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MILITARY SPENDING AND WAR

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ABSTRACT

We revisit the theory of critical deterrence—the idea that military spending may reduce the risk of conflict by increasing its expected costs. To test this theory, we assemble a new cross-national dataset combining information on armed conflicts and defense spending over a 75-year period. We find that increases in military spending have no effect on short-run conflict risk—contrary to concerns that buildups may provoke escalation—but that they do lead to a small and persistent decline in conflict over the long run. Although the effect of spending on conflict is modest, its effect on the cost of war is large: higher military spending raises the scale of battlefield casualties. The effect of military spending on conflict is more pronounced for democracies, which are less likely to use force offensively, and is concentrated in internal conflicts involving nonstate actors. The deterrent power of military spending is strongest in ethnically polarized societies—precisely where the threat of civil war is most acute. Last, using dyadic data, we find that higher military spending causes a decrease in bilateral interstate conflict, and that this effect may be larger than the one we document for intrastate conflict using panel data. Taken together, our results point to a low pass-through from the cost of war to its incidence: even sharp increases in military spending yield only modest gains in deterrence.

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I. Introduction

The recent rise in military expenditures has reignited a foundational question in international relations and the political economy of conflict: Does military spending reduce or increase the risk of conflict? Critical deterrence theory holds that stronger militaries lower the risk of conflict by raising the expected cost of aggression (Schelling, 1966; Fearon, 1995; Powell, 1999). Yet, military buildups may also provoke arms races and raise the likelihood of war (Jervis, 1978; Glaser, 1997). This paper revisits the logic of *critical deterrence*—the idea that military investment may increase short-run tensions but reduce long-run conflict—and provides new evidence supporting the role of military spending in preventing conflict.

To test the predictions of critical deterrence, we assemble a comprehensive dataset covering 161 countries over a 75-year period. We combine detailed information on all armed conflicts since 1948 with country-level data on military spending. In addition, we collect measures of institutional quality to account for heterogeneity in states' ability to deter conflict and to credibly commit not to initiate it.

We begin by examining the short-run consequences of increases in military spending. Specifically, we regress an indicator for conflict on contemporaneous military spending, controlling for country and year fixed effects as well as a lagged conflict indicator. This empirical design absorbs all time-invariant country characteristics—such as the presence of persistently hostile neighbors—and captures global trends in conflict risk. By including lagged conflict, we also account for the persistence of conflict and the possibility that countries increase spending in response to recent violence. However, our estimates are likely to suffer from positive bias due to reverse causality: countries may increase military spending in anticipation of future conflict. Despite this upward bias, when we control for country and year fixed effects, we find no statistically significant relationship between contemporaneous military spending and the likelihood of conflict. Taken together, these results suggest that military spending does not cause an increase in conflict risk over short horizons.

We then turn to the long-run consequences of military spending. To do so, we regress an indicator for conflict h periods ahead on military spending, controlling for country and year fixed effects as well as lagged conflict. As in the short-run analysis, our estimates likely suffer from positive bias due to reverse causality and should therefore be interpreted as upper bounds on the causal effect of military spending on conflict. We find that although higher military spending does not increase the likelihood of conflict in the short run—including two to three years after—it is associated with a lower probability of conflict in the medium run. For instance, a 12% increase in military spending—the average absolute year-to-year change in our sample—is linked to a 2% reduction in the likelihood of conflict 10 years later, relative to the unconditional mean. This reduction is also persistent: the estimated effect 20 years after the increase is nearly identical to that observed after 10 years. Given the direction of the bias, we interpret these findings as evidence that military spending reduces the likelihood of conflict over the medium and long run, although this reduction is modest.

Critical deterrence theory argues that higher military spending reduces the likelihood of conflict by increasing the cost of war. To test this, we draw on two datasets containing information on conflict resolution and battle-related casualties. Our findings support the idea that military spending raises the cost of war. First, we show that increased military spending improves the odds of victory: a 12% increase in military expenditure is associated with a 0.52% increase in the probability of winning a conflict. Second, military spending shapes the human toll of war. A 12% increase in total military spending is linked to a 1.8% rise in annual battle-related deaths, while reducing home-country casualties by 3.4%. Thus, military investment both raises the overall destructiveness of war and shifts the burden of casualties onto opposing forces. Together, these findings suggest that military spending increases the expected cost of war for potential challengers—by enhancing the likelihood of defeat and magnifying expected losses.

Yet the effect of military spending on conflict remains modest, despite its large effect on wartime outcomes, particularly casualties. This suggests a weak pass-through from the cost of war to the probability of war, contrary to the straightforward predictions of classical deterrence theory. One likely explanation is the persistence of conflict: as we show, the strongest predictor of conflict today is the presence of conflict yesterday. In such a setting, even large increases in the cost of war may not significantly reduce the probability of conflict. This suggests a more nuanced view of deterrence. Whereas military spending raises the stakes of war, its ability to prevent war is constrained by the inertia of conflict dynamics.

Although military spending is often framed as a tool of deterrence, it can also serve as a prelude to aggression. A buildup may signal a defensive posture—or preparation for offensive action. If the deterrence logic holds, increases in military spending should reduce the likelihood of conflict, but only when the spender is not the aggressor. To distinguish between these cases, we draw on the well-established finding that democracies are less likely to initiate aggressive wars (Levy, 1988; Rummel, 1995) and classify countries as democracies or autocracies using data from the Varieties of Democracy project. We find that a 12% increase in military spending reduces the long-run probability of conflict in democracies by 3.6% relative to the mean. In contrast, the same increase is associated with a 0.84% rise in conflict risk for autocracies. These results suggest that the strategic intent behind military spending—whether to deter or to provoke—matters critically for its effects.

So far, we have treated all conflicts equally. Interstate wars—those between sovereign states—have declined, while intrastate conflicts, often involving governments and non-state actors, have become far more common. Although critical deterrence theory was originally developed to explain strategic interactions between states, scholars have extended its logic to civil wars as well (Walter, 2002). Still, this extension is not without controversy. Some have argued that increased military capacity may fuel repression or embolden governments to suppress minority groups, potentially raising the risk of civil war (Fearon and Laitin, 2003; Collier and Hoeffler, 2004). Against this backdrop, our findings are striking. We find that military spending has no discernible effect on the likelihood of interstate conflict. In contrast, increases in military spending lead to a persistent decline in the probability of intrastate conflict. A 12% increase in military spending reduces the long-run likelihood of civil conflict by 2.2% relative to the unconditional mean. These results suggest that states may deter rebel groups not only by signaling strength but by raising the expected costs of insurgency—supporting the core logic of critical deterrence in the intrastate context.

Our findings on intrastate conflict suggest that states deter rebellion by raising the cost of war for potential insurgents. Yet this logic presumes that rebellion is a credible threat to begin with. One prominent driver of civil conflict is ethnic polarization, which undermines social cohesion and facilitates group-based mobilization (Montalvo and Reynal-Querol, 2005; Esteban, Mayoral and Ray, 2012). To examine whether the deterrent effect of military spending varies with underlying societal divisions, we divide countries into high and low ethnic polarization groups, using the cross-sectional median of the polarization index developed by Montalvo and Reynal-Querol (2005). We find that in countries with low ethnic polarization, increases in military spending have no discernible impact on the likelihood of civil conflict. In contrast, in highly polarized societies, a 12% increase in military spending is associated with a 2.3% reduction in the long-run probability of intrastate conflict, relative to the unconditional mean. These results suggest that military deterrence is effective only when internal divisions make rebellion plausible—strengthening the view that deterrence operates by raising the cost of violence where the threat is real. These results underscore the importance of matching deterrent strategies to local conditions: in low-risk environments, military spending may have little effect, but in polarized societies where the threat of rebellion is salient, it plays a meaningful role in preventing conflict.

We then turn to a dyadic analysis of the deterrent effect of military spending. Using a dataset in which each observation corresponds to a country pair (dyad) and year, we estimate a series of local projections where the outcome is a dyadic conflict indicator and the regressor is total military spending by both countries. When we restrict attention to direct conflict between the two states, we find no statistically significant effect. However, when we broaden the definition to include indirect conflict—such as cases in which one country supports the adversaries of the other—military spending is associated with a reduction in conflict risk over the medium run, with no evidence of short-run escalation. A one-time 11 percent increase in joint military spending—the average absolute year-on-year change in the data—reduces the probability of conflict by 5.3 percent ten years later. This implies an elasticity of approximately -0.5, larger in magnitude than our estimates from country-year regressions.

The difference in elasticities reflects three factors. First, the dyadic sample is smaller and skewed toward interstate conflict: only 61 conflicts are included, of which 82 percent are interstate. Second, the unconditional probability of dyadic conflict is low, mechanically amplifying the estimated elasticity. Nevertheless, our results show that, in a dyadic setting, military spending acts as a deterrent to interstate conflict. Our results suggest the effectiveness of military spending in deterring conflict may be more pronounced for interstate conflict than it is for intrastate conflict.

This paper contributes to a longstanding debate on the logic and empirical validity of critical deterrence theory. Classical deterrence theory emphasizes how credible threats can prevent aggression by raising the expected cost of war. Seminal works by Schelling (1966), Fearon (1995), and Powell (1999) frame conflict as a breakdown in bargaining, often due to issues of commitment, private information, or incentives for preemptive strikes. In these models, military power functions as a deterrent by shifting adversaries' expectations about the costs of escalation.[1]

The empirical literature on deterrence and military spending remains divided.[2] Some studies argue that arms buildups are destabilizing, increasing the risk of conflict by fu-

[1]There is also a large literature on the causes of war, which is reviewed by Baliga and Sjöström (2024) and Acemoglu and Wolitzky (2024). Baliga and Sjöström (2004) argue that uncertainty over resolve or costs can prevent peaceful bargaining and thus may lead to war. Baliga, Lucca and Sjöström (2011) argue that democratic accountability can either restrain or provoke aggression depending on domestic political stakes. Fearon (2018) argues that states

[2]There is also a broader literature on the consequences of military spending. Economists have investigated how military budgets interact with political incentives (Nordhaus, 1975), social spending (Aizenman and Glick, 2006), and capital accumulation (Deger and Smith, 1983). Political scientists, in turn, have focused on the signaling value of defense outlays. Gartzke (2007) argues that deterrence emerges from credible power projection rather than pacifist preferences.

eling mutual suspicion or triggering preemptive aggression (Jervis, 1978). Fearon (2018) models how the costs of anarchy can sustain cooperation in the absence of centralized enforcement, showing that states may invest in arming and incur inefficiencies precisely to maintain peaceful bargaining under the threat of war. Others find that greater military capabilities reduce the likelihood of conflict, especially when paired with credible signaling mechanisms (Huth, 1999; Reiter, 1999; Gartzke and Kroenig, 2009). Recent contributions attempt to reconcile these views. For instance, Jo and Simmons (2016) show that regime type and international legal commitments shape how military power translates into deterrence. Similarly, Powell (2006) and Walter (2009) argue that effective deterrence hinges on institutional mechanisms that enable commitment and reduce the risk of miscalculation.

This paper also contributes to the growing literature on rebel deterrence—the idea that strong states can deter insurgency and rebellion by raising the expected costs of opposition. Berman, Shapiro and Felter (2011) show that increased U.S. military presence in Iraq reduced insurgent attacks when combined with improvements in local governance. More broadly, Fearon and Laitin (2003) highlights that civil wars often arise not from grievance but from weak state capacity, which lowers the costs of rebellion and raises the costs of suppression. Similarly, Collier and Hoeffler (2004) argues that civil conflict is driven mostly by the ability of rebel groups to finance a rebellion.[3] Extensions of this view argue that military investment can deter civil conflict by increasing the costs of mobilization for nonstate actors—an idea that underlies much of the counterinsurgency and postconflict reconstruction literature (Weinstein, 2006; Fortna, 2008).

The rest of the paper is organized as follows. Section II describes the data. Section III presents our results on the short-run effects of military spending on conflict. Section IV presents our results on the long-run consequences of higher military spending. Section V concludes.

[3] Our analysis also intersects with the literature on ethnic polarization and civil conflict, as in Walter (2002), Walter (2004) and Pearlman and Cunningham (2012). A growing body of work (e.g., Denny and Walter 2014) suggests that ethnic polarization amplifies the risk of civil war by facilitating cohesive group mobilization and sharpening political grievances. Montalvo and Reynal-Querol (2005) provide evidence that ethnic polarization strongly predicts civil conflict, outperforming other measures of diversity. Theoretical foundations for this result are developed in Esteban and Ray (2011) and Esteban, Mayoral and Ray (2012), who argue that polarization fosters a zero-sum logic of competition and increases the potential gains from rebellion, especially in weak institutional contexts. Other papers, including Fearon and Laitin (2003) disagree and argue state capacity is more important.

II. Data

This section describes the main data sources used in our analysis and presents key summary statistics. We begin by outlining the datasets used to measure conflict, military spending, and institutional quality. We then provide descriptive evidence on trends in global military expenditures and the incidence of conflict over time.

II.A Data Sources

We describe the main data sources used in our analysis below.

1. Conflict Data. Our main data source is the UCDP/PRIO Armed Conflict Dataset from the Uppsala Conflict Data Program. It records all armed conflicts worldwide.[4] A conflict meets four criteria. First, it involves the use of armed force. Second, it causes at least 25 battle-related deaths per year. Third, at least two parties are involved, one of which must be a state government. Fourth, the conflict must be over an incompatibility, either about government—such as the political system or control of the central government—or about territory. The dataset covers 299 conflicts and 2,686 conflict-year pairs. For each, it reports the parties involved, the conflict's location or locations, and the type of incompatibility. We present a list of the main conflicts in our sample in Table I.

2. Conflict Resolution. We draw on data from the Uppsala Conflict Data Program to identify the resolution of conflicts, focusing on the subset—approximately 30%—in which a clear victor is recorded. From this, we construct a conflict-country level dataset that includes the date of resolution and the identity of the winning side.

3. Casualties. We assemble two datasets containing information on war casualties. First, we use data from the Uppsala Conflict Data Program, which reports annual battle-related deaths for all conflicts since 1989.[5] These data allow us to construct a conflict-

[4]We use the 24.1 version of the UCDP/PRIO Armed Conflict Dataset, developed by Gleditsch et al. (2002) and Davies et al. (2024).

[5]Battle-related deaths are defined as fatalities directly attributable to combat and caused by the warring parties. This includes deaths from battlefield engagements, guerrilla activities (e.g., hit-and-run attacks and ambushes), and bombardments of military targets, cities, or villages. Although the intended targets are typically military forces or representatives of the warring sides, significant collateral damage involving civilian deaths often occurs. Importantly, battle-related deaths refer to direct fatalities and exclude indirect war-related deaths caused by disease, starvation, criminality, or deliberate attacks on civilians (i.e., one-sided violence). We use the UCDP's best estimates of battle-related deaths.

year panel of total battle-related deaths. However, they do not allow us to disaggregate casualties by participant.

To address this limitation, we turn to the Correlates of War (COW) Project, which provides participant-level estimates of war casualties for conflicts up to 2007.[6] Although the COW dataset lacks annual casualty data, it enables us to construct a conflict-participant level dataset with disaggregated information on battle-related deaths.

