country of occurrence determine the timing and location of treatment exposure to conflict.

Our data contain two types of episodes. *First episodes* of a given conflict are dated from first time an country-actor-pair enters the event-level data. *Subsequent episodes*, instead, are those that occur after a period of relative peacefulness and cause significant damage, representing a revival of a given conflict. Two conditions must be met in order for conflict event e to be classified as the onset event of a subsequent episode:

1. The number of peaceful days between e and $e - 1$ is greater than the 95th percentile of its event-day-level empirical distribution within an specific armed conflict.

2. The number of fatalities that occur during conflict event e is greater than the 95th percentile of its event-day-level empirical distribution within an specific armed conflict.

Note that some conflicts in our data may only have subsequent episodes, because we do not have bond trading observations for the treated country around the time of the first onset event. Note also that the percentile cutoffs for subsequent events are from within-conflict distributions, so that differences across conflicts in deadliness or periodicity, which should be

[10] https://ucdp.uu.se/downloads/index.html#ged_global

[11] According to our definition, 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.

priced in by markets, do not affect the selection threshold.[12]

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

2.2 Economic and financial data

We use global bond market data from Cbonds, a bond trading platform. We leverage daily bond price quotes on 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. Our primary outcome is the daily bond price (relative to par=100), though we also estimate models with yields as the dependent variable.

We complement these data with a large set of country characteristics constructed from several different sources. We include macroeconomic fundamentals such as GDP growth, GDP per capita, population, foreign investment, external balance and natural resources dependence 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. Moreover, we obtain daily data on commodity prices and global equity and bond market indices from the World Bank and the Federal Reserve Bank of St. Louis (FRED).

We generate our final estimation sample by merging Cbonds, UCDP event dates, and country characteristics. We construct event-specific datasets as follows: for each episode, we identify the treated country and onset event date. We then obtain all bond-day price observations for the conflict-affected country within a 61 day estimation window ($t = -30$ to $t = 30$),

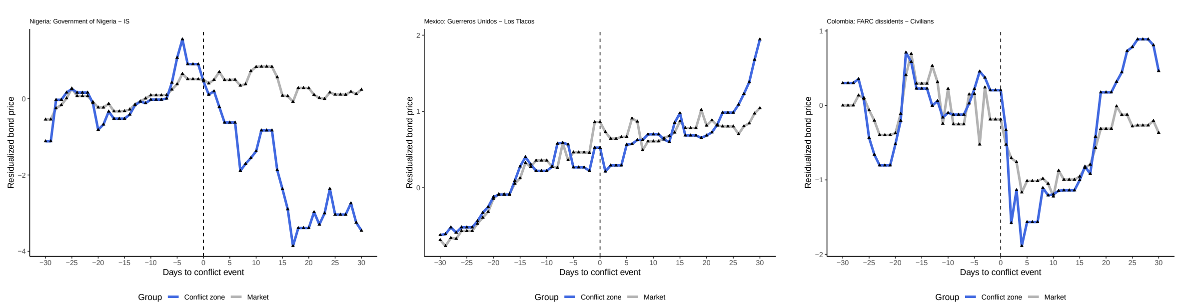
[12] In the appendix, we consider the robustness of the results to three arbitrary choices: i) definition of fatalities, ii) the percentile threshold, and iii) inclusion of conflicts with subsequent episodes but no initial episode.

as well as our time-invariant bond and country-level characteristics. To this we append the set of "clean" control observations within the estimation window; these are bonds from 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 episodes. The final stacked dataset (at the episode-bond-day-level) for 2004-2020 consists of 1731 unique bonds (667 ever-treated and 1064 control) and 1,082,119 bond-days from 120 countries (44 ever-treated, 76 control). The treated bonds are affected by 313 conflict episodes connected to 262 armed conflicts (country-dyad).[13]

2.3 Trading prices trends

We begin by reporting on the evolution of bond prices for a selection of armed conflict episodes.[14] We select these episodes as case-study illustrations of systematic empirical patterns that we demonstrate in Sections 4 and 6. For each armed conflict episode, we first calculate average bond prices for treatment (conflict zone) and control (market) groups along the 61-day estimation period. In order to normalize the scales of the treatment and control price series', the average prices are then residualized by the pre-event mean price difference.

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



(a) State-involved (b) Non-state (c) One-sided

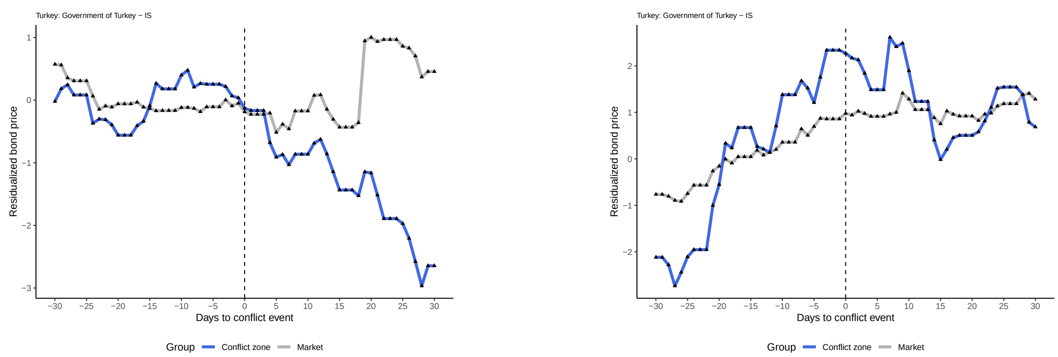
We examine the trends in bond prices for state-involved, non-state, and one-sided conflict episodes in Figure 1. We hypothesize that markets are likely to respond to conflicts involving

[13] Within each episode dataset, the composition of treated and control observations may change from day to day since there are gaps in some price series, creating an unbalanced panel. We test robustness to holding sample composition fixed by restricting the sample to fully only bonds fully balanced within an event window.

[14] Each conflict is indicated by the name of the country in which it occurs and the names of the two armed actors that participate in an conflict event.

government forces, which strain government budgets, threaten political instability, and may destroy critical infrastructure. 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 Northern Nigeria has been a source of significant territorial loss and military investment for the Nigerian 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 viability of 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.[15] 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.

Figure 2: Conflict events and bond prices



(a) Conflict onset

(b) Subsequent episode

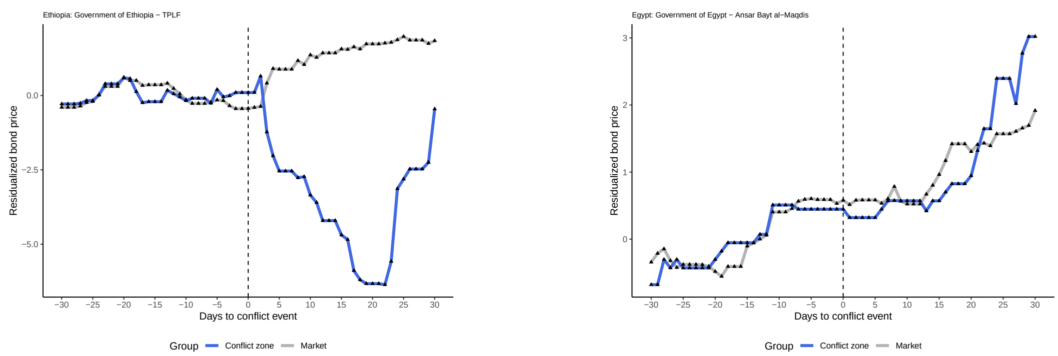
Next, we argue that investors learn about the probability of conflict from past experience, so that new onsets should be more surprising than recurring episodes. We examine this hypothesis in Figure 2 in the context of the conflict between the Turkish government and IS. Beginning in 2015, IS began attacks on both civilian and government targets within Turkey, representing a substantial increase in conflict risk for Turkey. 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 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 pro-

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

voke larger market responses. Figure 3 contrasts 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.