4. Defense Spending Data. We obtain data on military spending from the Military Expenditure Database from the Stockholm International Peace Research Institute. This dataset provides consistent time series on the military spending of countries since the 1950s, although the panel is unbalanced.

5. Data on Democracies. We use data from the Varieties of Democracy (V-Dem) project, which provides a rich set of indicators measuring the quality of political institutions across a wide range of countries. We focus on five high-level democracy indices: electoral, liberal, participatory, deliberative, and egalitarian democracy. To construct a summary measure of institutional quality, we take the average of these five indices.

6. Military Capabilities Data. We obtain data on countries' military capabilities from the COW Project. Our primary measure of military power is the number of active military personnel, which is available for a large set of countries through 2016.

II.B Summary Statistics

Our final dataset is an unbalanced panel covering 161 countries over a 75-year period. Summary statistics are reported in Table II. On average, military spending accounts for 2.74% of GDP, but the distribution is highly right-skewed: the median is only 1.91%. Conflict is common—26% of all observations (defined as country-year pairs) occur during a conflict. Intrastate conflict is far more prevalent than interstate conflict: 24% of observations involve intrastate conflict, compared to just 3% for interstate conflict.

The democracy index from V-Dem is also right-skewed, with a mean of 0.31 on a 0-to-1 scale. In contrast, the ethnic fractionalization index is approximately symmetric, with a mean of 0.45 and a median of 0.46.[7]

[6] We use Version 6 of the National Material Capabilities (NMC) dataset, originally developed by Singer, Bremer and Stuckey (1972) and later expanded by Singer (1988).

[7] We present the distribution of the democracy index in Online Appendix Figure A.1 and the distribution of the ethnic fractionalization index in Online Appendix Figure A.2.

The key variable in our analysis is the level of military spending. In Figure I, we plot total military spending—measured in constant 2015 USD—aggregated across all countries in our sample since 1955.

Panel A of Figure I reveals three distinct periods in global military spending. The first, spanning from the 1950s to the late 1980s, is marked by a steady increase in total spending—driven both by the growing number of countries (globally and in our sample) and by heightened geopolitical tensions during the Cold War. The second period, from the early 1990s through 2001, shows a broad decline in spending, largely reflecting the dissolution of the Soviet Union and a temporary easing of global conflict risk. The third period begins in the aftermath of the September 11 attacks and features a sharp and sustained rise in military expenditures, continuing through the end of the sample. This trend intensifies following the Russian annexation of Crimea in 2014, which coincides with another pronounced increase in global military spending.

Panel B scales military spending by GDP (gross domestic product). Although nominal military spending rose steadily between the 1950s and the late 1980s, its share of national output exhibited a persistent downward trend. This decline suggests that, despite rising absolute expenditures, military spending became progressively less central to the economy over time.

In Figure II, we decompose the number of countries involved in conflict by conflict type: interstate (conflicts between states) and intrastate (conflicts between a state and a nonstate actor, such as a rebel group). Although the total number of countries engaged in conflict has increased over time, this rise accelerates after 2001. Before the end of the Cold War, interstate conflicts were more prevalent. However, since 2001, the growth in conflict participation is driven almost entirely by intrastate conflicts.[8]

In Online Appendix B, we investigate the determinants of conflict and military spending through a series of auxiliary regressions. Online Appendix Table B.1 shows that neither economic variables (GDP, GDP per capita) nor institutional characteristics explain much of the variation in conflict incidence.[9] Instead, conflict persistence dominates: a

[8] The post-2001 rise in intrastate conflict reflects broader shifts in the nature of warfare. Weak state capacity, ethnic polarization, and political exclusion have made many countries vulnerable to internal violence (Fearon, 2003; Collier and Hoeffler, 2004). In parallel, globalization and technological diffusion have lowered the barriers to organization and mobilization for nonstate actors, while the decline of great-power rivalry reduced the incidence of traditional interstate wars. Moreover, foreign involvement in civil conflicts—often via funding, arms, or proxies—has become increasingly common (Kalyvas, 2001), blurring the lines between domestic and international conflict.

[9] Our empirical strategy differs from most of the existing literature in two key ways. First, we include country fixed effects, allowing us to isolate within-country (primarily cyclical) variation in GDP. In contrast, many studies—including Gartzke (2007)—rely heavily on cross-sectional differences in income levels across countries. By focusing on cyclical variation within countries, our approach is better suited to identifying

simple regression on a lagged conflict indicator yields an R^2 of 63%, indicating that the best predictor of conflict in a given period is whether one was already ongoing.[10]

In contrast, military spending is closely tied to economic fundamentals. As shown in Online Appendix Table B.4, GDP and GDP per capita alone account for 81% of the variation in military expenditures. This contrast underscores a key point: while conflict dynamics are largely decoupled from observable economic variables, military spending is tightly anchored to them.

This asymmetry carries important implications for identification. Specifically, it strengthens the case for treating military spending as plausibly exogenous to conflict shocks. Because military spending is well explained by economic fundamentals that do not themselves predict conflict, variation in military spending—conditional on conflict history—is unlikely to be driven by the same unobserved factors that influence conflict onset or intensity.

In short, we find little evidence of a shared economic or institutional determinant that simultaneously drives both military spending and conflict. This reduces concerns about reverse causality or omitted variable bias and strengthens the case for a causal interpretation of our estimates.

III. The Short-Run Effects of Military Spending

To understand the short-run relationship between military spending and the likelihood of conflict, we estimate the following regression:

$$\text{Conflict}_{i,t} = \mu_i + \lambda_t + \gamma \log \text{Spending}_{i,t} + \beta \text{Conflict}_{i,t-1} + \varepsilon_{i,t}, \tag{1}$$

short-run causal effects, rather than capturing long-run structural correlations. Second, we use country-level data, whereas Gartzke (2007) works with dyadic data, measuring GDP at the dyad level, which is conceptually distinct from our framework. Moreover, his analysis does not report R^2 or pseudo-R^2 statistics, making it difficult to assess the explanatory power of economic variables relative to our results.

[10]This pattern holds across conflict types. As shown in Online Appendix Tables B.2 and B.3, lag dependence is strong for both interstate and intrastate conflicts, although interstate conflicts appear somewhat harder to explain. In Online Appendix Table B.3, we also find that ethnic polarization does not significantly explain the prevalence of intrastate conflict. This result contrasts with the findings of Montalvo and Reynal-Querol (2005). We believe this discrepancy can be attributed to three key differences between our analyses. First, Montalvo and Reynal-Querol (2005) restrict attention to severe intrastate conflicts (those with more than 1,000 battle-related deaths), whereas we include all conflicts with at least 25 deaths. Second, their data covers the 1960–1999 period, whereas our sample spans a broader window, from 1953 to 2022. Third, they estimate a logit model, while we use a linear probability model. Last, despite the difference in functional form, their pseudo-R^2 statistics are comparable to our R^2, reinforcing the broader point that economic and institutional variables exhibit limited explanatory power for the incidence of conflict.

where the outcome variable is an indicator that equals one if country i is involved in at least one conflict in year t, and zero if otherwise. The specification includes country fixed effects (μ_i), year fixed effects (λ_t), and a lag of the conflict indicator. Our parameter of interest is γ, which captures the effect of a change in military spending on the change in the conflict status. Standard errors are clustered at the country level.

We present the results from estimating equation (1) in Table III. Without any controls or fixed effects, a 12% increase in military spending is associated with a 0.5 percentage point increase in the likelihood of conflict—equivalent to a 1.9% rise relative to the unconditional mean. Adding country fixed effects substantially increases the estimated coefficient, as well as the R^2, suggesting that much of the variation in military spending and conflict is time-invariant and country-specific. However, once country and year fixed effects are included, the coefficient on military spending effectively drops to zero: a 12% increase is then associated with just a 0.18% rise in the likelihood of conflict. Including a lag of the dependent variable does not materially change the estimate of γ.[11]

Our estimates of γ in equation (1) can be interpreted as an upper bound on the causal effect of military spending on the likelihood of conflict. Accordingly, the most we can say is that increases in military spending have no positive causal effect on conflict onset; the true effect could be negative, but it is unlikely to be positive. This interpretation is justified by the likely direction of endogeneity: if governments tend to increase military spending in anticipation of rising conflict risk, then an ordinary least squares (OLS) regression will overstate any positive relationship between spending and conflict. In that case, controlling imperfectly for conflict risk would bias γ upward, making our estimate a conservative upper bound.

We estimate equation (1) using 75 years of data, during which the nature and incidence of conflict have changed substantially. In particular, recent decades have seen a shift toward conflicts involving states and nonstate actors, rather than traditional interstate wars. Moreover, comparing military spending across distinct geopolitical eras—such as before and after the end of the Cold War—may conflate structurally different relationships. To assess how the estimate of γ evolves over time, we conduct a expanding-window estimation of equation (1). Starting in $\tau = 1960$, we estimate γ using all data for $t \leq \tau$, and then progressively increase τ until 2023. The results of this exercise are presented in Figure III.

Until 1997, the coefficient on military spending is positive, suggesting that the causal effect of military spending on the likelihood of conflict may have been positive during

[11] The coefficient on the lagged dependent variable captures the probability that country i is in conflict at time t, conditional on being in conflict at time $t-1$ and on all included controls. The estimate implies a persistence rate of roughly 65%, indicating that conflicts, once initiated, tend to be long-lasting.

that period. After the Cold War, however, the coefficient is no longer statistically different from zero. Importantly, the end of the Cold War does not appear to represent a discrete structural break. Rather, it marks a continuation of a gradual decline in the estimated effect that begins in the 1970s.[12]

IV. The Long-Run Effects of Military Spending

In this section, we turn to the *long-run* effects of military spending on the likelihood of conflict. Whereas earlier results highlight average effects across horizons, it is crucial to understand how these effects evolve over time in order to evaluate whether military buildups produce lasting deterrence or merely delay conflict. We also examine how the long-run impact of military spending varies across different state characteristics—that is, the extent to which these effects are state-dependent. Finally, we study the billateral deterrent power of military spending.

IV.A Baseline Effects

To assess the long-run relationship between military spending and the likelihood of conflict, we estimate the equation:

$$(2)\ \text{Conflict}_{i,t+h} = \mu_i + \lambda_t + \gamma_h \log \text{Spending}_{i,t} + \beta \text{Conflict}_{i,t-1} + \varepsilon_{i,t}, \quad h = 0, \ldots, 20,$$

where $\text{Conflict}_{i,t+h}$ is an indicator equal to one if country i is involved in at least one conflict in year $t+h$, and zero if otherwise. The specification includes country fixed effects (μ_i), year fixed effects (λ_t), and a lag of the dependent variable. Our parameter of interest

[12] Several factors may explain the decline in the estimated effect of military spending on conflict since the 1970s. First, the nature of conflict has shifted: intrastate conflicts involving nonstate actors have become more prevalent than interstate wars, especially after the end of colonial conflicts and the Cold War (Fearon, 2003; Gleditsch, 2004). Deterrence mechanisms based on conventional military capacity may be less effective in preventing asymmetric or civil conflicts, where state power does not directly translate into reduced conflict risk, as argued by Walter (2002) or Fearon and Laitin (2003). For example, Fearon and Laitin (2003, p. 80) argue that policing may be more effective than investment in military capabilities: "If government forces knew who the rebels were and how to find them, they would be fairly easily destroyed or captured. This is true even in states whose military and police capacities are low". Second, the geopolitical structure of the Cold War period created relatively stable alliances and spheres of influence, where military buildups by client states often had direct strategic backing from superpowers (Lake, 1992; Kalyvas, 2001). As this system unraveled, the strategic logic of military spending may have weakened. Finally, post-1970s global economic integration and norms of multilateralism have increased the opportunity cost of interstate war, potentially muting the strategic relevance of marginal increases in defense spending (Gartzke, 2007; Martin, Mayer and Thoenig, 2008).

is γ_h, which captures the effect of military spending at time t on the probability of conflict h years ahead. Standard errors are clustered at the country level.

Our estimates of γ_h from equation (2) should not be interpreted as causal. Military spending is itself a response to evolving security concerns, and thus likely correlated with unobserved determinants of conflict. For instance, if country i anticipates a rising risk of conflict, it may increase military expenditures. Should conflict subsequently materialize, this reverse causality induces an upward bias in γ_h. As a result, our estimates likely overstate the true effect of military spending on conflict, and should be viewed as upper bounds.

We present the results of estimating equation (2) in Figure IV. Throughout our analysis, we focus on 90% confidence intervals, since we are testing a directional hypothesis—that military spending reduces the likelihood of conflict—rather than a two-sided alternative.[13] That said, all of our results remain robust when using 95% confidence intervals.

We find no evidence that increases in military spending raise the likelihood of conflict in the short run. Over the medium and long run, however, higher military spending is associated with a lower probability of conflict. For example, a 12% increase in military spending—the average year-to-year absolute change in our sample—is linked to a 0.6 percentage point decline in conflict risk, or a 2.4% reduction relative to the unconditional mean. This effect is persistent: the estimate at horizon 20 is nearly identical to that at horizon 15.[14]

Increases in military spending may be persistent, and studying the effects of a one-time increase may understate the full impact of sustained investment. Using our estimates from Figure IV, we find that if a country increases military spending by 12% annually for 10 consecutive years, the probability of conflict declines by 8.3% relative to the unconditional mean.

The estimated effect of military spending on conflict risk is modest. This may reflect the upward bias discussed above—if countries increase military budgets in anticipation of conflict, our estimates understate the true deterrent effect. However, it is also possible that the true causal effect is small. Conflict is highly persistent, as shown in Online Appendix Table B.1, where lagged conflict strongly predicts current conflict. In such a setting, even meaningful changes in military posture may have limited traction. This

[13] A 90% two-sided confidence interval corresponds to a 95% one-sided test. In contrast, using a 95% two-sided interval implies a 97.5% one-sided test, which is unnecessarily conservative.

[14] Our findings are robust to alternative specifications of equation (2). In Online Appendix Figure B.5, we show results from a specification that includes the logarithm of GDP as an additional control. The estimates remain virtually unchanged.

suggests a more nuanced version of critical deterrence, in which the scope for reducing conflict risk through military spending is constrained by the inherent inertia of violent episodes.

All results are robust to the exclusion of the United States, indicating that they are not driven by its exceptional level of military spending or its extensive involvement in armed conflicts.

Our results are also not driven by small-scale conflicts that is, conflicts with relatively few battle-related deaths. In Online Appendix Figure B.2, we reestimate equation (2) using as the outcome variable an indicator that equals one if the country is involved in at least one *severe* conflict in a given year—defined as a conflict with at least 1,000 battle-related deaths—and zero if otherwise. The results closely mirror those presented in Figure IV, confirming that our findings are not sensitive to conflict severity. However, the magnitudes are larger - a 12% increase in military spending is associated with a 4.4% drop in the likelihood of a severe conflict relative to the unconditional mean.

We also estimate equation (2) using the logarithm of cumulative military spending from year $t - \tau$ to t as the regressor. The results, presented ni Online Appendix Figure B.3, are quantitatively similar: a 12% increase in cumulative military spending is associated with a 3.3% decline in the long-run probability of conflict, relative to the unconditional mean.

One of the main challenges to identifying equation (2) is the presence of omitted variables that capture latent conflict risk and may simultaneously drive military spending at time t and conflict at time $t + h$. To assess the robustness of our estimates to such bias, we implement the method proposed by Oster (2019). This approach yields a statistic, δ, which quantifies how strong selection on unobservables would have to be—relative to selection on observables—to reduce our estimated effects to zero. As shown in Online Appendix Figure B.1, our estimates of δ consistently exceed 1.5, with several approaching 2. This suggests that unobserved confounders would need to be 1.5 to 2 times more correlated with both military spending and conflict than our observed covariates—a scenario we view as unlikely, given the structure of the fixed effects in our specification.

IV.B The Cost of War

We find that increases in military spending reduce the likelihood of conflict, though the effect is modest. According to traditional deterrence theory Schelling (1966), this decline should result from an increase in the expected cost of war. To shed light on this relationship, we examine how military spending affects both the probability of victory and the human toll of war—two outcomes that shape the strategic calculus of potential challengers.

1. Military Spending and Victory We begin by examining the relationship between military spending and the probability of winning a conflict. Using our dataset on conflict outcomes, we estimate the following specification:

$$\text{Victory}_{c,i} = \alpha + \beta \log \text{Spending}_{i,\tau_c - h:\tau_c} + \varepsilon_{c,i}, \tag{3}$$

where $\text{Victory}_{c,i}$ equals one if country i is identified as the victor in conflict c, and zero otherwise. The key regressor is the logarithm of cumulative military spending by country i over the h years preceding the resolution of the conflict in year τ_c.[15] Estimation results are presented in Table IV.

We find that higher military spending is associated with a greater likelihood of victory. A 1% increase in military expenditure is linked to a 0.52% rise in the probability of winning, relative to the unconditional mean. This effect is stable across different values of the horizon h used to compute cumulative spending, as shown in Online Figure B.11.[16] Taken together, these results suggest that military spending is productive in the narrow sense, as it improves a country's chances of prevailing in armed conflict. However, the effect of military spending on the likelihood of victory is small.

2. Military Spending and Casualties To examine the relationship between military spending and the intensity of conflict, we estimate two complementary specifications.

First, using our dataset on total battle-related deaths at the conflict-year level, we estimate:

$$\log \text{Total Deaths}_{c,t} = \mu_c + \lambda_t + \beta \log \text{Spending}_{i,t-h:t} + \varepsilon_{c,t}, \tag{4}$$

where the dependent variable is the logarithm of total battle-related deaths in conflict c during year t. The key independent variable is the logarithm of aggregate military spend-

[15] We do not include country fixed effects; doing so would restrict identification to countries involved in multiple conflicts, significantly reducing variation and limiting the generalizability of our results.

[16] When we include conflict fixed effects, the coefficient on military spending remains positive and statistically significant at longer horizons, but becomes statistically insignificant at short horizons—though still positive in magnitude. We interpret this attenuation as a mechanical consequence of the structure of the outcome variable. Victory is defined in relative terms: within each conflict, one country wins and another loses. Including conflict fixed effects removes all between-conflict variation and forces identification to rely solely on within-conflict differences in spending. At short horizons, these differences may be modest or correlated across participants, making it harder to detect an effect. As a result, the fixed effects may artificially attenuate the relationship between military spending and victory, even when a true effect exists.

ing by all participants over the h years preceding year t. We include conflict and year fixed effects, and we cluster standard errors at the conflict level. Conflict fixed effects absorb the overall intensity of each conflict, while year fixed effects capture time-varying factors that influence the productivity of military spending. Estimation results are presented in Table V.

We find that higher military spending is associated with more intense conflict. A 12% increase in total spending by all participants leads to a 1.8% rise in annual battle-related deaths—equivalent to roughly 22 additional fatalities. These findings are robust to alternative specifications, as shown in Table V, and to different values of the horizon h used to compute cumulative spending, as shown in Online Appendix Figure B.12. Overall, the estimates in Table V point to a clear pattern: greater military spending increases the human cost of conflict.

Second, we turn to our conflict-participant level dataset and estimate:

$$\log \text{Deaths}_{c,i} = \mu_c + \beta \log \text{Spending}_{i,\tau_c - h:\tau_c} + \varepsilon_{c,i}, \tag{5}$$

where the dependent variable is the logarithm of total battle-related deaths incurred by country i in conflict c.[17] The main regressor is the logarithm of cumulative military spending by country i over the h years leading up to the conflict's resolution in year τ_c. We again include conflict fixed effects and cluster standard errors at the conflict level. The inclusion of conflict fixed effects is particularly important, as it allows us to interpret country-level casualties in relative terms. Estimation results are reported in Table VI.

We find that although greater military spending is associated with more intense conflicts overall, the distribution of casualties is not uniform. Countries that spend more experience significantly fewer relative losses: a 12% increase in military spending reduces battle-related deaths for the spending country by 3.4%, or approximately 462 fatalities. These results are robust to alternative specifications (Table VI) and to the choice of horizon h used to construct cumulative spending (Online Appendix Figure B.13).

We find that increases in military spending substantially raise the costs of war by amplifying casualties, although the effects on the likelihood of victory are small. Yet the overall effect of military spending on conflict remains modest. This suggests that traditional deterrence theory may overstate the elasticity of conflict with respect to the expected cost of war. As shown in Online Appendix Table B.1, conflict is highly history-dependent: the strongest predictor of conflict in a given year is whether conflict occurred the year before.

[17] We cannot apply the same approach as in equation (2), where the outcome is measured h periods ahead and a sequence of dynamic coefficients is estimated, because the datasets used in equations (3)–(5) either lack a time dimension altogether or span too few periods to support such analysis.

As a result, even large increases in military spending may fail to generate large declines in conflict risk, despite meaningfully raising the expected costs of engagement.

There are two key identification challenges in estimating equations (3)–(5). The first concerns selection: as shown in Figure IV, changes in military spending also affect the likelihood that conflict occurs. If the joint distribution of outcomes (victory or casualties) and military spending differs systematically between realized and deterred conflicts, our estimates may be biased.

This bias is likely negative in the case of equation (3). By conditioning on conflicts that occurred, we focus on cases where deterrence failed—that is, where increases in military spending did not sufficiently raise the expected cost of war for potential aggressors. As a result, we may systematically observe cases where military investment had a limited effect on the likelihood of victory, thus biasing our estimate downward.[18]

A similar logic applies to equation (4): by excluding deterred conflicts, we likely understate the effect of military spending on total casualties. In both cases, our estimates should be interpreted as lower bounds on the true causal effect.

In contrast, the estimates from equation (5) may be biased in the opposite direction. Because we include conflict fixed effects, total casualties in a given conflict are effectively held constant. Suppose one country increases military spending and thereby reduces its own casualties. In our fixed effects framework, this mechanically implies that other countries in the same conflict bear relatively higher casualties. Because we hold their spending constant, this may introduce a negative bias in the estimates—overstating the protective effect of military spending at the country level.[19]

The second identification challenge concerns strategic responses by other countries. If an increase in military spending by country i prompts rival states to boost their own spending—an arms race—then both the probability of victory and the intensity of conflict may change in equilibrium.

In the context of equation (3), such strategic reactions would tend to attenuate the estimated effect of military spending. If other countries respond in kind, the marginal advantage of country i's military buildup is diluted, introducing a negative bias in the

[18] It is possible that countries increase military spending in anticipation of conflict rather than to deter it. In this case, our estimates still capture the effect of military spending on outcomes such as victory and casualties. A more serious concern would arise if anticipatory spending by one country triggers an arms race, leading other participants to also increase spending. Such strategic responses could introduce bias. However, our inclusion of conflict fixed effects helps absorb this common variation across participants within the same conflict, mitigating this concern.

[19] To see this more clearly, suppose country i increases spending and experiences fewer deaths. With conflict fixed effects in place, the decrease in i's casualties is offset by an increase in casualties attributed to other participants. Since we do not account for changes in their spending, this may exaggerate the marginal benefit of spending for country i.

estimated relationship between spending and victory.

For equations (4) and (5), however, this concern is less acute. Our inclusion of conflict fixed effects absorbs the level of overall military spending across participants, effectively controlling for the intensity of arms competition within a given conflict. As a result, any bias arising from endogenous responses by other countries should be negligible in these specifications.

IV.C State-Dependent Effects: Role of Institutions

We have shown that increases in military spending are associated with a lower likelihood of conflict in the medium and long run. This effect supports the logic of critical deterrence: higher military spending raises the expected cost of conflict for potentially hostile states. However, deterrence is most effective when the increase in spending is not driven by predatory intent—that is, when the country is not preparing to initiate aggression. Democracies, in particular, are generally less likely to behave aggressively toward other states.[20] As a result, we expect the deterrent effect of military spending to be stronger—both in magnitude and statistical significance—for democracies than for autocracies.

To test this hypothesis, we classify countries as democracies or autocracies using the Varieties of Democracy (V-Dem) dataset. Specifically, we construct a composite measure by averaging the five main democracy indices reported by V-Dem.[21] For each year $t-1$, we split the sample at the cross-sectional median: countries with an index below the median are classified as autocracies, while those above are classified as democracies. We then estimate equation (2) separately for each subsample and present the results in Figure V.

We find that increases in military spending have little impact on the likelihood of conflict for autocracies. In contrast, democracies experience substantial and persistent reductions in conflict risk following similar spending increases. For instance, a 12% increase in military spending reduces the probability of conflict in democracies by 0.9 percentage points—or 3.6% relative to the unconditional mean. The same increase is associated with a 0.84% *increase* in conflict risk for autocracies. These results support the hypothesis that

[20] Numerous studies show that democracies are far less likely to initiate aggressive wars, especially against other democracies. For example, Rummel (1995) finds no conflicts between full democracies from 1816 to 1991. This pattern—known as the "democratic peace"—is one of the most robust empirical findings in political science (Levy, 1988; Maoz and Russett, 1993; Reiter and Stam, 2003, 2010). Institutional constraints, public accountability, and shared norms reduce both the willingness and the capacity of democratic governments to act as aggressors.

[21] These indices are: electoral, liberal, participatory, deliberative, and egalitarian democracy. Our results are qualitatively identical if we use any of the five indices individually.

democratic institutions reduce the likelihood of predatory behavior.

Our results are also robust to alternative measures of institutional quality. In Online Appendix Figure B.4, we replicate the analysis using V-Dem's index of political corruption in place of the democracy measure. We find that increases in military spending reduce the likelihood of conflict only in countries with low levels of political corruption; in highly corrupt countries, the deterrent effect is absent.[22]

IV.D *Interstate versus Intrastate Conflict*

So far, we have treated all conflicts as identical. However, as shown in Figure II, the rise in the number of countries engaged in conflict is driven primarily by an increase in violence between states and nonstate actors. Traditional deterrence theory was developed with interstate conflict in mind, but its core logic could also apply to civil or asymmetric warfare, as argued by Walter (2002), Fearon and Laitin (2003), and Gartzke (2007).[23] That said, while the qualitative implications of deterrence theory may carry over, its quantitative predictions could differ across conflict types.

We therefore examine whether the effects of military spending differ across conflict types—specifically, between conflicts involving two states and those involving states and nonstate actors. To do so, we use the UCDP classification of conflicts to divide our sample accordingly and estimate equation (2) separately for each subsample.[24] The results of this analysis are presented in Figure VI.

We find that military spending has no discernible effect on the likelihood of conflict between states. In contrast, increases in military spending lead to a sharp and persistent reduction in the probability of conflict between states and nonstate actors. For instance, a 12% increase in military spending reduces the likelihood of conflict 15 years later by 0.6 percentage points, or 2.2% relative to the unconditional mean. These effects are also long-lasting, persisting well beyond the initial years following the increase. Taken together, our results indicate that the overall relationship between military spending and

[22]In Online Appendix Figure B.6 we present the results of the estimation while including the logarithm of GDP as an additional control. The results are very similar.

[23]Many conflicts between states and nonstate actors can also be interpreted as proxy wars between states, where one side delegates warfare to a nonstate actor. This form of indirect conflict was common during the Cold War and remains prevalent today—for instance, the Israel–Hamas war can be seen as a conflict between Israel and states such as Iran.

[24]We use the UCDP/PRIO Armed Conflict Dataset, which classifies conflicts into two relevant categories: (1) interstate conflicts, defined as armed contests between two or more states; and (2) intrastate conflicts, defined as armed conflicts between a state and a nonstate actor, with or without foreign involvement. Our results are robust to the exclusion of extrasystemic conflicts, which are mostly wars between colonial powers and liberation movements in the colonies, which are rare and primarily historical

conflict is driven entirely by conflicts involving nonstate actors—not by traditional interstate wars.[25]

Our findings diverge from parts of the civil conflict literature. Notably, Fearon and Laitin (2003) argue that it is overall state capacity—such as the ability to govern and police territory effectively—that deters rebellion, rather than military spending in isolation. In contrast, we show that changes in military spending can act as a credible deterrent to civil conflict. This suggests that the signaling function of military investment—the ability to raise the expected cost of rebellion in the short to medium run—may operate independently of underlying state capacity. At the same time, our results do not imply that other dimensions of state power, such as policing or administrative control, are unimportant. Rather, they underscore the specific role of military spending as a tool of deterrence.

As our results show, higher military spending reduces the likelihood of conflict between states and nonstate actors. The underlying causes of conflict differ markedly depending on whether the adversaries are other states or nonstate groups. Interstate wars are often driven by power asymmetries and weak institutional constraints, which increase the risk of miscalculation, opportunism, or failures in credible commitment.[26] In contrast, conflicts between states and nonstate actors—such as rebel groups—are frequently fueled by internal divisions, especially along ethnic lines. Ethnic polarization undermines social cohesion and increases the gains from group-based mobilization, significantly raising the risk of civil war, as shown by Montalvo and Reynal-Querol (2005) and Esteban, Mayoral and Ray (2012).

Our hypothesis is that the effectiveness of military spending in reducing the likelihood of conflict between states and nonstate actors is greater in countries with higher ex-ante levels of ethnic polarization—and that this heterogeneity does not apply to interstate conflict. To measure ethnic polarization, we use the index developed by Montalvo and Reynal-Querol (2005), defined as

$$\text{Polarization} = 1 - \sum_{i=1}^{n} \left(\frac{1}{2} - p_i \right)^2 p_i,$$

where p_i is the population share of ethnic group i in a given country. This measure is maximized when the population is evenly split between two large groups, and it is low when

[25]In Online Appendix Figure B.8 we present the results of the estimation while including the logarithm of GDP as an additional control. Moreover, in Online Appendix Figure B.7 we present the results of the estimation using only severe conflicts (at least 1,000 battle-related deaths). The results remain unchanged.

[26]Empirical work supports this view: states with low institutional quality are more prone to initiating external conflict, particularly when relative military power favors aggression (Mansfield and Snyder, 2005; Besley and Persson, 2011).

either one group dominates or there are many small groups. In these latter cases, the risk of conflict with nonstate actors should be lower—either because one group holds effective control or because coordination among fragmented groups is difficult. Montalvo and Reynal-Querol (2005) show that this polarization index is a strong and robust predictor of civil war.[27] We divide countries into two groups based on the cross-sectional median and estimate equation (2) separately for each subsample. The outcome variable is an indicator equal to one if the country is involved in conflict with a nonstate actor (intrastate conflict), and zero otherwise. The results are presented in Figure VII.