Figure 3: State-involved conflict onset and bond prices



(a) Most violent event

(b) Least violent event

This descriptive analysis provides insights into a possible systematic relationship between conflict onset and sovereign bond prices. Financial markets tend to react unfavorably following a state-involved conflict event. Furthermore, these declines in bond prices are stronger for initial outbreaks of violence and more severe conflicts. However, these case studies may not be externally valid, and there is limited event-level heterogeneity to test hypothesis about investor behavior. We therefore turn to our event-study empirical strategy.

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 episode 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 episode (stack) e, for $t \in [k_e - 30, k_e + 30]$:

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

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 episode.[16] τ_k are coefficients for leads and lags of the onset date, estimating the differential dynamic path of treated bond prices relative to controls, 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.[17]

The stacked specification can be thought of as a standard two-way fixed effects event-study regression, estimated separately for each episode. The resulting aggregate τ_k coefficients then represent a variance-weighted average of the episode-wise dynamic effects. To fully saturated model requires event-specific effects for unit and time, given by δ_{be} and δ_{te}. Our main specifications also include time-invariant bond-level characteristics X_b – in particular, annual maturity fixed effects – interacted with γ_{te}.

Note that bond-day pairs may repeat in our stacked dataset, since the same country may serve as controls in multiple different concurrent episodes. Similarly, one country may be treated multiply by different new that overlap chronologically. As such, standard errors must be adjusted to account for serial correlation induced both by the underlying panel DGP and mechanically by the stacked data structure. In this case, country-level clustering is appropriate, allowing for unrestricted covariance in errors across all bonds, time periods, and episodes within a given country, which is the unit of treatment assignment (Abadie et al., 2022).We also

[16] Note that if $Treat_{ce} = 1$, country c may also be treated across multiple other events e', but there is no other event such that $Treat_{ce'} = 0$, since all controls are never-treated.

[17] 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.

run specifications collapsing the event-time effects into a single post-event indicator to obtain the average daily effect over the event window. Finally, we test various hypotheses about investor responses to conflict by introducing event-level heterogeneity into our main specification. We do this by conditioning on event 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 interaction with the δ_{te} fixed effects.

3.2 Identification

Our set-up is an event-study (dynamic difference-in-differences) estimated with a stacked approach. The first key identifying assumption is that of 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 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 per se, but also a test for pre-period information shocks.

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 including indices of global bond, equity, and commodity market conditions – the S&P, VIX, EMBI, and commodity indices – interacted with the treatment indicator. In addition, we allow for differential trends in outcomes by interacting year-by-event fixed effects with a host of country-level control variables, including macroeconomic conditions, institutional characteristics, and country risk ratings.

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. 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.[18,19]

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. We also focus on a relatively narrow event window in order to minimize exposure to other confounding shocks. Our stacked specification therefore purges the standard TWFE estimator of biased comparisons, also called "negative weights" (de Chaisemartin and D'Haultfoeuille, 2020).

4 Main results

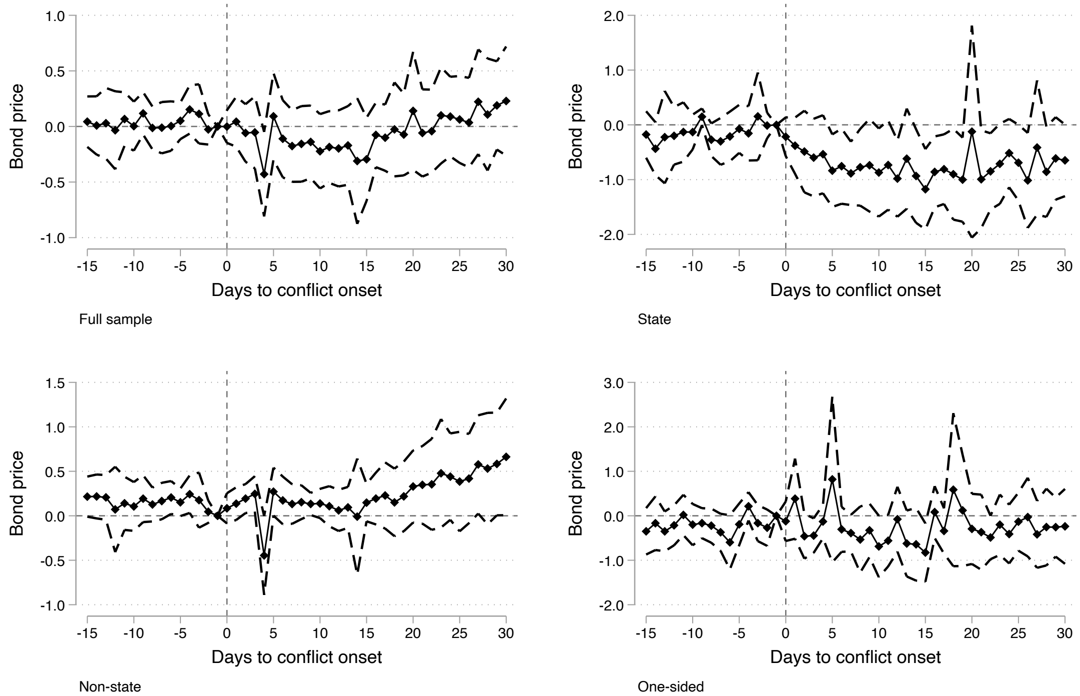
We begin by considering the aggregate impact of conflict on bond prices. Because we expect substantial heterogeneity in price responses, we also estimate equation 1 for three mutually exclusive subsamples of armed conflict events: state-involved, non-state conflicts, and violence against civilians. Figure 4 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.

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.

[18]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.

[19]For a comprehensive treatment of our estimator, also known as the "local projections" approach, see Dube et al. (2022).

Figure 4: 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.

The remaining panels of Figure 4 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-points lower trading price after just 15 days, which persists for the remainder of the estimation window. 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.[20]

Table 1 provides the corresponding daily average estimates for these four specifications.

[20] 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 4.

All models include interacted fixed effects for bond maturity. 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 $\times$ Treated	-0.096	-0.701***	0.076	0.138
	(0.166)	(0.224)	(0.230)	(0.302)
Bond $\times$ event FE	Yes	Yes	Yes	Yes
Day $\times$ event $\times$ 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
R^2	0.981	0.978	0.982	0.982

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 A1 may be too small for standard cluster-robust inference (Cameron et al., 2008). To address this, we use wild cluster bootstrapping inference in Table A2. 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 directly payoff-relevant, we restrict the sample of one-sided conflict events to those involving only state actors and civilians in Table A3; the average effect is negative but small in magnitude and statistically insignificant. Lastly, Table A4 shows that conflict-induced capital flight is more pronounced for countries with higher credit risk, as measured by the most recent sovereign credit rating.

Tests of pre-trends such as those in Figure 4 may be underpowered, so that large violations of parallel trends may not be detected as significant pre-trends (Roth, 2022). In Figure A2, 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[21]. We also conduct non-inferiority tests that are able to reject the null of pre-trends for all violations greater than 0.4 (Dette and Schumann, 2020). We discuss more extensive robustness tests in Section 7.

We interpret the reduction in bond prices after state conflict onset as reflecting investors' perceptions of an increase in underlying credit risk. But does conflict actually affect government capacity to meet financial obligations? Plausibly, state-involved conflict both destroys output and thus tax revenue, and also increases government expenditures, hampering timely repayment of debt. Asonuma et al. (2017) defines weakly preemptive restructurings as missed debt payments following negotiations with creditors. We test whether such restructurings rise after conflict onset.