We find that in countries with low levels of ethnic polarization—where the likelihood of civil war is relatively low—increases in military spending have no discernible impact on the risk of intrastate conflict.[28] In contrast, in countries with high ethnic polarization, higher military spending leads to a significant and persistent decline in conflict risk. For instance, a 12% increase in military spending results in a 0.8 percentage point reduction in the probability of intrastate conflict 20 years later, equivalent to a 2.3% decline relative to the unconditional mean. These findings support our hypothesis that military spending is particularly effective at reducing intrastate conflict in ethnically polarized societies, where it raises the cost of conflict for potentially rebellious groups.[29]

Our results also speak to an ongoing debate in the civil conflict literature. On one side, Montalvo and Reynal-Querol (2005) emphasize ethnic polarization as a primary driver of civil war, arguing that deep social divisions (grievance) fuel group-based mobilization. On the other, Collier and Hoeffler (2004) contend that greed—the material opportunity to finance and sustain rebellion—rather than grievance—such as ethnic tension— better explains conflict onset. Our findings suggest that both perspectives capture important elements of the truth. In countries with low ethnic polarization, where the basis for grievance is weak, increases in military spending have no discernible effect on the likelihood of civil conflict. By contrast, in more polarized societies, where grievance-based mobilization is more likely, military spending appears to deter rebellion by raising its expected cost. In this sense, the deterrent effect of military investment operates most clearly where the threat of conflict is already present.

[27] The polarization index is grounded in the theoretical framework developed by Esteban and Ray (1994), who argue that the potential for conflict is highest when social groups are sufficiently large to have political weight but distinct enough to generate intergroup tension.

[28] Our results are also robust to alternative measures of ethnic divisions. In Online Appendix Figure B.14, we use the ethnic fractionalization index from Alesina et al. (2003), and the results remain largely unchanged.

[29] In Online Appendix Figure B.10 we present the results of the estimation while including the logarithm of GDP as an additional control. The results are unchanged.

IV.E Billateral Deterrence

Until now, our analysis has relied on variation at the country-year level to examine the deterrent effects of military spending. Yet conflict is inherently dyadic: it emerges from interactions between at least two actors. To account for this structure, we now turn to a dyadic empirical framework, following Gartzke (2007), and estimate regressions at the dyad-year level to assess how bilateral patterns of military expenditure influence the likelihood of conflict.

We construct a panel at the dyad-year level, where each dyad consists of a pair of countries i and j. Given that international conflict is symmetric—war between the United States and China is identical whether indexed as USA–China or China–USA—we define dyads as symmetric and restrict the sample accordingly.[30] For each dyad-year, we define a binary indicator of bilateral conflict based on data from the Uppsala Conflict Data Program. We distinguish between two forms of conflict: *direct conflict*, in which i and j are formally engaged in war with one another; and *indirect conflict*, which includes cases where i is at war with j, supports the enemies of j, or fights an adversary supported by j.

We restrict the sample to dyads that experienced at least one episode of direct or indirect conflict between 1948 and 2023. Without this restriction, the sample would include dyads such as Nigeria–Chile, which never enter into conflict and for which variation in military spending is unlikely to affect the probability of war. Including such dyads would introduce a form of attenuation bias: in cases where military spending is empirically irrelevant to the onset of conflict, its estimated effect is mechanically muted. By focusing on dyads with nonzero conflict probability, we ensure that the identifying variation pertains to settings in which deterrence is empirically meaningful.[31]

We measure dyadic military spending as the sum of expenditures by both countries in the pair. This construction naturally favors conflicts between states, as data on non-state actors' military resources are typically unavailable. While our baseline estimation in equation (2) covers 299 conflicts—most of them intrastate—the dyadic dataset includes at most 61 conflicts, of which 50 are classified as interstate. If we restrict attention to cases of direct conflict between dyad members and impose data availability on military spending, the sample further narrows to 40 conflicts.[32]

[30] Formally, we include all unordered combinations of N countries taken two at a time, excluding self-pairs. The resulting dataset contains $N(N-1)/2$ dyads.

[31] We present results without this exclusion in Online Appendix B.

[32] Missing data on military spending excludes some otherwise eligible interstate conflicts.

Using the dyadic dataset, we estimate the following local projection equation:

$$(6) \qquad Y_{i,j,t+h} = \mu_{i,j} + \lambda_t + \gamma_h \log(\text{Spending}_{i,j,t}) + \beta Y_{i,j,t-1} + \varepsilon_{i,j,t}, \quad h = 0, \ldots, 20,$$

where the outcome variable $Y_{i,j,t+h}$ is an indicator equal to one if dyad (i, j) is engaged in conflict—direct or indirect—in year $t + h$, and zero otherwise. The specification includes dyad fixed effects $\mu_{i,j}$, year fixed effects λ_t, and a lagged dependent variable to account for persistence. The inclusion of dyadic fixed effects is essential for identification as we rely on cyclical variation to identify the impact of military spending on conflict rather than long-run trends. The parameter of interest is γ_h, which captures the effect of an increase in dyadic military spending on the probability of future conflict h years ahead. Military spending is measured as the log of the sum of expenditures by countries i and j in year t. Standard errors are clustered at the dyad level.[33]

Identification of γ_h in equation (6) requires stronger assumptions than in standard local projections. Beyond the usual condition of conditional exogeneity, identification rests on the symmetry of the spending variable: we must have $\text{Spending}_{i,j,t} = \text{Spending}_{j,i,t}$, as argued by Fafchamps and Gubert (2007). This condition is satisfied by construction, as we define dyads to be unordered pairs. The assumption is necessary given the symmetric structure of the dataset.[34]

The concerns discussed in the estimation of equation (2) remain relevant. In particular, military spending may respond to expectations of future conflict, introducing endogeneity and a likely upward bias in estimates of γ_h. We therefore interpret the resulting coefficients as upper bounds on the true deterrent effect.

Figure VIII presents the results from estimating equation (6) for two outcomes: direct

[33]The appropriate level of clustering in dyadic regressions is a subtle matter. Clustering by dyad accounts for arbitrary serial correlation and heteroskedasticity within bilateral relationships over time. This approach is appropriate given that the unit of observation and the source of dependence is the dyad, as argued by Cameron and Miller (2015). One potential concern is that unobserved country-level shocks—especially those originating from large states such as the United States, the USSR, or China—may induce cross-dyad correlation, leading to downward-biased standard errors. A common remedy is multi-way clustering. However, in the context of symmetric dyads, four-way clustering is technically cumbersome and analytically opaque. Moreover, to the extent that large country shocks spill over broadly, they are plausibly absorbed by year fixed effects. We therefore interpret our estimates conservatively: any remaining correlation across dyads would, if anything, bias our standard errors downward and render our inference more cautious.

[34]An alternative would be to treat dyads as directional, including both USA–China and China–USA as distinct observations. This approach would artificially double the number of dyads, introduce redundant information, and imply that the ordering of countries carries substantive meaning where none exists. More importantly, directional dyads violate the assumption of independent observations: the dyads (i, j) and (j, i) are statistically dependent. Including both would inflate test statistics, underestimate standard errors, and overstate significance—all while estimating a symmetric relationship. Modeling dyads symmetrically avoids these pitfalls and aligns the econometric specification with the structure of the underlying data.

conflict only (Panel A) and the union of direct and indirect conflict (Panel B).

In Panel A, military spending has no statistically significant effect on the likelihood of direct conflict at any horizon. However, as in Figure IV, the point estimates turn negative roughly ten years after the increase in spending, suggesting a delayed but imprecisely estimated effect. In contrast, Panel B reveals a clearer pattern: when we consider both direct and indirect forms of conflict, increases in military spending are associated with a statistically significant reduction in the probability of conflict, with no evidence of short-term escalation.[35] A one-time 11 percent increase in military spending—the average absolute change in dyadic spending in our sample—reduces the likelihood of conflict by approximately 5.4 percent. This implies an elasticity of -0.50, which is notably larger in magnitude than the corresponding estimate from the country-year regressions in equation (2).

The difference in estimated elasticities may reflect several factors. First, it may arise from sample selection: the dyadic regressions include a smaller set of conflicts, limited to cases for which bilateral military spending is observable. Second, the discrepancy may reflect composition effects. If military spending is more effective at deterring interstate conflict, then the overrepresentation of such conflicts in the dyadic sample would, by construction, yield a larger elasticity. In contrast, the country-year regressions draw more heavily on intrastate conflict, where state military spending may play a less decisive role in deterrence.

Finally, the gap in elasticities may also reflect a scale effect. The average probability of conflict in the country-year dataset is 26 percent, while in the dyadic sample it is only 2 percent. A given absolute reduction in conflict likelihood thus translates into a larger elasticity when the baseline probability is lower. The higher elasticity in the dyadic regressions may therefore partly reflect the arithmetic of rare events.

Taken together, these findings suggest that military spending may be more effective as a deterrent in interstate conflicts than in intrastate ones. However, the elasticity estimated from dyadic regressions is likely to overstate the true effect, due to both sample selection and mechanical scaling.

V. Conclusion

This paper revisits the logic of critical deterrence and provides new evidence that increases in military spending reduce the long-run likelihood of conflict. Using a global panel of 161 countries over 75 years, we show that while military buildups do not affect

[35]We also estimate equation (6) for all symmetric dyads. We present the results in Online Appendix B.9. Consistent with the presence of attenuation bias, we find no statistically significant causal impact of military spending on conflict

short-run conflict risk, they lead to a small yet persistent decline in the probability of conflict over longer horizons. These effects are robust across specifications and cannot be explained by reverse causality alone.

We also find that higher military spending increases the expected cost of war, raising the likelihood of victory for the spender and significantly amplifying the human cost of combat. However, this increase in the costs of war is larger than the reduction in the likelihood of conflict, which suggests a low pass-through from the costs of war to the incidence of conflict.

The deterrent effect is concentrated in democracies—where military buildups are less likely to signal aggression—and in intrastate conflicts, particularly in ethnically polarized societies where the threat of civil war is more salient.

Finally, we estimate a set of dyadic regressions to examine the bilateral relationship between military spending and conflict. When we restrict attention to direct conflict between states, joint military spending has no discernible effect. However, once indirect forms of conflict are included, higher military spending is associated with a lower likelihood of conflict in the medium run. The estimated elasticity is larger in magnitude than in our panel regressions, suggesting that military spending may be more effective in deterring interstate conflict than intrastate violence.

Our findings have implications for how we think about military investment. While military spending does not appear to deter conflict in the short run, it reduces conflict risk over longer horizons—particularly in civil war settings and ethnically polarized societies. Yet the effects are modest, and conflict remains highly path-dependent. This suggests that deterrence through military investment can be effective, but its power is conditional and limited. Recognizing these limits is as important as recognizing its potential.

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TABLES AND FIGURES

TABLE I
MAIN CONFLICTS IN CHRONOLOGICAL ORDER

Conflict	Location	Duration	Type	Number of deaths
Korean War	Korea	1950–1953	Interstate	2–3 million
Vietnam War	Vietnam	1955–1975	Interstate	1–4 million
Nigerian Civil War	Nigeria	1967–1970	Intrastate	1–4 million
Soviet–Afghan War	Afghanistan	1979–1989	Interstate	1–2 million
Indochina Conflicts	Vietnam,Cambodia,Laos	1975–1990s	Intrastate	1.7–2.5 million
Salvadoran Civil War	El Salvador	1980–1992	Intrastate	70,000
Second Congo War	DR Congo	1998–2003	Intrastate	5 million+
Gulf War	Iraq,Kuwait	1990–1991	Interstate	30,000
War on Terror	Afghanistan,Iraq	2001–2021	Intrastate	1 million+
Darfur Conflict	Sudan	2003–	Intrastate	300,000+
Syrian Civil War	Syria	2011–	Intrastate	500,000+
Yemen Civil War	Yemen	2014–	Intrastate	370,000+
Russia–Ukraine War	Ukraine	2022–	Interstate	500,000+
Gaza War	Gaza	2023–	Intrastate	50,000+

Notes. This table presents the main conflicts per decade in our sample. For each conflict, we report the type. An interstate conflict is defined as a conflict where two sovereign states engage in direct conflict. An intrastate conflict involves a state fighting one, or more, rebel forces (which may have support by another sovereign state). Deaths include military and civilian casualties, as well as excess deaths caused by the conflict.

TABLE II
SUMMARY STATISTICS

Variable	Mean	St. Dev.	25th Percentile	Median	75th Percentile	*N*
Military Spending	11,266	64,787	112	768	4,259	8,101
GDP	368,890	1,308,008	12,941	44,828	226,056	9,189
Spending/GDP	2.74	3.16	1.20	1.91	3.20	8,067
Conflict	0.26	0.44	0.00	0.00	1.00	12,692
Severe Conflict	0.13	0.33	0.00	0.00	0.00	12,692
Interstate Conflict	0.03	0.18	0.00	0.00	0.00	12,692
Intrastate Conflict	0.24	0.43	0.00	0.00	0.00	12,692
Democracy Index	0.31	0.26	0.10	0.21	0.52	11,559
Ethnic Fractionalization	0.45	0.28	0.19	0.46	0.71	9,348

Notes. This table presents summary statistics for military spending in million 2015 USD, GDP in million 2015 USD, military spending as a share of GDP (in percentage), the indicator for conflict, the indicator for severe conflict (more than 1,000 battle-related deaths in a year), the indicator for interstate conflict, the indicator for intrastate conflict, the democracy index we obtain from V-Dem, and the ethnic fractionalization index computed by Montalvo and Reynal-Querol (2005). For each variable, we compute the average, standard deviation, 25th percentile, median, 75th percentile, and number of observations.

TABLE III
SHORT-RUN EFFECTS OF MILITARY SPENDING ON CONFLICT

	(1)	(2)	(3)	(4)	(5)
Log Spending	0.042***	0.117***	0.038***	0.016	0.004
	(0.008)	(0.026)	(0.008)	(0.020)	(0.008)
Conflict_{t-1}					0.652***
					(0.014)
Country FE		✓		✓	✓
Year FE			✓	✓	✓
Observations	7,948	7,948	7,948	7,948	7,944
Number of countries	161	161	161	161	161
Number of years	75	75	75	75	75
R^2	0.05	0.35	0.14	0.43	0.67

Notes. This table shows the results of estimating equation (1) on our sample of 7,944 observations, where the dependent variable is an indicator equal to one if a country is involved in at least one conflict, and zero if otherwise. We include country fixed effects, year fixed effects, and one lag of the outcome variable. Our coefficient of interest is the one multiplying the logarithm of military spending. Standard errors are clustered at the country level. $***$, $**$, and $*$ denote significance at the 1%, 5%, and 10% level.

TABLE IV
MILITARY SPENDING AND VICTORY

	(1)	(2)	(3)
Log Spending$_t$	0.0283*		
	(0.0149)		
Log Spending$_{t-5:t}$		0.0262*	
		(0.0132)	
Log Spending$_{t-10:t}$			0.0267**
			(0.0133)
Average of outcome	0.61	0.61	0.61
Observations	193	202	205
R^2	0.03	0.03	0.03

Notes. This table shows the results of estimating equation (3) on our dataset on conflict resolution which is at the conflict-participant level, where the dependent variable is an indicator variable that takes the value of one if the country is the victor in the conflict, and zero if otherwise. The regressor is the logarithm of total military spending by the country in the h years that precede the conclusion of the conflict. We present results for $h = 0, 5$, and 10. Standard errors are clustered at the conflict level. $***$, $**$, and $*$ denote significance at the 1%, 5%, and 10% level.