We estimate equation 1 using monthly data on preemptive restructurings from Asonuma et al. (2017). In this case, the outcome variable is a weak restructuring dummy, and the stacked model includes country-event and month-event fixed effects. Appendix Figure A3 plots the dynamic event-study coefficients. In the full sample, conflict onset makes a weak restructuring significantly more likely. But state-involved conflict drives this effect. 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 of weak restructurings across our sample (0.005). Conflicts involving state forces substantially reduce government creditworthiness.[22]

5 Quantitative framework

Do market prices reflect accurate responses to the information revealed by conflict events? In this section, we build a simple quantitative framework to answer this question. We then es-

[21]It is also worth nothing that these linear trends are a poor fit for the pattern of event-study coefficients, suggesting that differential linear trends are unlikely to be driving the results.

[22]We also tested the effect on both defaults and weak restructurings. The results are similar but somewhat weaker, since the main effect is only driven by weak restructurings and not defaults.

timate 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 and learning. 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.

5.1 Model

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

$$EV_0 = \sum_{t=0}^{T} \frac{C_t}{(1+r)^t}(1-\zeta_t\gamma) + \frac{F}{(1+r)^T}(1-\zeta_T\gamma) \tag{2}$$

Where r is the market 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 shock to the cash flow 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 γ.[23,24] 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 conflict will follow an AR(1) process going forward

$$z_t = \alpha + \rho z_{t-1} + \epsilon_t \tag{3}$$

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

[23]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.

[24]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.

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 (3) beginning at z_0.[25] The ex-ante expected value can be written as an explicit function of the $t = 0$ prior belief on conflict probability ζ_0

$$EV_0 = \zeta_0 EV_0(1) + (1-\zeta_0)EV_0(0) \tag{4}$$

After observing the realization $z_0 = 1$ but before subsequent periods, market participants may update their priors from ζ_0 to $\tilde{\zeta}_0$.[26] Then the ex-post expected value, $\widetilde{EV}_0$ will be as in (4), 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:

$$\tau = \widetilde{EV}_0 - EV_0 = (\tilde{\zeta}_0 - \zeta_0)[EV_0(1) - EV_0(0)] \tag{5}$$

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 (5). 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 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.

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

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

5.2 Estimation and simulation

To simulate the efficient 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 (log) income follows a two-way fixed-effects process, which varies around its long-run level in response to conflict shocks. For country i in year t, we have:

$$\log(y_{it}) = \alpha_0 + \gamma z_{it} + \delta_i + \delta_t + u_{it} \tag{6}$$

The results of the estimation of this equation are in Appendix Table A5, 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 $\hat{\gamma}$ roughly equal to 0.058, or a 5.8% average annual loss in output for each year that a conflict is active.[27] Our estimates are similar in magnitude to those in Novta and Pugacheva (2020). Next, we estimate the AR(1) parameters from equation (3) using OLS on the same country-year panel.[28] The results are in Table A5, column (5). We estimate $\hat{\rho} = 0.8$ and $\hat{\alpha} = 0.028$.

Lastly, we take the remaining parameter values from the data. We focus on ten-year maturities, $T = 10$, as this is the median maturity in our sample. We set $r = 0.029$, the average 10-year T-bill rate over the sample period (2004-2020) 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 Appendix Table A6.

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. Appendix Figure A4 shows the path of beliefs on conflict probability after the state is revealed at $t = 0$. If the state is revealed to be $z_0 = 1$, 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, it converges to the to the accurate prior in

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

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

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 likelihood conflict 20 years hence. Using the sequence of ζ_t in Figure A4, 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 $\tau(\zeta_0)$.

5.3 Results

5.3.1 Efficient 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 $\tilde{\zeta}_0 = 1$. We compare the simulated τ under these true parameter values with our time-varying post-event coefficients $\hat{\tau}_k$ to determine whether the market has accurately priced conflict.

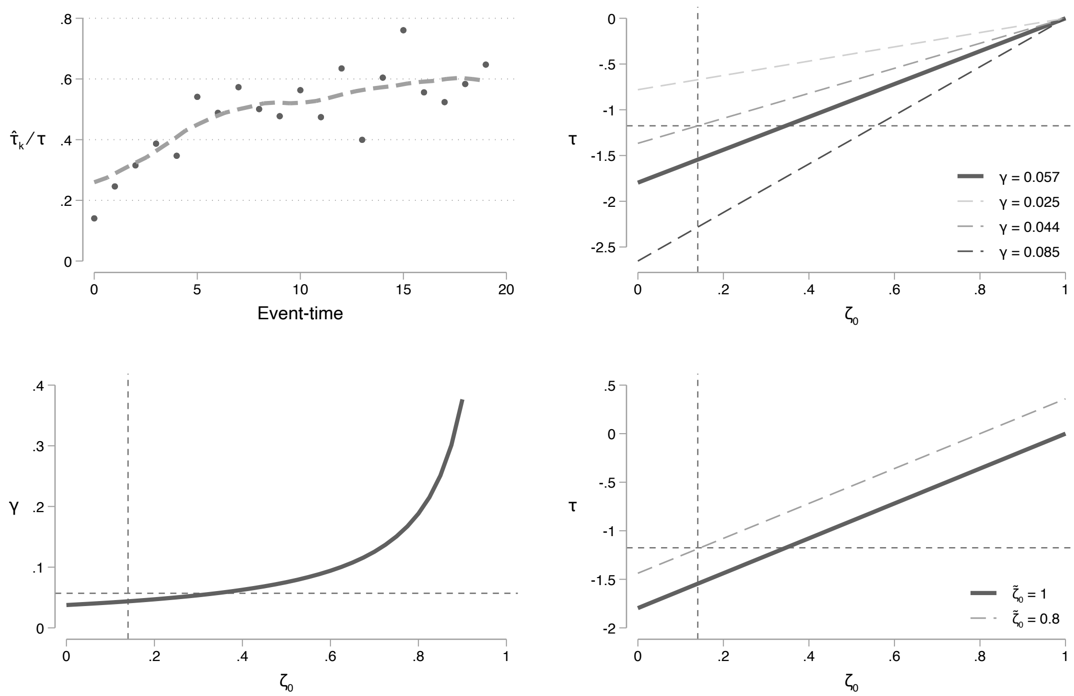
The results are in Figure 5, top-left panel. We plot each $\frac{\hat{\tau}_k}{\tau}$ as well as overlay a local polynomial fit to smooth day-to-day noise in the coefficients. We estimate that in the perfect information case with accurate beliefs, investors' long-run downward revision on the value of 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. Thereafter, the coefficients begin to fall slightly again.[29] The evidence suggests that while investors learn rapidly about conflict probability, they also underreact substantially even at the largest post-event effect. Nonetheless, the confidence intervals in Figure 4 make it clear that we cannot rule out market efficiency for many of the estimated τ_k.

5.3.2 Implied biases

In our simple model, 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.

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

Figure 5: 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 4 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 $\tilde{\zeta}_0$.

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 5 plots $\tau(\zeta_0)$ 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.[30]

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. We estimate a prior belief on the probability of conflict of 34.6%, much larger than the empirical likelihood of 14%.

Second, investors may be misinformed about the costs of conflict, underestimating γ. If

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

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 (5) 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 5, top-right. Assuming full information and accurate priors about conflict probability, the observed treatment effect implies a 1.38% underestimation 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, it seems reasonable that market underreaction 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 4 suggest substantial updating up to day 15, and leveling off thereafter. However, investor posteriors may not actually converge to 1, if for example, there is no new information after this time. Allowing for incomplete information shifts $\tau(\zeta_0)$ upward, affecting the intercept without changing the slope, as in Figure 5, bottom-right panel. Assuming unbiased priors, the posterior belief that rationalizes the treatment effect on day 15 is roughly 0.8, implying that incomplete updating can also explain the results.