TABLE V
MILITARY SPENDING AND TOTAL DEATHS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log Spending$_t$	0.123**	0.062*	0.127***						
	(0.048)	(0.032)	(0.034)						
Log Spending$_{t-5:t}$				0.120**	0.083**	0.146***			
				(0.046)	(0.035)	(0.033)			
Log Spending$_{t-10:t}$							0.114**	0.075**	0.139***
							(0.046)	(0.036)	(0.035)
Conflict FE		✓	✓		✓	✓		✓	✓
Year FE			✓			✓			✓
Average of outcome	1,491	1,491	1,491	1,491	1,491	1,491	1,491	1,491	1,491
Observations	1,392	1,392	1,392	1,430	1,430	1,430	1,437	1,437	1,437
R^2	0.04	0.61	0.64	0.04	0.61	0.63	0.03	0.61	0.63

Notes. This table shows the results of estimating equation (4) on a dataset at the conflict-year level, where the dependent variable is the logarithm of the total number of battle-related deaths in the conflict for all participants. The regressor is the logarithm of military spending by all participants in the conflict in the h years before. We include conflict and year fixed effects. We present results for $h = 0, 5$, and 10. Standard errors are clustered at the conflict level. $***$, $**$, and $*$ denote significance at the 1%, 5%, and 10% level.

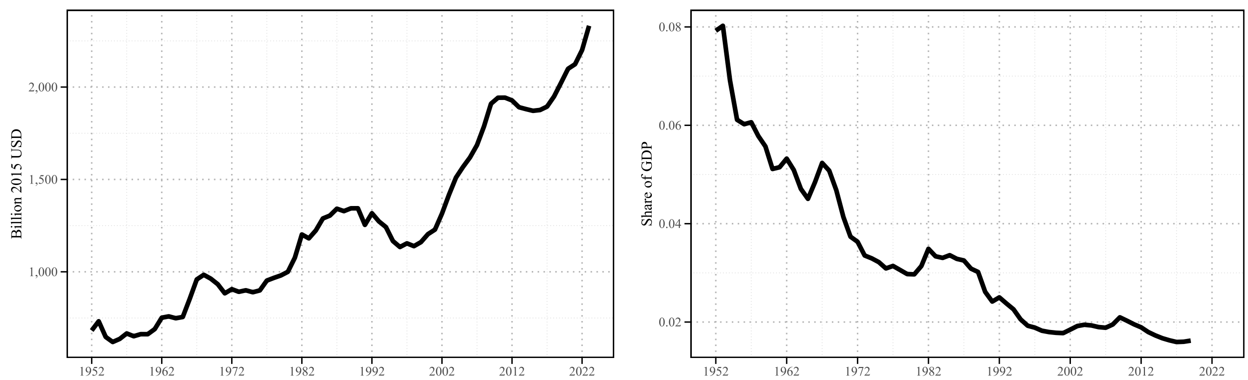
TABLE VI
MILITARY SPENDING AND COUNTRY-LEVEL DEATHS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log Spending$_t$	-0.154***	-0.106	-0.199						
	(0.056)	(0.069)	(0.134)						
Log Spending$_{t-5:t}$				-0.173***	-0.121*	-0.287**			
				(0.053)	(0.069)	(0.087)			
Log Spending$_{t-10:t}$							-0.176***	-0.124*	-0.290***
							(0.052)	(0.069)	(0.083)
Total Deaths		✓			✓			✓	
Conflict FE			✓			✓			✓
Average of outcome	13,258	13,258	13,258	13,258	13,258	13,258	13,258	13,258	13,258
Observations	180	166	180	186	172	186	186	172	186
R^2	0.06	0.08	0.76	0.08	0.10	0.78	0.08	0.10	0.78

Notes. This table shows the results of estimating equation (5) on a dataset at the conflict-participant level, where the dependent variable is the logarithm of the total number of battle-related deaths in the conflict for each participants. The regressor is the logarithm of total military spending by the country in the h years that precede the conclusion of the conflict. We include a conflict fixed effect and the logarithm of the total number of casualties in the conflict as a control. We present results for $h = 0, 5$, and 10. Standard errors are clustered at the conflict level. $***$, $**$, and $*$ denote significance at the 1%, 5%, and 10% level.

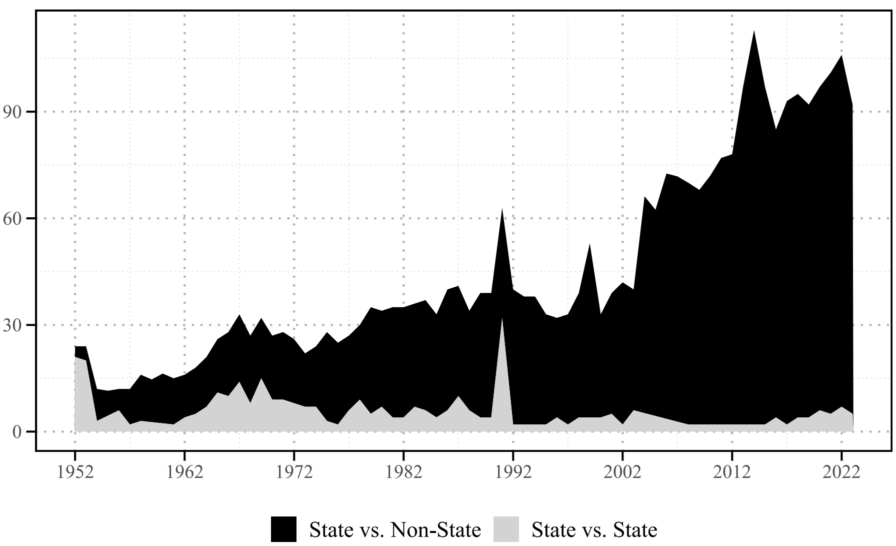
FIGURE I
Evolution of Military Spending

Panel A. In USD Panel B. Share of World GDP



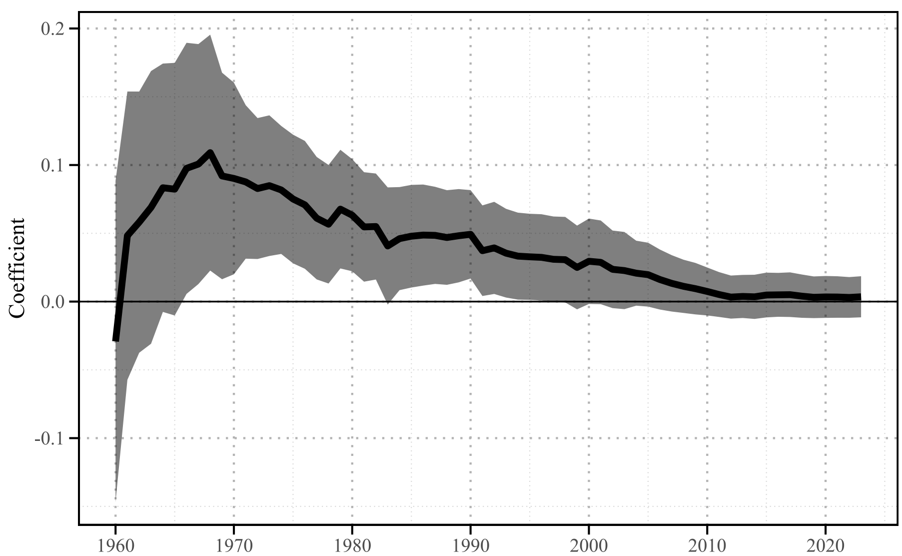
Notes. This figure displays total military spending, in 2015 USD, across all countries in our sample.

FIGURE II
Evolution of Number of Countries in Conflict



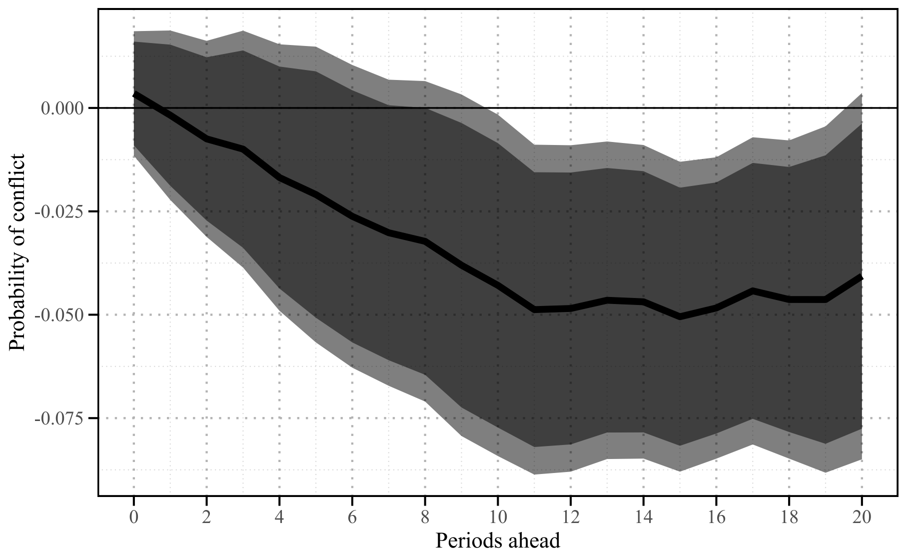
Notes. This figure displays the number of countries engaged in conflict over time. We consider two types of conflict: (1) conflicts involving at least two states, and (2) conflicts involving one state against one nonstate actor (e.g. rebel groups).

FIGURE III
Short-Run Effects of Military Spending on Conflict



Notes. This figure displays estimates of γ from equation (1), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. For each year τ we estimate the equation using all years $t \leq \tau$. The plotted coefficients correspond to the log of military spending, with 95% confidence intervals. Standard errors are clustered at the country level.

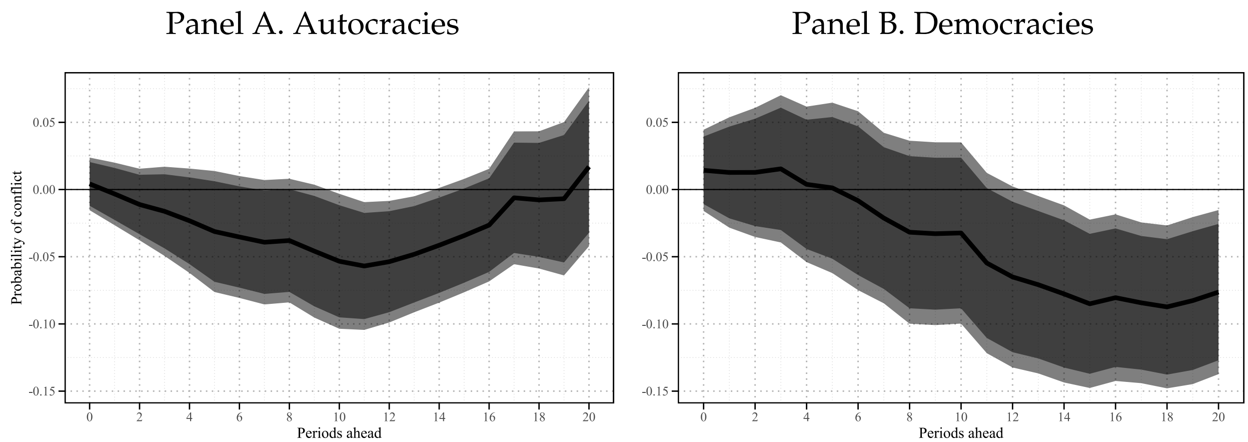
FIGURE IV
Long-Run Effects of Military Spending on Conflict



Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE V

Long-Run Effects of Military Spending on Conflict: Role of Democratic Institutions

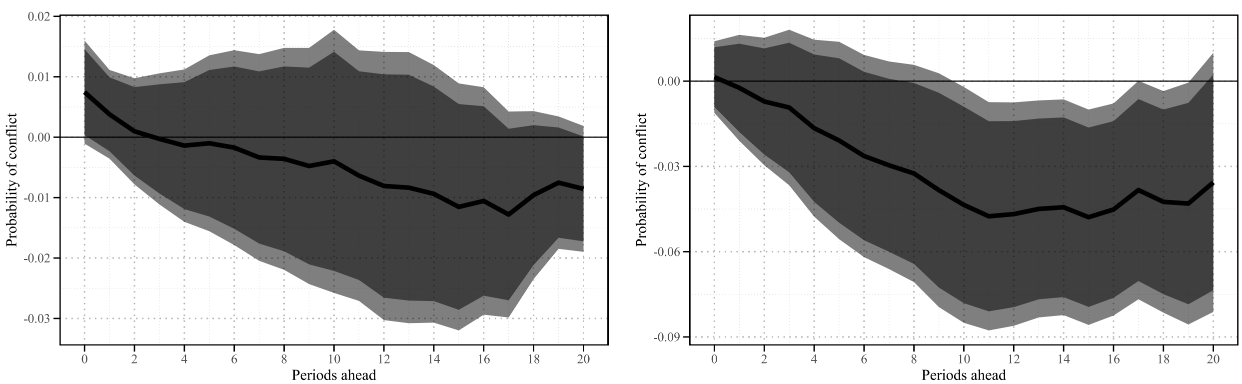


Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We divide our sample into two groups - autocracies and democracies. We use V-Dem's measure of democracy, which is an index between 0 and 1 and is the average of five indices of democracy. Countries that, at time $t-1$, are below the cross-sectional median are classified as autocracies. Countries above the median are classified as democracies. We then estimate equation (2) in each subsample. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE VI
Long-Run Effects of Military Spending on Conflict: Decomposition by Type of Conflict

Panel A. State vs. State

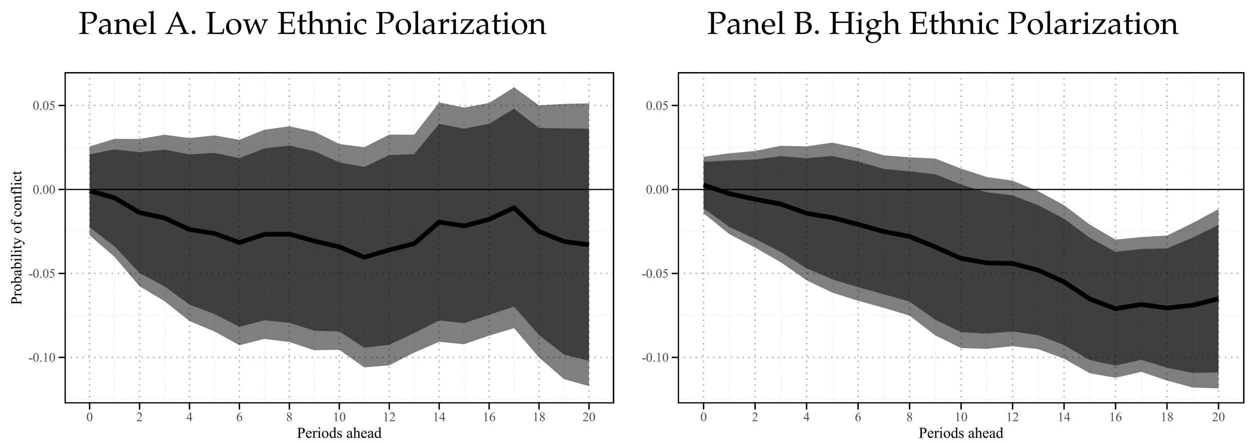
Panel B. State vs. Nonstate



Notes. This figure displays estimates of γ_h from equation (2). We consider two outcome variables. In panel A, the dependent variable is an indicator equal to one if a country is involved in at least one interstate conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. In panel B, the dependent variable is an indicator equal to one if a country is involved in at least one intrastate conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE VII

Long-Run Effects of Military Spending on Intrastate Conflict: Role of Ethnic Polarization

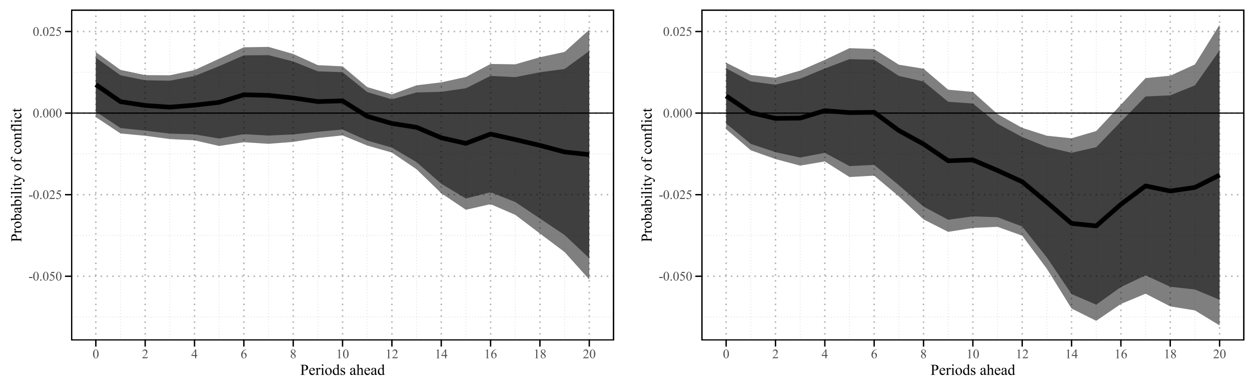


Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict involving one state and one nonstate actor $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We divide countries into two groups based on the measure of ethnic polarization developed by Montalvo and Reynal-Querol (2005): countries with ethnic polarization below the median are classified as having low polarization, whereas countries above the median have high polarization. We then estimate equation (2) in each subsample. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE VIII
Long-Run Effects of Military Spending on Interstate Conflict

Panel A. Direct Conflict

Panel B. Direct or Indirect Conflict

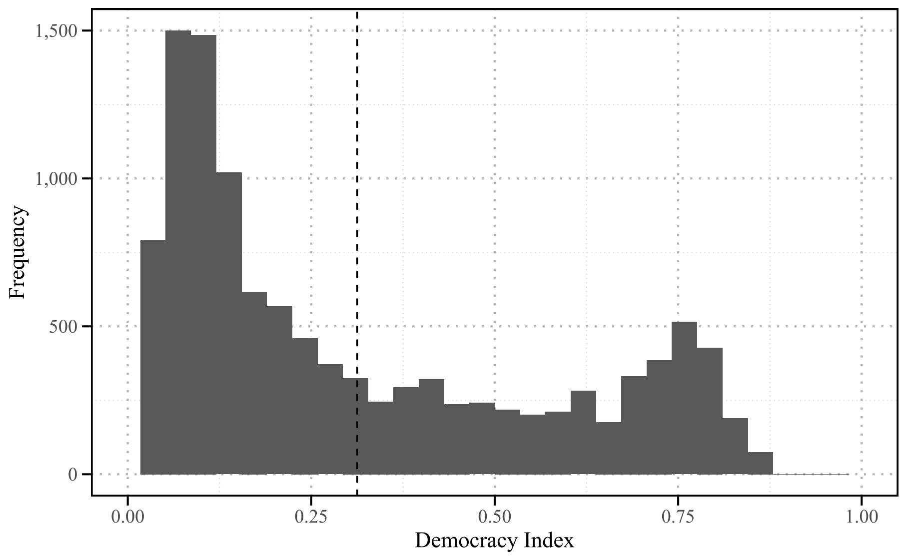


Notes. This figure displays estimates of γ_h from equation (2) using only dyads that engage in direct or indirect conflict at least once in the sample period. We consider two outcome variables. In panel A, the outcome variable takes the value of one if the dyad is involved in a direct conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. In panel A, the outcome variable takes the value of one if the dyad is involved in a direct or indirect conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes dyad fixed effects, year fixed effects, and one lag of the dependent variable. The plotted coefficients correspond to the log of military spending of the dyad, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the dyad level.

Online Appendix

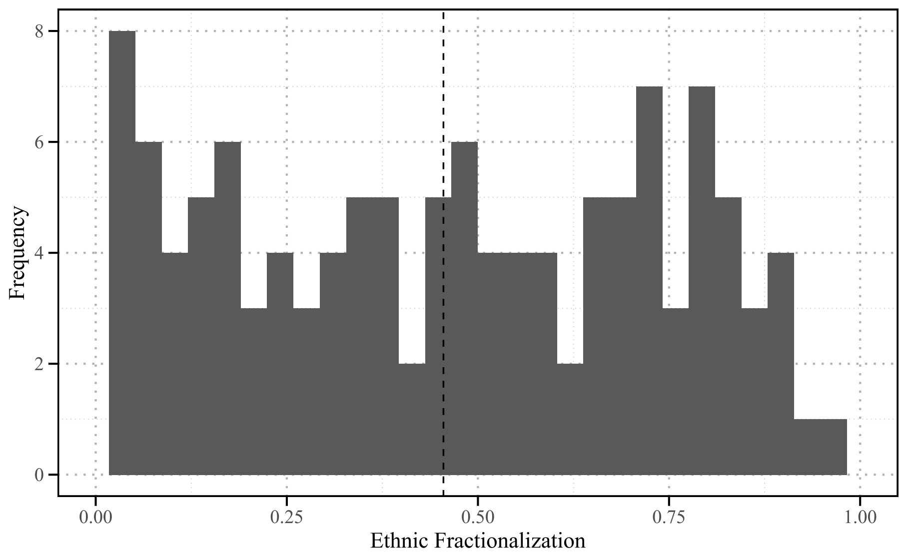
A. Additional Summary Statistics

FIGURE A.1
Distribution of Democracy Index



Notes. This Figure shows the distribution of the democracy index we obtain from V-Dem across all observations, where an observation is a country-year pair. The vertical dashed line represents the mean.

FIGURE A.2
Distribution of Ethnic Fractionalization Index



Notes. This figure shows the distribution of the ethnic fractionalization index computed by Montalvo and Reynal-Querol (2005) across all countries. An observation is a country. The vertical dashed line represents the mean.

B. Additional Results

TABLE B.1
DETERMINANTS OF CONFLICT

	(1)	(2)	(3)	(4)	(5)
Log GDP	0.0872***			0.0251***	-0.0237
	(0.0107)			(0.0359)	(0.0237)
Log GDP per capita	-0.0808***			-0.0235***	-0.0284
	(0.0160)			(0.0050)	(0.0210)
Democracy		0.0305		-0.0026	-0.0071
		(0.0402)		(0.0100)	(0.0143)
Ethnic Polarization		0.0526		0.0140	
		(0.0811)		(0.0203)	
Conflict_{t-1}			0.8023***	0.7440***	0.6329***
			(0.0106)	(0.0153)	(0.0147)
Average of Dep. Var.	0.26	0.26	0.26	0.26	0.26
Country FE					✓
Year FE					✓
Observations	9,189	9,288	12,525	7,694	9,059
R^2	0.104	0.002	0.634	0.600	0.641
Within R^2					0.407

Notes. This table shows the results of estimating a regression of a dependent variable that takes the value of one if the country is involved in at least one conflict and zero if otherwise. We include the logarithm of the real GDP, the logarithm of the real GDP per capita, an indicator variable which takes the value of one if the country is a democracy according to V-Dem, and zero if otherwise, the ethnic polarization measure developed by Montalvo and Reynal-Querol (2005), and a lagged value of the outcome variable. We also include country and year fixed effects. Standard errors are clustered at the country level. $***$, $**$, and $*$ denote significance at the 1%, 5%, and 10% level.

TABLE B.2
DETERMINANTS OF INTERSTATE CONFLICT

	(1)	(2)	(3)	(4)	(5)
Log GDP	0.0199***			0.0107***	0.0070
	(0.0050)			(0.0029)	(0.0136)
Log GDP per capita	-0.0245***			-0.0114**	-0.0274*
	(0.0070)			(0.0043)	(0.0114)
Democracy		0.0005		-0.0002	0.0077
		(0.0131)		(0.0065)	(0.0061)
Ethnic Polarization		0.0091		0.0083	
		(0.0219)		(0.0104)	
Conflict_{t-1}			0.5458***	0.4945***	0.4286***
			(0.0323)	(0.0346)	(0.0358)
Average of Dep. Var.	0.03	0.03	0.03	0.03	0.03
Country FE					✓
Year FE					✓
Observations	9,189	9,288	12,525	7,694	9,059
R^2	0.034	0.000	0.300	0.271	0.368
Within R^2					0.190

Notes. This table shows the results of estimating a regression of a dependent variable that takes the value of one if the country is involved in at least one interstate conflict and zero if otherwise. We include the logarithm of the real GDP, the logarithm of the real GDP per capita, an indicator variable that takes the value of one if the country is a democracy according to V-Dem, and zero if otherwise, the ethnic polarization measure developed by Montalvo and Reynal-Querol (2005), and a lagged value of the outcome variable. We also include country and year fixed effects. Standard errors are clustered at the country level. $***$, $**$, and $*$ denote significant at the 1%, 5%, and 10% level.

TABLE B.3
DETERMINANTS OF INTRASTATE CONFLICT

	(1)	(2)	(3)	(4)	(5)
Log GDP	0.0802***			0.0200***	-0.0165
	(0.0116)			(0.0035)	(0.0213)
Log GDP per capita	-0.0714***			-0.0186***	-0.0198
	(0.0174)			(0.0047)	(0.0191)
Democracy		0.0384		-0.0015	0.0581
		(0.0390)		(0.0086)	(0.0788)
Ethnic Polarization		0.0581			
		(0.0788)			
Conflict_{t-1}			0.8266***	0.7840***	0.6711***
			(0.0101)	(0.0146)	(0.0149)
Average of Dep. Var.	0.24	0.24	0.24	0.24	0.24
Country FE					✓
Year FE					✓
Observations	9,189	9,288	12,525	7,694	9,059
R^2	0.092	0.002	0.671	0.646	0.673
Within R^2					0.451

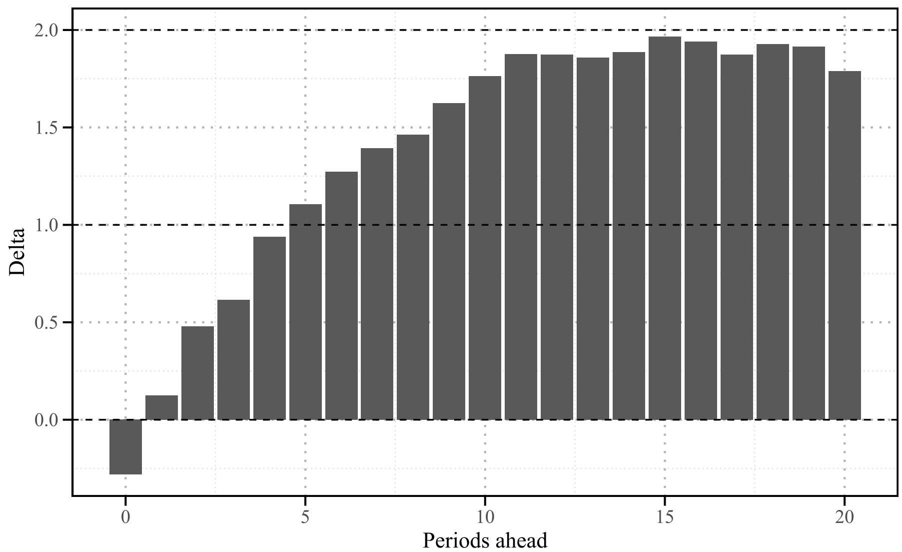
Notes. This table shows the results of estimating a regression of a dependent variable that takes the value of one if the country is involved in at least one intrastate conflict and zero if otherwise. We include the logarithm of the real GDP, the logarithm of the real GDP per capita, an indicator variable that takes the value of one if the country is a democracy according to V-Dem, and zero if otherwise, the ethnic polarization measure developed by Montalvo and Reynal-Querol (2005), and a lagged value of the outcome variable. We also include country and year fixed effects. Standard errors are clustered at the country level. $***$, $**$, and $*$ denote significant at the 1%, 5%, and 10% level.

TABLE B.4
DETERMINANTS OF MILITARY SPENDING

	(1)	(2)	(3)	(4)	(5)
Log GDP	1.042***			1.023***	1.354***
	(0.0413)			(0.0454)	(0.1634)
Log GDP per capita	0.2995***			0.3730***	-0.6476***
	(0.0886)			(0.1094)	(0.1701)
Democracy		1.279***		-0.2761	-0.1126
		(0.3032)		(0.1711)	(0.0859)
Conflict_{t-1}			1.141***	0.2826*	0.1179*
			(0.2350)	(0.1219)	(0.0455)
Country FE					✓
Year FE					✓
Observations	7,137	7,858	7,944	7,060	7,060
R^2	0.808	0.067	0.048	0.810	0.952
Within R^2					0.153

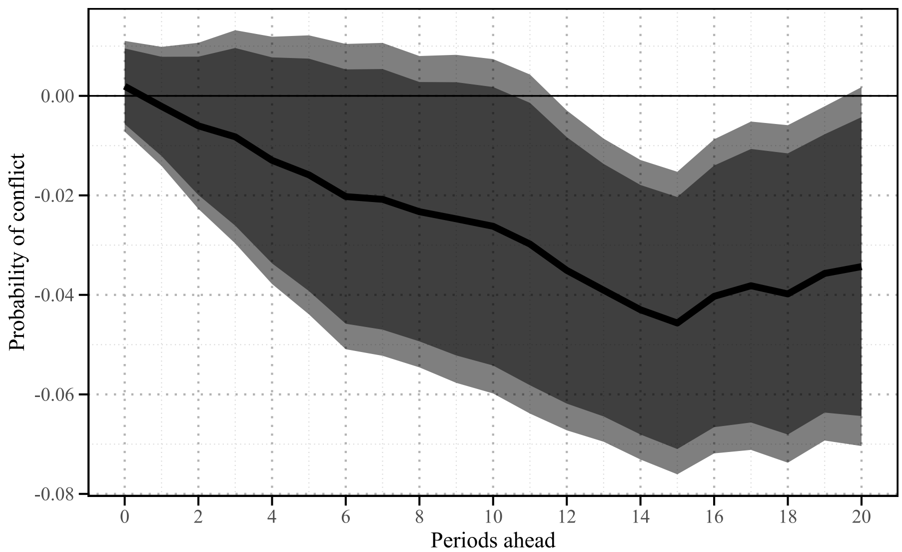
Notes. This table shows the results of estimating a regression where the dependent variable is the logarithm of military spending, in 2015 USD. We include the logarithm of the real GDP, the logarithm of the real GDP per capita, an indicator variable that takes the value of one if the country is a democracy according to V-Dem, and zero if otherwise, and a lagged value of the conflict indicator. We also include country and year fixed effects. Standard errors are clustered at the country level. $***$, $**$, and $*$ denote significant at the 1%, 5%, and 10% level.