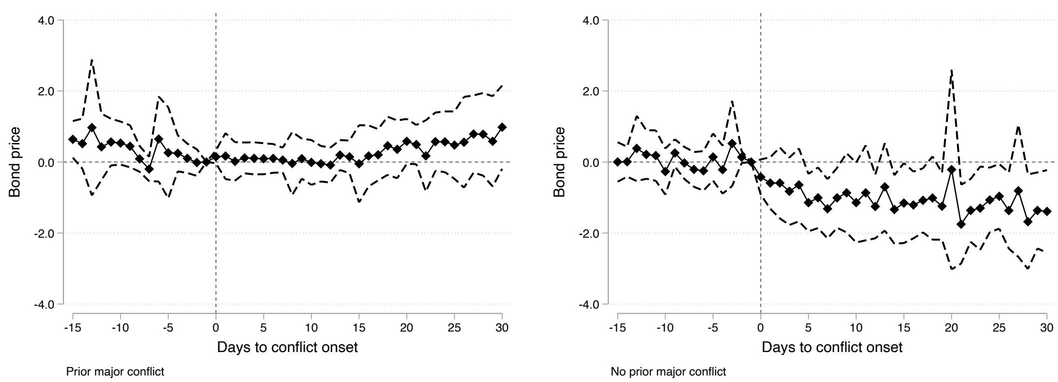
6 Heterogeneity

Our pricing model predicts heterogeneity in responses to conflict onset. First, conflict onset resolves uncertainty about the likelihood of violence. The size of the price effect therefore depends on the location of the prior. In particular, only surprising conflicts should provoke bondholder selloffs. Second, conflict onset provides the market information about the expected costs of conflict, and costlier conflicts should provoke larger sell-offs. Conflict-level heterogeneity in investor responses by conflict severity allow us to infer the initial information investors use to form expectations of cost. Going forward, we maintain only the sample of state-involved conflict events, since non-state conflicts do not provoke market responses.

6.1 Beliefs about conflict risk

We use data on conflict history to capture investors country-specific prior beliefs on conflict probability. As a first pass, we split the data by initial and subsequent conflict episodes. The initial episode of a conflict may encounter investors with less informed priors, provoking a larger price response and updating pessimistically. Future episodes of that conflict should encounter more pessimistic priors, and are priced in. Table A7 splits estimates the state conflict effect in each event subsample and finds strong evidence for these patterns. Initial episodes produce a negative price response by investors, significant at 1%, causing bonds lose 0.94% of their value relative to par on average daily. Subsequent events do not elicit the same investor behavior, with point estimates near zero.

Figure 6: 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.

More systematically, we estimate 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 has experienced state-involved conflict. Figure 6 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 large, negative, and significant for surprising events in countries without recent conflict history, but absent for countries with a history of violence. Holding a bond for 30 days following a surprising conflict event results in a 1.4% loss in value, relative to par.

Table 2 estimates these effects in the linear difference-in-differences setup, using continuous variables of prior conflict probability. We further disaggregate country conflict history into major and minor conflicts, using the fatality thresholds from UCDP.[31] 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
R^2	0.980	0.980	0.980	0.980	0.980	0.980	0.980	0.980

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

[31] Minor conflict is defined as 25-999 battle-related deaths in a given year, while major conflicts have least 1,000 battle-related deaths.

6.2 What do investors know 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 accurate 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 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
R^2	0.978	0.978	0.981	0.980	0.981	0.975

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. However, these heterogeneous effects may not be linear in conflict severity. 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. For example, the linear predicted effect of the average in the 5th quintile is -1.70, while the observed effect in column (6) is 2.46.[32] Figure A5 shows the corresponding event-studies; pre-trends remain broadly parallel for all subgroups, but higher-fatality events exhibit a much sharper initial drop in bond prices.

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
R^2	0.978	0.978	0.978	0.978
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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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%.[33] 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

[32]However, the small numbers of events and treated clusters for individual quintiles imply that these results should be treated somewhat speculatively.

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

further away converge rapidly to the full sample estimate.[34] 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.[35]

Finally, we consider whether investors understand and respond to the underlying political dynamics of conflict. We classify our sample of state conflicts 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).[36]

Table 5: Conflict onset and bond prices: conflict type

Dependent variable	Bond price				
Event-type	Spillover	Own			All
Conflict-type	All	All	Center	Regional	All
	(1)	(2)	(3)	(4)	(5)
Post $\times$ Treated	-0.261	-0.880***	-1.967**	-0.534**	-1.967**
	(0.246)	(0.260)	(0.858)	(0.245)	(0.857)
Post $\times$ Treated $\times$ Regional					1.433
					(0.889)
Post $\times$ Treated $\times$ Spillover					1.706**
					(0.858)
Bond $\times$ event FE	Yes	Yes	Yes	Yes	Yes
Day $\times$ event $\times$ maturity FE	Yes	Yes	Yes	Yes	Yes
Observations	375979	906166	239940	666226	1282145
R^2	0.977	0.979	0.976	0.980	0.978

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. Column (1) shows that average daily effects for spillover conflicts are negative but 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. Columns (2)

[34] Table A8 shows that interaction effects using the log of distance, while positive, are not significant. This suggests that the distance gradient of heterogeneous effects is concentrated only in areas very near the capital.

[35] This pattern could be driven by media bias if more information is available about events very near the capital.

[36] 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.

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 by center-seeking and regional conflicts, respectively. The results suggest that 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. Regional conflicts, while economically costly, may not pose the same risks of state collapse, and therefore see much smaller, though still significant, investor responses, at 0.53.

6.3 Are cost beliefs accurate?

Are these conflict characteristics actually predictive of conflict cost, and are the implied investor beliefs accurate? Allow linear heterogeneity in the conflict cost parameter from (6):

$$\gamma_{it} = \gamma_0 + \gamma_1 f_{it} + \gamma_2 c_{it} + \gamma_3 d_{it}^{50} \tag{7}$$

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 assess the accuracy of investor cost beliefs as follows. First we plug (7) into (6) and estimate the interacted conflict cost model. We then plug the estimated γ_j coefficients into the pricing model to predict benchmark efficient differential responses along each dimension of conflict heterogeneity. Lastly, we estimate our bond event-study with the same interactions and compare these observed responses with the predicted heterogeneous τ. However, we restrict the post-event sample here to $k \geq 15$ to capture only the long-run bond market effect.