FIGURE B.1
Role of Unobserved Covariates



Notes. This figure reports the Oster (2019) δ statistics for our main long-run estimates of the effect of military spending on the likelihood of conflict at various horizons, as presented in Figure IV. The δ statistic measures the strength of selection on unobservables that would be required to reduce the estimated coefficients to zero. It is computed using the coefficients and R^2 values from a restricted model (with no controls or fixed effects) and a fully controlled model (the specification in equation (2)), assuming a maximum attainable R^2 of 1.3 times the observed R^2 in the full model. Values of δ above 1 suggest that selection on unobservables would need to be stronger than selection on unobservables to eliminate the estimated effect. We present the δ for every horizon.

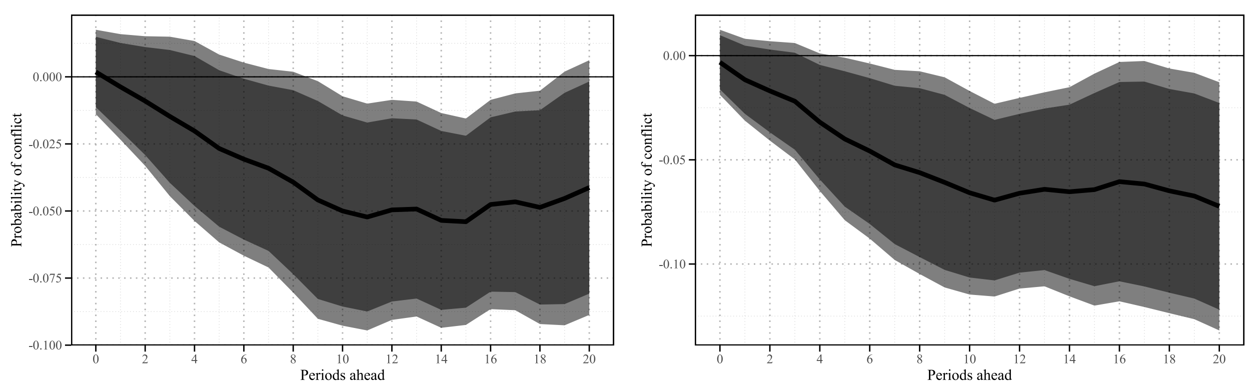
FIGURE B.2
Long-Run Effects of Military Spending on Severe Conflict



Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one severe conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. We define severe conflict as a conflict that has at least 1,000 battle-related deaths in a given year. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

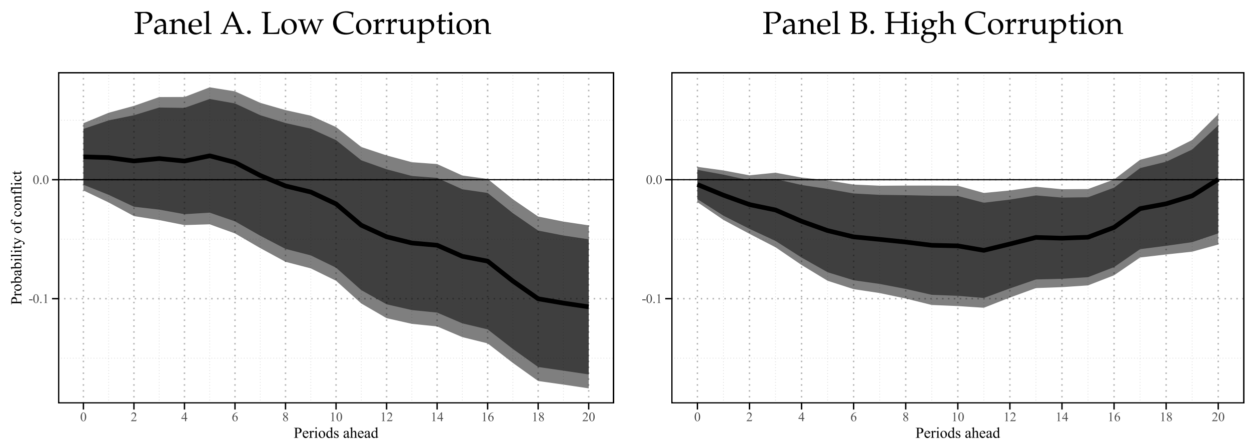
FIGURE B.3
Long-Run Effects of Military Spending on Conflict

Panel A. $\tau = 1$ Panel B. $\tau = 5$



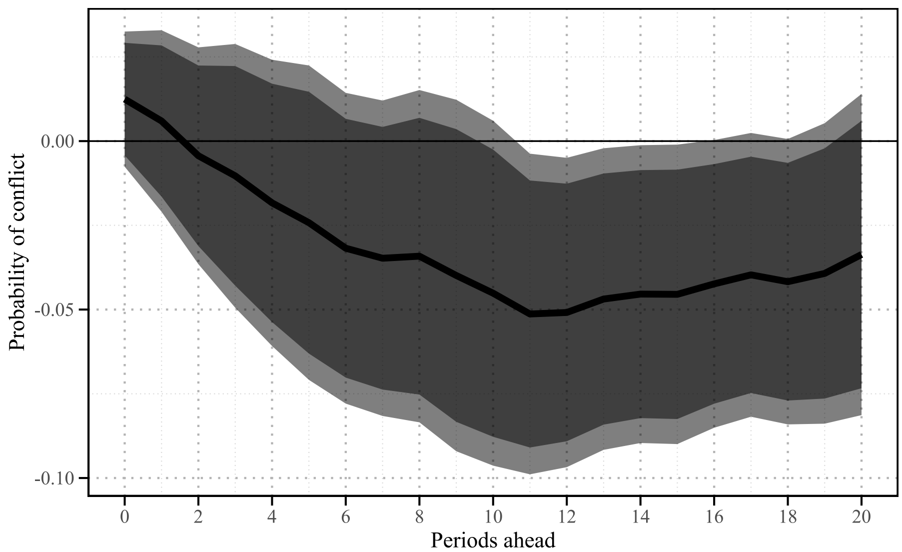
Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. The main regressor is the logarithm of the sum of military spending between period $t - \tau$ and t. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE B.4
Long-Run Effects of Military Spending on Conflict: Role of Political Corruption



Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We divide our sample into two groups: autocracies and democracies. We use V-Dem's measure of political corruption, which is an index between 0 and 1. Countries that, at time $t-1$, are below the cross-sectional median are classified as having low political corruption. Countries above the median are classified as having high political corruption. We then estimate equation (2) in each subsample. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE B.5
Long-Run Effects of Military Spending on Conflict

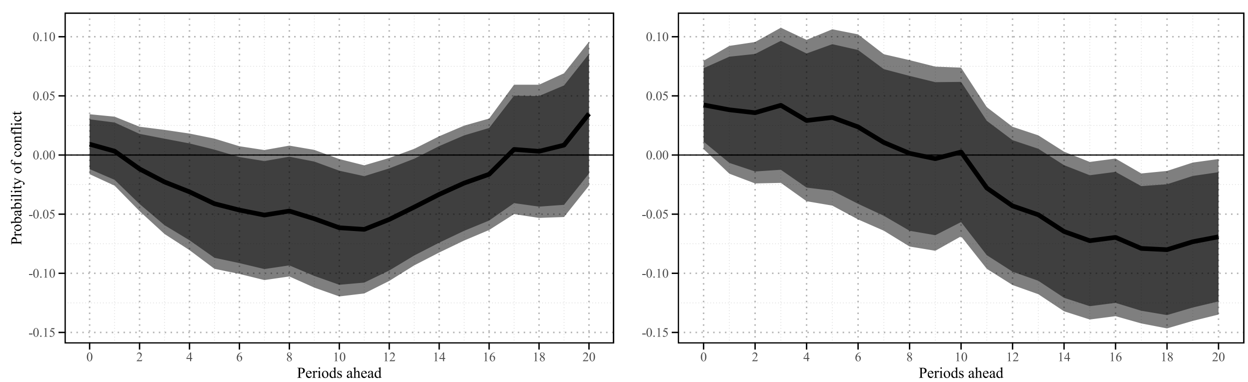


Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We also include the logarithm of real GDP as a control. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE B.6

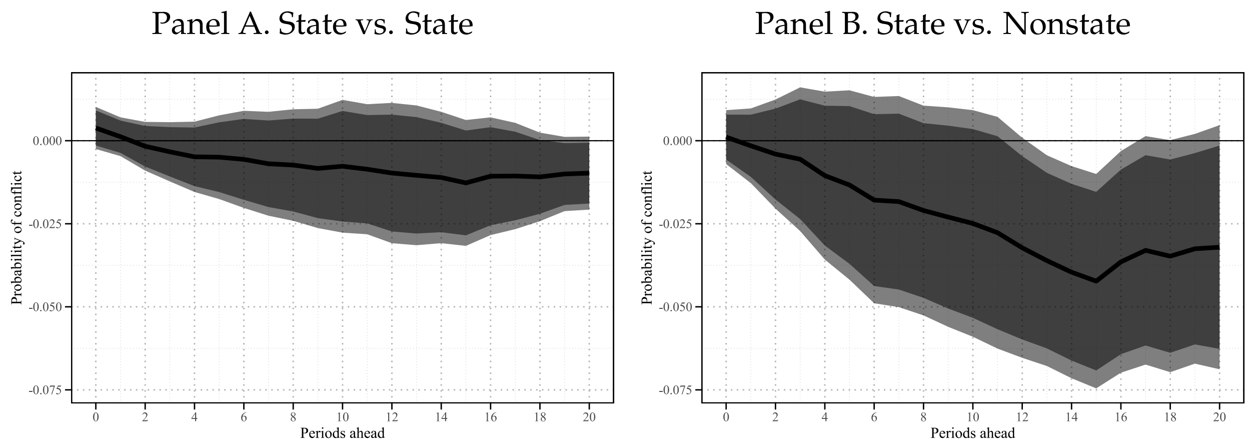
Long-Run Effects of Military Spending on Conflict: Role of Democratic Institutions

Panel A. Autocracies — Panel B. Democracies



Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We also include the logarithm of real GDP as a control. We divide our sample into two groups - autocracies and democracies. We use V-Dem's measure of democracy, which is an index between 0 and 1 and is the average of five indices of democracy. Countries that, at time $t-1$, are below the cross-sectional median are classified as autocracies. Countries above the median are classified as democracies. We then estimate equation (2) in each subsample. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

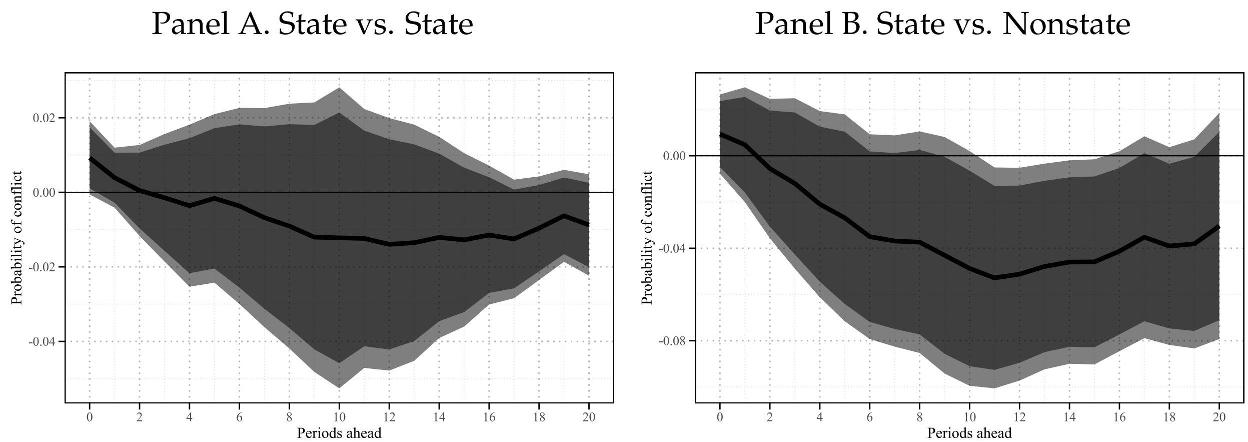
FIGURE B.7
Long-Run Effects of Military Spending on Conflict: Decomposition by Type of Conflict



Notes. This figure displays estimates of γ_h from equation (2). We consider two outcome variables. In panel A, the dependent variable is an indicator equal to one if a country is involved in at least one severe interstate conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. In panel B, the dependent variable is an indicator equal to one if a country is involved in at least one severe intrastate conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We also include the logarithm of real GDP as a control. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE B.8

Long-Run Effects of Military Spending on Conflict: Decomposition by Type of Conflict



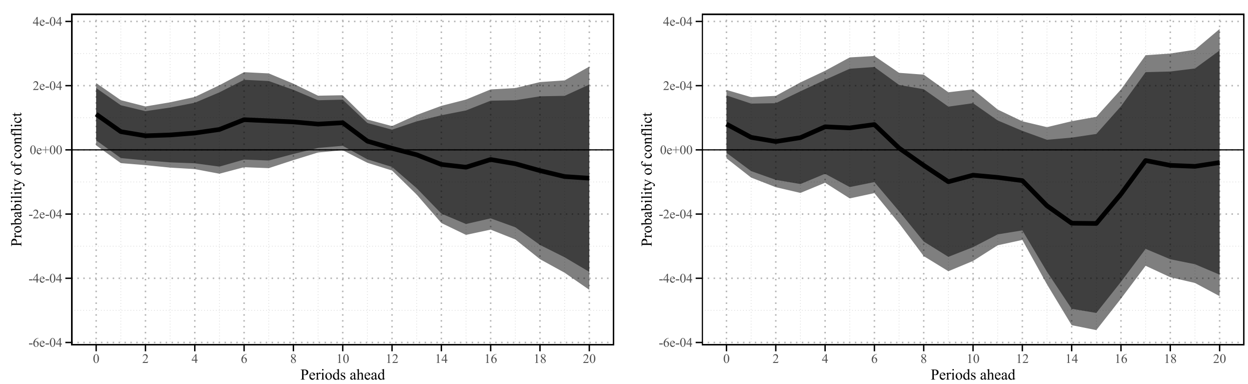
Notes. This figure displays estimates of γ_h from equation (2). We consider two outcome variables. In panel A, the dependent variable is an indicator equal to one if a country is involved in at least one interstate conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. In panel B, the dependent variable is an indicator equal to one if a country is involved in at least one intrastate conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We also include the logarithm of real GDP as a control. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE B.9

Long-Run Effects of Military Spending on Interstate Conflict

Panel A. Direct Conflict

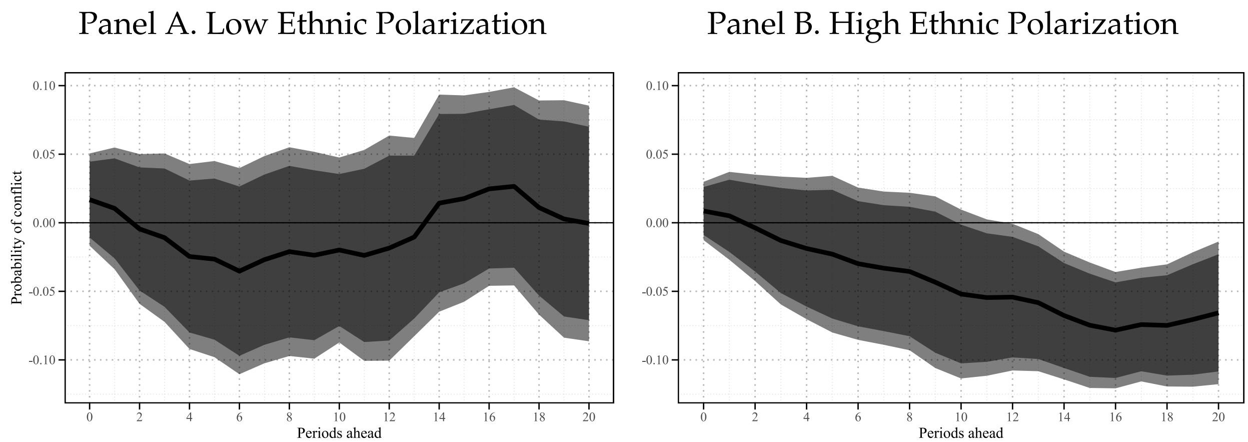
Panel B. Direct or Indirect Conflict



Notes. This figure displays estimates of γ_h from equation (2) using all dyads. We consider two outcome variables. In panel A, the outcome variable takes the value of one if the dyad is involved in a direct conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. In panel A, the outcome variable takes the value of one if the dyad is involved in a direct or indirect conflict $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes dyad fixed effects, year fixed effects, and one lag of the dependent variable. The plotted coefficients correspond to the log of military spending of the dyad, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the dyad level.

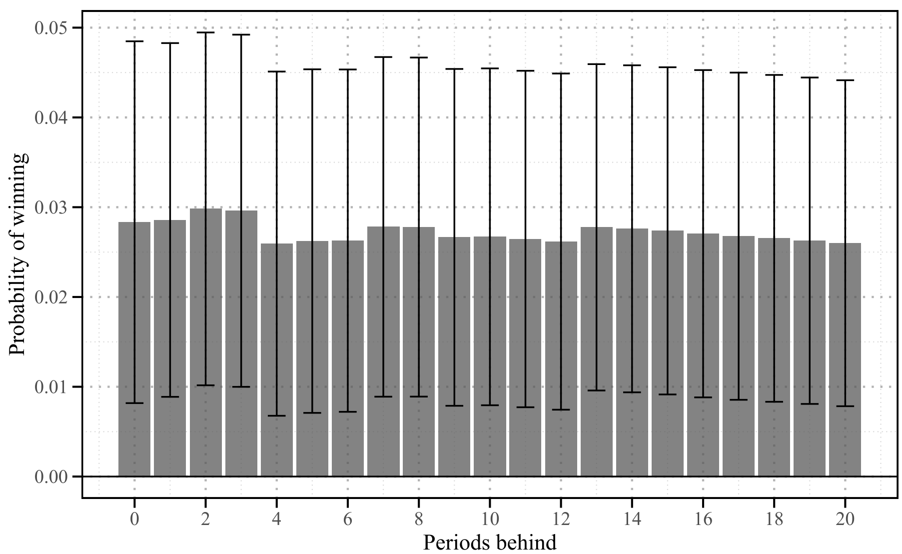
FIGURE B.10

Long-Run Effects of Military Spending on Intrastate Conflict: Role of Ethnic Polarization



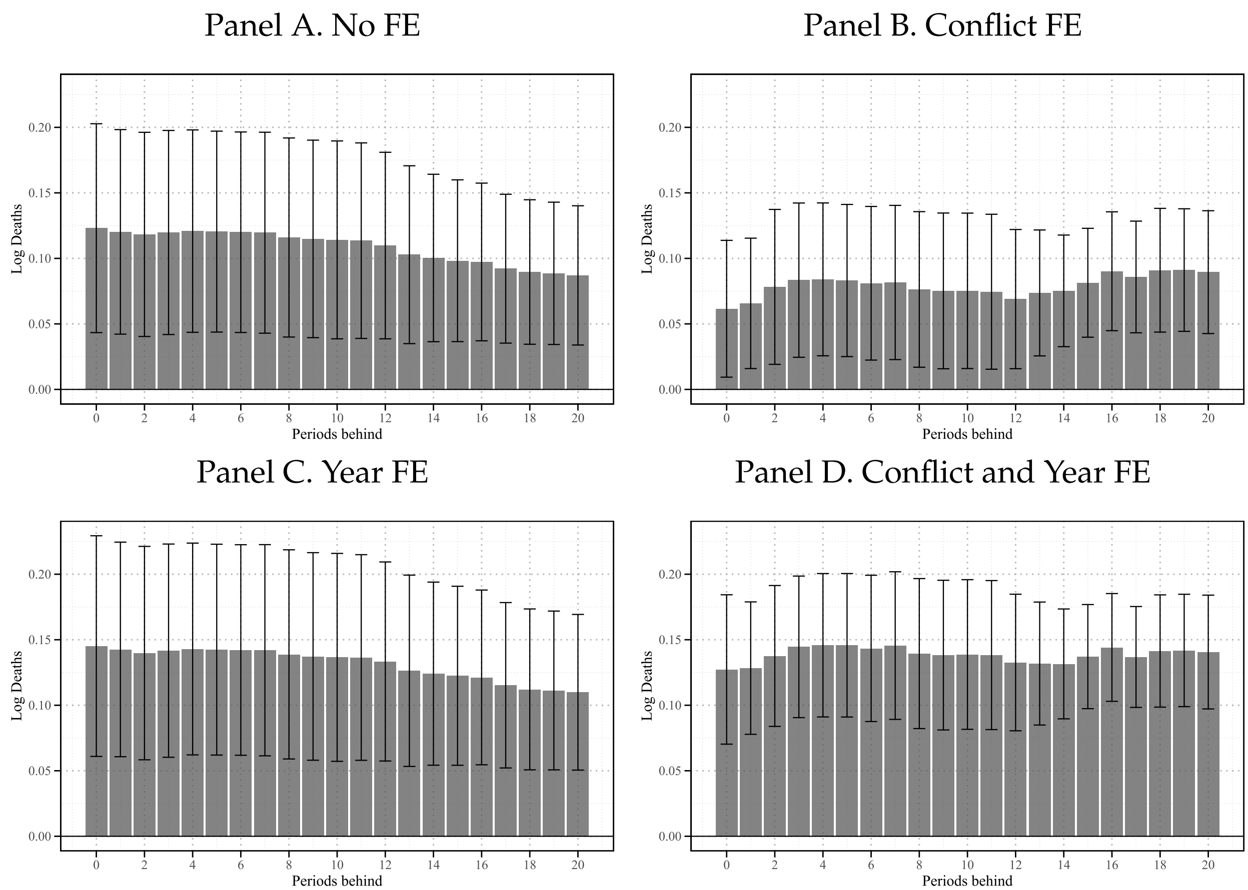
Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict involving one state and one nonstate actor $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We also include the logarithm of real GDP as a control. We divide countries into two groups based on the measure of ethnic polarization developed by Montalvo and Reynal-Querol (2005): countries with ethnic polarization below the median are classified as having low polarization, while countries above the median have high polarization. We then estimate equation (2) in each subsample. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.

FIGURE B.11
Military Spending and Likelihood of Victory



Notes. This figure shows the results of estimating equation (3), which we estimate on our dataset on conflict resolution which is at the conflict-participant level, and where the outcome variable is an indicator variable which takes the value of one if the country is the victor in the conflict, and zero if otherwise. The regressor is the logarithm of total military spending by the country in the h years that precede the conclusion of the conflict. Errors are clustered by conflict. We present the coefficients for several values of h, along with 90% confidence intervals.

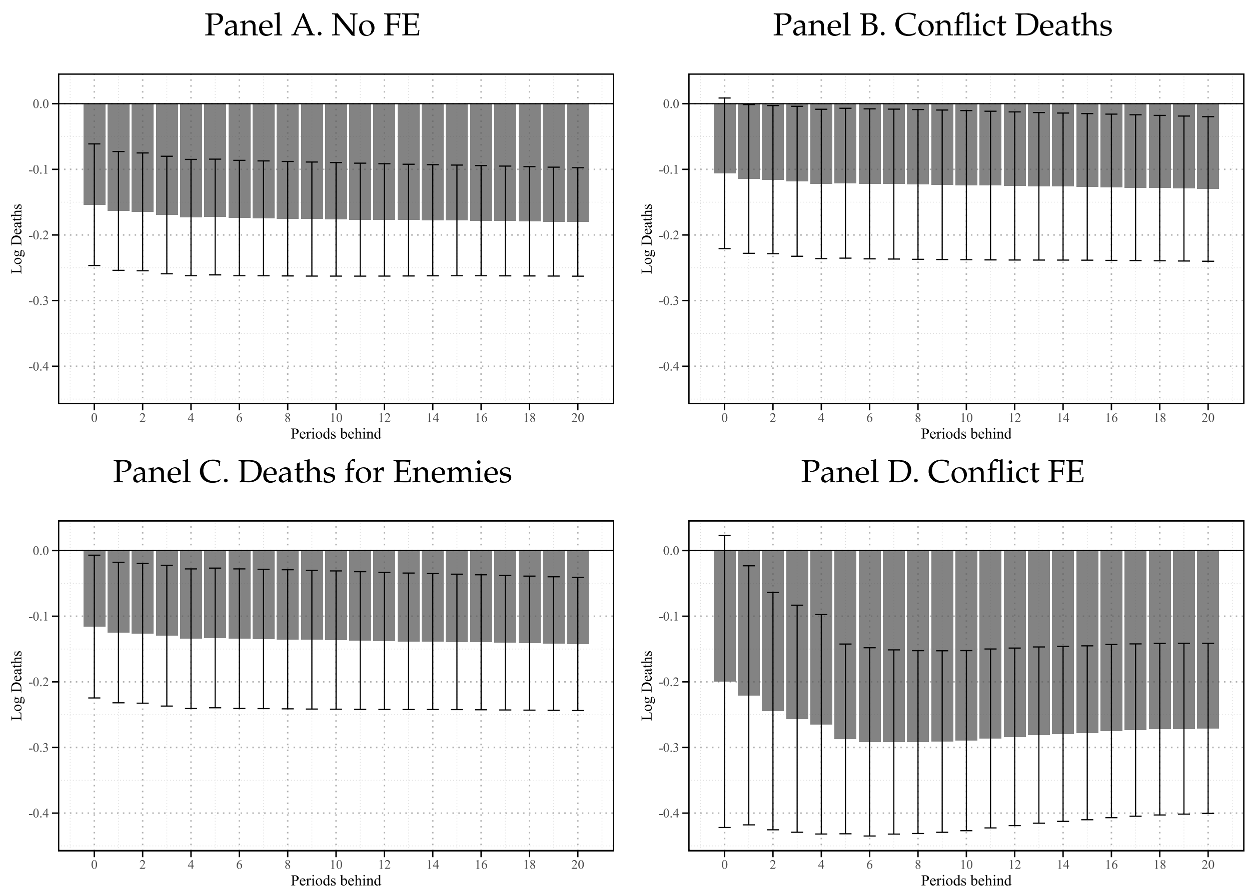
FIGURE B.12
Military Spending and Conflict Casualties



Notes. This figure shows the results of estimating equation (4), which we estimate on a dataset at the conflict-year level, and where the outcome variable is the logarithm of the total number of battle-related deaths in the conflict for all participants. The regressor is the logarithm of military spending by all participants in the conflict in the h years before. We present results for four specifications: (A) no fixed effects, (B) with conflict fixed effects, (C) with year fixed effects, and (D) with conflict and year fixed effects. We present the coefficients for several values of h, along with 90% confidence intervals. Standard errors are clustered at the country level.

FIGURE B.13

Military Spending and Country-Level Casualties

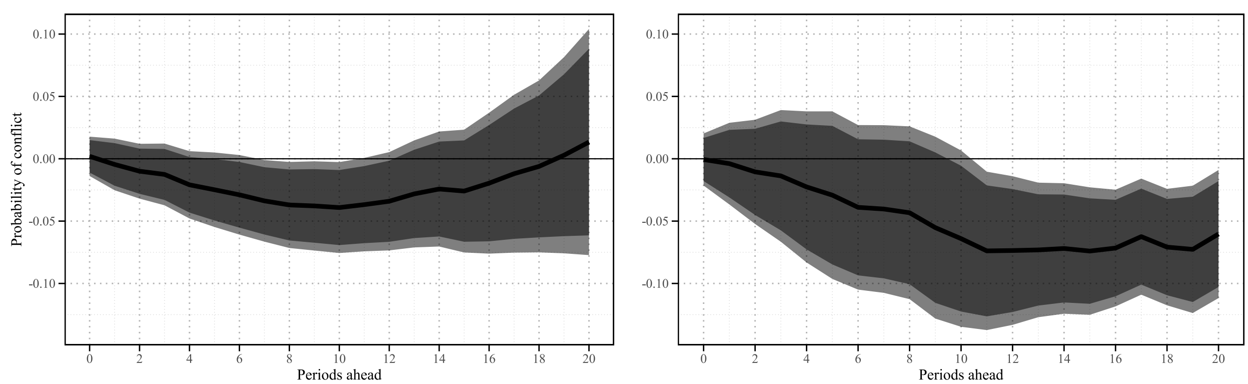


Notes. This figure shows the results of estimating equation (5), which we estimate on a dataset at the conflict-participant level, and where the outcome variable is the logarithm of battle-related deaths for each country-conflict pair. The regressor is the logarithm of total military spending by the country in the h years that precede the conclusion of the conflict. We consider four specifications: (A) no controls or fixed effects, (B) including the logarithm of total deaths in the conflict as a control, (C) including the logarithm of total deaths for enemies, and (D) including conflict fixed effects. We present the coefficients for several values of h, along with 90% confidence intervals. Standard errors are clustered at the country level.

FIGURE B.14

Long-Run Effects of Military Spending on Intrastate Conflict: Role of Ethnic Fractionalization

Panel A. Low Ethnic Fractionalization Panel B. High Ethnic Fractionalization



Notes. This figure displays estimates of γ_h from equation (2), where the dependent variable is an indicator equal to one if a country is involved in at least one conflict involving one state and one nonstate actor $h = 0, \ldots, 20$ years ahead, and zero if otherwise. Each specification includes country fixed effects, year fixed effects, and one lag of the dependent variable. We divide countries into two groups based on the measure of ethnic fractionalization developed by Alesina et al. (2003): countries with ethnic fractionalization below the median are classified as having low fractionalization, while countries above the median have high fractionalization. We then estimate equation (2) in each subsample. The plotted coefficients correspond to the log of military spending, with 95% (light gray) and 90% (dark gray) confidence intervals. Standard errors are clustered at the country level.