Table 6 presents reduced-form 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, most substantially for center-seeking indicator, which

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
R^2	0.992	0.992	0.992	0.992
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
R^2	0.977	0.977	0.977	0.977

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

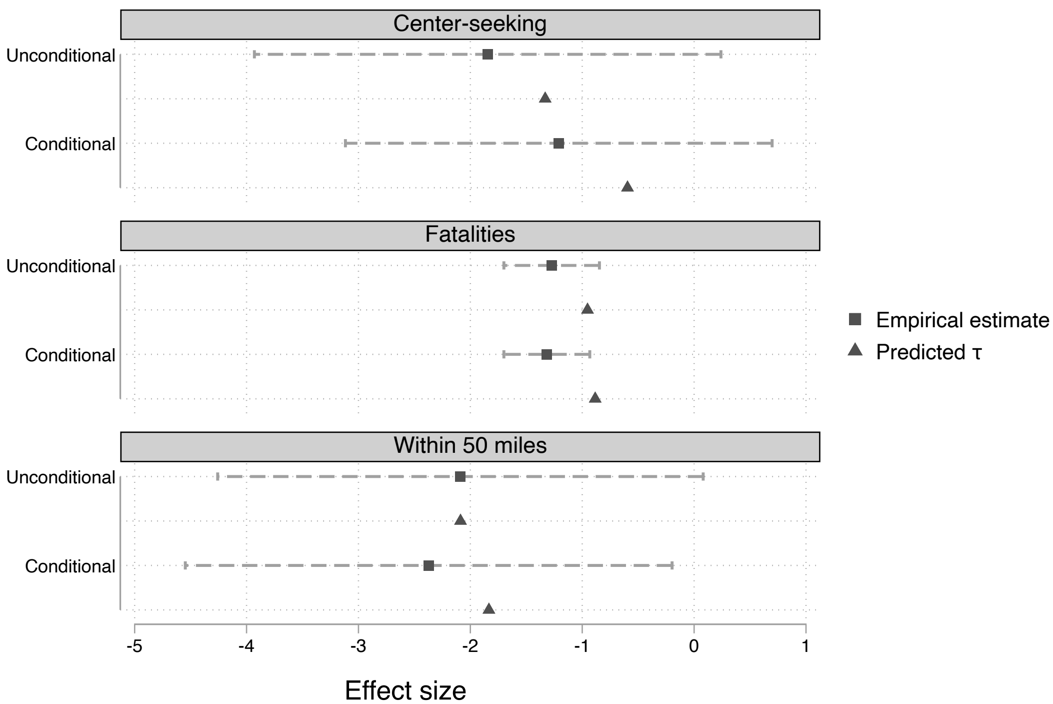
loses significance.[37] Conflicts that threaten the capital reduce income by 6.8-7.8% relative to baseline, suggestive of large nonlinear costs of state collapse in civil war.

The results in Panel B largely reprise what we learned Section 6.2, but it is informative to compare magnitudes with Panel A. Across all specifications, the magnitude of market re-

[37] This is perhaps unsurprising, since center-seeking conflicts are more likely to threaten the capital city and may be deadlier as well.

sponses are ordered identically to the cost effects in Panel A – that is, the most payoff-relevant information provokes the largest response. Furthermore, column (4) suggests that investors account for correlation across 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 correlation neglect on this dimension of conflict cost belief formation (Enke and Zimmermann, 2017).

Figure 7: Benchmarking heterogeneous responses by conflict cost predictors



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.

But what do these magnitudes say about investors' beliefs? Figure 7 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 identical 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 remarkably accurate beliefs about conflict costs, especially considering the difficulties of information aggregation and inference in highly uncertain wartime environments.

7 Identification threats

7.1 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 A9 includes various combinations of 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 A10, including obvious potential confounders such as per-capita income and population, interacting each control variable individually with δ_{te}. Table A11 controls for institutional differences by including interactions with country-level World Bank Governance Indicators, while Table A12 measures country risk with ICRG scores. Finally, Table A13 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.

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). Macro shocks that are correlated in time with conflict events may differentially affect conflict-prone countries, biasing our result. We control for this in Table A14 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 various emerging market equity and bond indices, including the EMBI global. We include several major commodity price indices in Table A15 and find the results broadly unchanged.

7.2 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 A7 we estimate the main 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 A8, 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.

Table A16 assesses the role of measurement error induced by imprecision in the UCDP data, finding no evidence that the main effect varies systematically with the level of data precision. To address the role of outliers, in Table A17 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 A18 and Figure A9, 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 A19. 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.

8 Conclusion

As the war in Ukraine vividly demonstrates, 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. This suggests that investors react to relevant information about state solvency, rather than naively to bad news. Along those lines, we show that markets respond more to surprising events where priors are optimistic, and severe conflicts where expected costs are high and rebels explicitly threaten the state. Despite a market replete with information asymmetries and plausible behavioral biases, investors form data-driven, conflict-specific priors and cost expectations that incorporate both historical knowledge and initial signals on the spatial and political characteristics of war.

To quantify the effects relative to an efficient benchmark, we build a simple two-state, fixed-income asset pricing model with uncertainty on the likelihood and cost of conflict. Combining the model with reduced form estimates of the conditional dynamic treatment effects allows us to identify the speed of learning and the magnitude of bias in investors' beliefs about conflict. We find that while investors initially underreact by 86%, they price in up to 75% of the shock by day 15. A combination of both information frictions and biased beliefs likely explain why the investor response, while rapidly converging, is not immediate or complete.

When interpreted through the lens of the model, the magnitudes of heterogeneous market responses show that bondholders form empirically accurate beliefs about conflict costs from initial information on conflict characteristics.

The findings provide the most externally valid, well-identified estimate of the bond market effects of conflict, and provide the first systematic tests of market efficiency in the face of political violence. Still, several questions remain unanswered. How much do investors really understand about 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? Do local bond markets, often dominated by insiders, respond differently than investors in international bond issues? How does local corporate and banking sector react to conflict in terms of investment decisions and the deployment of personnel? We view these as important avenues for future research.

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A Appendix

A.1 Appendix tables

Table A1: 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 A2: 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 A3: 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
R^2	0.977

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 A4: 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
R^2	0.978	0.978	0.978	0.978

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 A5: 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
R^2	0.037	0.975	0.992	0.993	0.689

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 5. Sample is all country-years for which data is available from 1990-2000 *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: 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 5.

Table A7: 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
R^2	0.978	0.980	0.971	0.974	0.959	0.976	0.978	0.981

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 A8: 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
R^2	0.978	0.980	0.976	0.978

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 A9: Conflict onset and bond prices: robustness to bond characteristics

Dependent variable	Bond price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: State forces, 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
R^2	0.979	0.981	0.982	0.984	0.979	0.981	0.977	0.979
Panel B: State forces, 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
R^2	0.978	0.980	0.981	0.983	0.978	0.980	0.976	0.978
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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10: Conflict onset and bond prices: robustness to macroeconomic characteristics

Dependent variable Bond price	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
State forces, all episodes												
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
R^2	0.978	0.980	0.980	0.980	0.980	0.980	0.980	0.980	0.980	0.980	0.980	0.980
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
Date FE × Event × Military expenditure (GDP)	No	No	Yes	No	No	No	No	No	No	No	No	No
Date FE × Event FE × GDP growth	No	No	No	Yes	No	No	No	No	No	No	No	No
Date FE × Event FE × GDP per capita (PPP)	No	No	No	No	Yes	No	No	No	No	No	No	No
Date FE × Event FE × Population	No	No	No	No	No	Yes	No	No	No	No	No	No
Date FE × Event FE Foreign investment	No	No	No	No	No	No	Yes	No	No	No	No	No
Date FE × Event FE × External balance	No	No	No	No	No	No	No	Yes	No	No	No	No
Date FE × Event FE × Financial development index	No	No	No	No	No	No	No	No	Yes	No	No	No
Date FE × Event FE × Unemployment rate	No	No	No	No	No	No	No	No	No	Yes	No	No
Date FE × Event FE × Total reserves	No	No	No	No	No	No	No	No	No	No	Yes	No
Date FE × Event FE × Consumer price index	No	No	No	No	No	No	No	No	No	No	No	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 A11: Conflict onset and bond prices: robustness to institutional risk factors

Dependent variable	Bond price					
	(1)	(2)	(3)	(4)	(5)	(6)
State forces, all episodes						
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
R^2	0.975	0.976	0.977	0.977	0.976	0.977
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 × Control of corruption	No	No	Yes	No	No	Yes
Date FE × Event FE × Government effectiveness	No	No	No	Yes	No	Yes
Date FE × Event FE × Political stability	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 A12: Conflict onset and bond prices: robustness to country risk factors

Dependent variable	Bond price													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
State forces, all episodes														
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
R^2	0.975	0.976	0.977	0.977	0.977	0.977	0.977	0.979	0.976	0.978	0.977	0.978	0.976	0.980
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
Date FE × Event FE × Composite risk rating	No	No	Yes	No	No	No	No	No	No	No	No	No	No	Yes
Date FE × Event FE × Economic risk rating	No	No	No	Yes	No	No	No	No	No	No	No	No	No	Yes
Date FE × Event FE × Financial risk rating	No	No	No	No	Yes	No	No	No	No	No	No	No	No	Yes
Date FE × Event FE × Payment delays	No	No	No	No	No	Yes	No	No	No	No	No	No	No	Yes
Date FE × Event FE × Political risk rating	No	No	No	No	No	No	Yes	No	No	No	No	No	No	Yes
Date FE × Event FE × Risk for budget balance	No	No	No	No	No	No	No	Yes	No	No	No	No	No	Yes
Date FE × Event FE × Risk for debt service	No	No	No	No	No	No	No	No	Yes	No	No	No	No	Yes
Date FE × Event FE × Risk for exchange rate stability	No	No	No	No	No	No	No	No	No	Yes	No	No	No	Yes
Date FE × Event FE × Risk for GDP growth	No	No	No	No	No	No	No	No	No	No	Yes	No	No	Yes
Date FE × Event FE × Risk for inflation	No	No	No	No	No	No	No	No	No	No	No	Yes	No	Yes
Date FE × Event FE × Risk for international liquidity	No	No	No	No	No	No	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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13: 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
R^2	0.975	0.976	0.976	0.977	0.977	0.978
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 A14: Conflict onset and bond prices: robustness to global market indices

Dependent variable	Bond price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State forces, all episodes								
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
R^2	0.975	0.976	0.975	0.976	0.975	0.976	0.976	0.975
Bond FE × Event FE	Yes	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). Event sample is all conflict involving state forces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A15: Conflict onset and bond prices: robustness to major commodity prices

Dependent variable	Bond price								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
State forces, all episodes									
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
R^2	0.976	0.976	0.976	0.976	0.976	0.976	0.976	0.976	0.976
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). Event sample is all conflict involving state forces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A16: 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
R^2	0.978
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 A17: 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
R^2	0.980	0.967	0.982	0.981	0.979	0.966	0.982	0.980

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 A18: 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
R^2	0.999	0.983

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 A19: 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
R^2	0.994	0.995	0.995	0.995	0.995	0.994	0.994	0.994	0.994	0.994
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
R^2	0.995	0.996	0.995	0.996	0.996	0.995	0.994	0.994	0.994	0.994
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

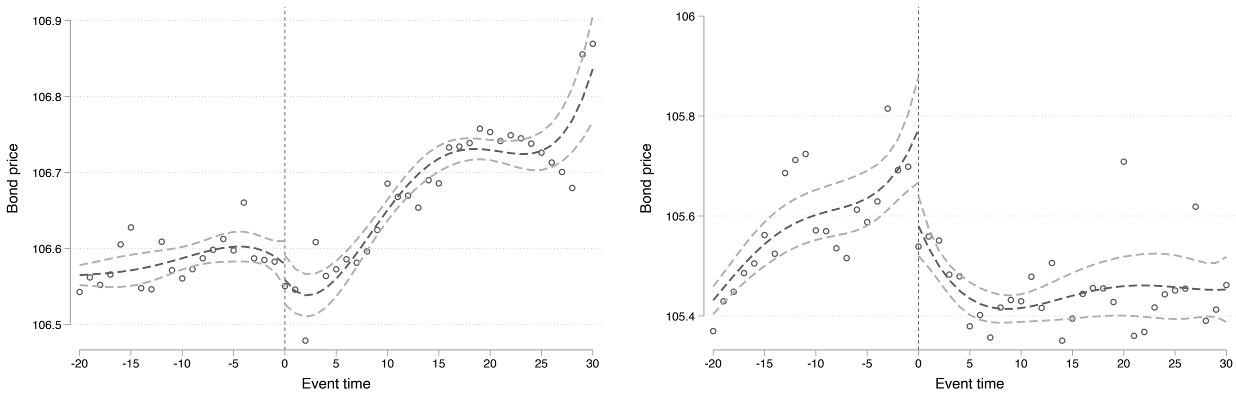
Note: Standard errors in parentheses clustered at the country level. Outcome variable is either daily current yields or log yields, as indicated in the panel headers. Event sample is all conflict involving state forces, with subsamples given in table header. Fatality quintiles indicate samples of events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

A.2 Appendix Figures

Figure A1: Interrupted time series

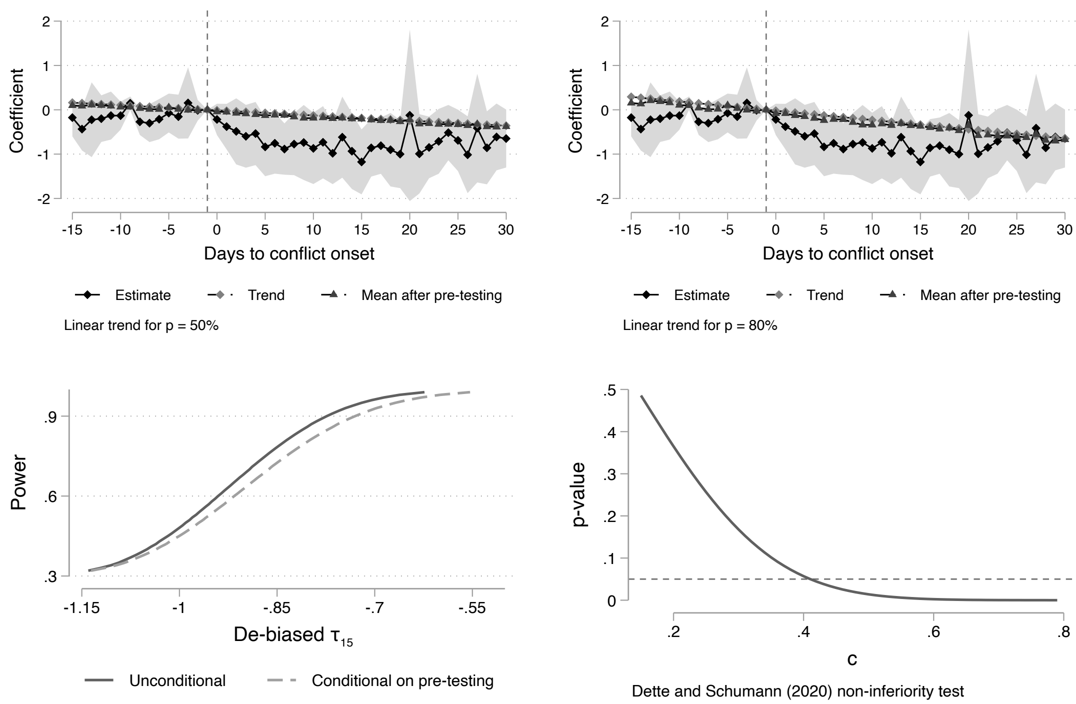
(a) Control bonds

(b) Treated bonds



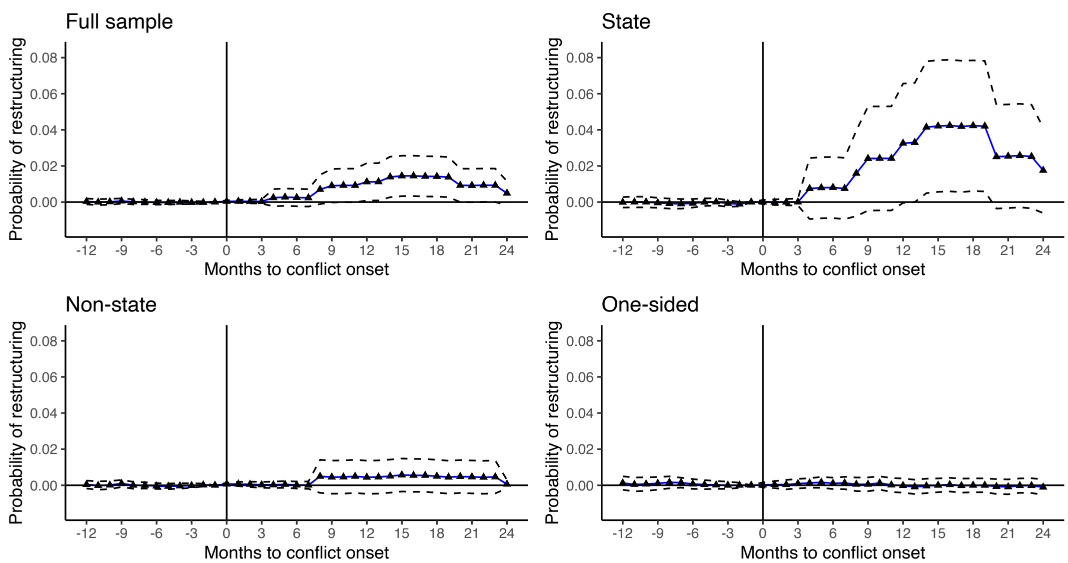
Note: Figure shows results from interrupted time series model of bond prices. Panel (a) and (b) show average bond prices over event-days for control and treated bonds, respectively. Each plot fits a cubic model separately on either side of the event date.

Figure A2: Parallel trends power analysis



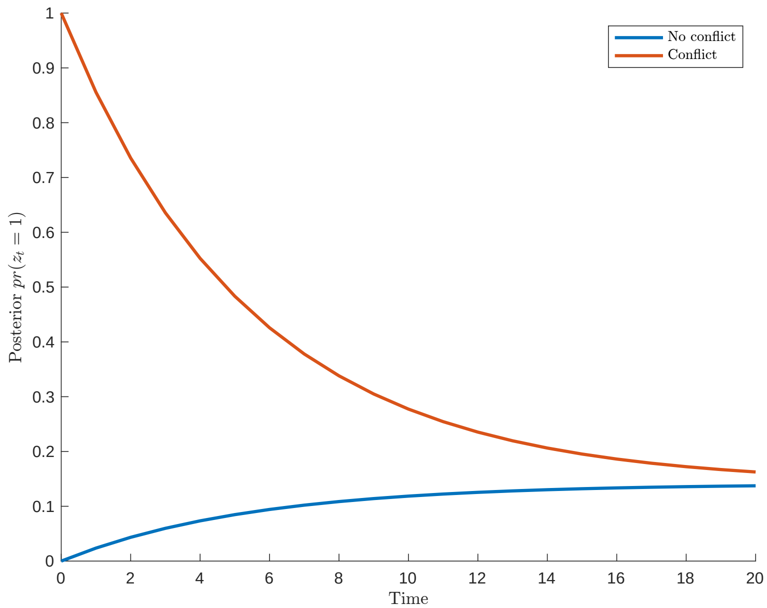
Note: Figure shows pre-test power analysis from Roth (2022). Top panel plots event-study coefficients from Figure 4 for state-involved conflicts. Overlaid are hypothesized linear trends before and after pretesting, with slopes detectable with 0.5 and 0.8 power (top left and right, respectively). Bottom-left 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 A3: Event-study: weakly preemptive restructurings



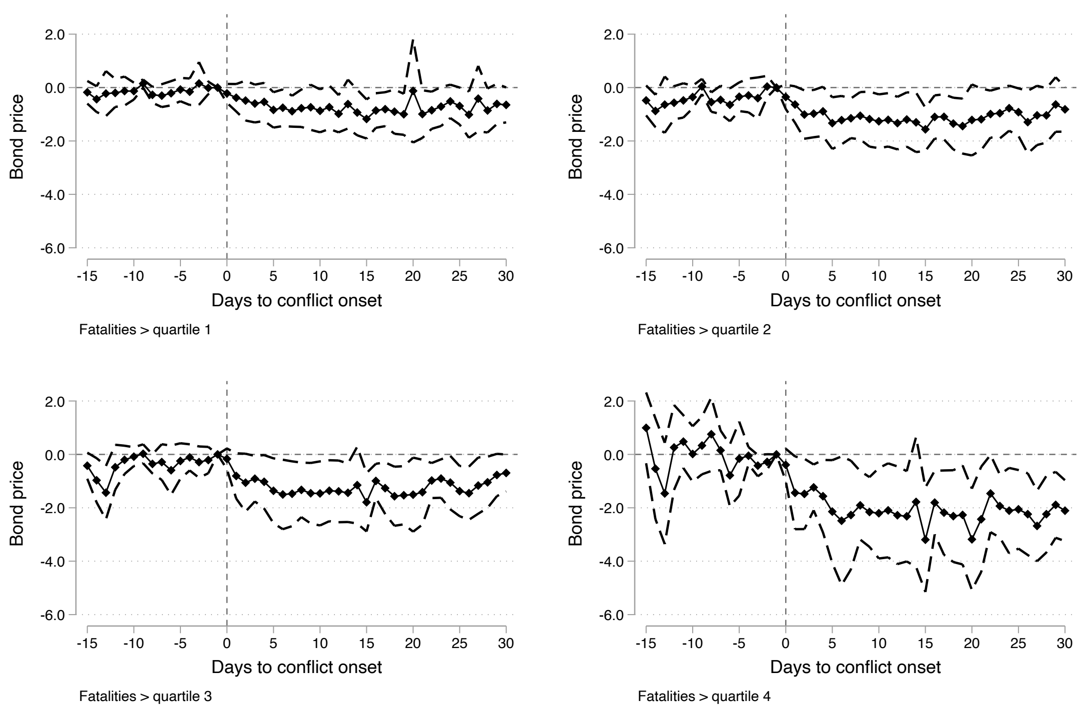
Note: Figure shows coefficients from stacked event-study regressions described in Section 3 on monthly weak preemptive restructurings as defined by Asonuma et al. (2017) for four different samples of conflicts, indicated in each subfigure title. Standard errors are clustered at the country level. Outcome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

Figure A4: Prior beliefs, conflict costs, and price responses



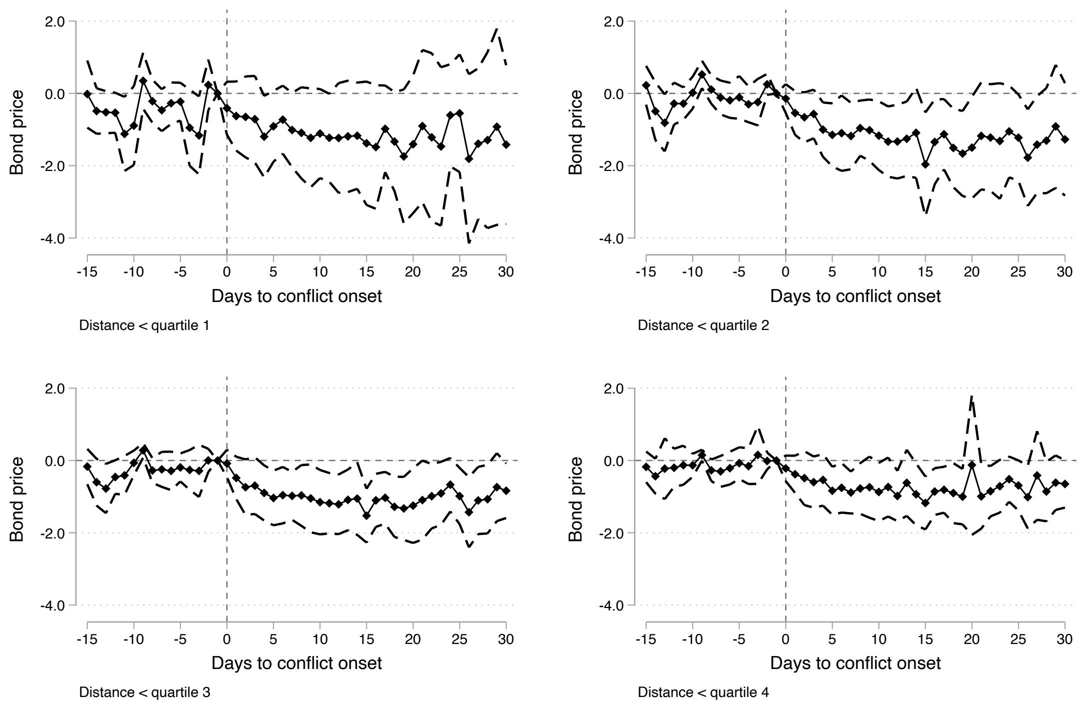
Note: Figure shows the simulated path of beliefs ζ_t on the probability of conflict after observing an event of either conflict or no-conflict. Beliefs are derived from the Markov process of the AR(1) estimates in equation (3).

Figure A5: 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.

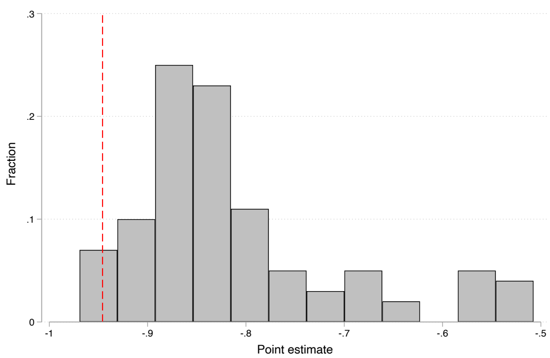
Figure A6: Event-study: distance to capital



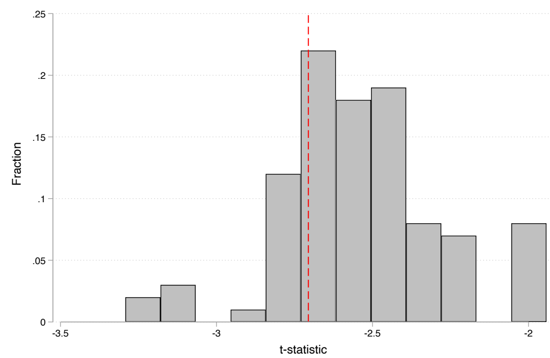
Note: Figure shows coefficients from stacked event-study regressions described in Section 3 on daily bond data for four distance quartiles, indicated in each subfigure footer. Sample includes only events involving state forces. Standard errors are clustered at the country level. Outcome is the daily 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 A7: Robustness to event-windows

(a) β point estimate

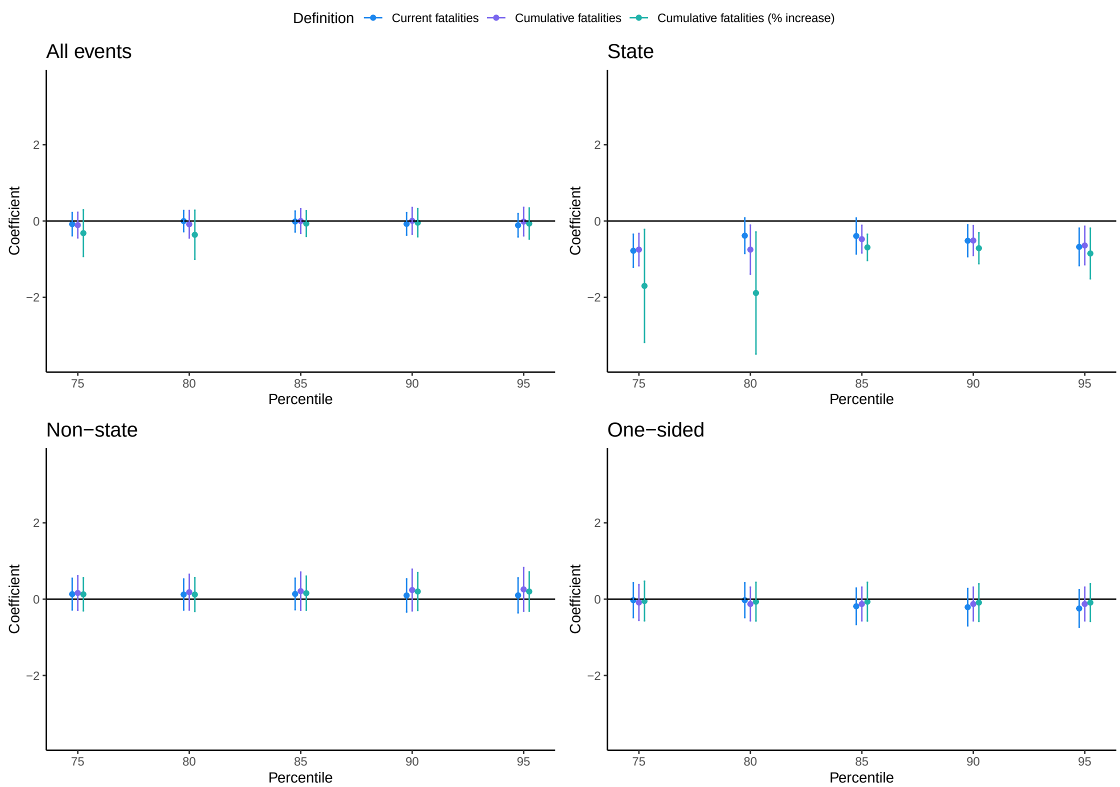


(b) t-statistic



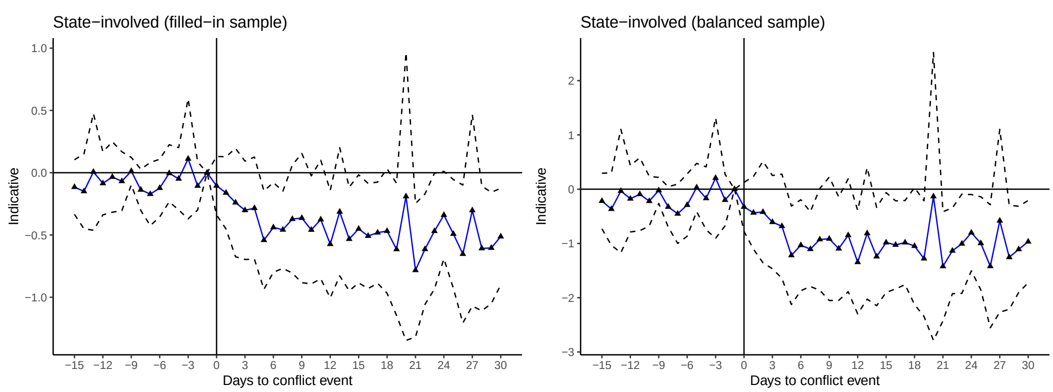
Note: Figure shows the distribution of estimates (a) and t-statistics (b) from all possible event-windows between contained in the +/-30 day interval. Sample is all state-involved conflicts. Regression specification is the stacked estimation in Table 1 column (2).

Figure A8: 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 A9: 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.