

One-Sided Violence in Two-Sided Wars

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Abstract

When and where do two-sided armed conflicts become one-sided? In contested territories, one combatant often sustains coercive violence persistently and unilaterally while its rival restrains. This asymmetry can manifest as wartime terrorism, repression, or other patterns of one-sided violence during conflict. We argue that two conditions produce such behavior: targeting selectivity and outside options. A combatant sustains unilateral escalation when its violence is selective enough to extract civilian compliance, or when it believes it can survive without civilian cooperation (e.g., due to external support or natural resource rents). We develop this argument theoretically and derive a scalar measure of coercive asymmetry — a combatant’s behavioral commitment to sustained unilateral punishment — that researchers can estimate from any dyadic conflict panel. We validate our claims with cross-national data spanning dozens of modern armed conflicts in 50 countries and with declassified Soviet secret police records on counterinsurgency in western Ukraine (1944–1950).

Keywords: one-sided violence, coercive asymmetry, civilian targeting, outside options

Armed conflicts are not always as two-sided as their labels suggest. In wars where both sides field organized forces, one combatant often escalates persistently and unilaterally while its rival restrains. The phenomenon takes many forms: wartime state repression (Schubiger, 2021), terrorism in civil war (Stanton, 2016), indiscriminate counterinsurgency (Lyll, 2009), mass atrocities against civilians (Eck and Hultman, 2007). This asymmetry is among the most common structural features of armed conflict, with direct bearing on how wars unfold (Leites and Wolff, Jr., 1970; Mack, 1975; Arreguin-Toft, 2001), who pays their costs (Davenport et al., 2019; Anderton and Brauer, 2021), and the line between two-sided warfare and one-sided terror (Besley and Persson, 2009; Rohner et al., 2025).

The scope of our study extends to any setting where two organized armed actors simultaneously claim authority over the same civilian population and neither holds a monopoly on the legitimate use of force. This category encompasses insurgencies, civil wars, military occupations, and foreign interventions, and excludes settings where one side has effectively consolidated control.¹ We ask why, when two sides compete over the same population, one finds sustained unilateral violence worthwhile and the other does not.

Existing literatures illuminate pieces of the puzzle, but no existing theory directly explains the dyadic asymmetry. The rationalist bargaining tradition can generate inferences about coercive persistence (e.g., Slantchev, 2003; Powell, 2012), but treats the outbreak or continuation of fighting — not the allocation of coercive effort between two rivals — as its core explananda.² Research on the dynamics of civil conflict (e.g., Kalyvas, 2006; Sale-

¹This distinguishes our subject from state repression in contexts where no organized armed challenger contests civilian loyalties (e.g., Davenport, 1995; Ritter and Conrad, 2016; Bueno de Mesquita et al., 2024). There, a single actor monopolizes coercive capacity, with no dyadic allocation of violence.

²Asymmetric escalation could be an equilibrium outcome in bargaining models if one actor possesses private information about its resolve or capabilities (Fearon, 1995), if a shifting power distribution creates commitment problems (Powell, 2012), or if one side’s private beliefs about relative strength take longer to update (Slantchev, 2003; Wolford et al., 2011). However, predictions about asymmetric persistence are byproducts of these models, not primary objects of explanation. The civilian population, furthermore, is not an optimizing actor in their equilibrium logic.

hyan et al., 2014; Blair, 2023) has identified key mechanisms that bear on the puzzle (e.g., information, outside options), but has not theorized how cross-combatant asymmetries in these capacities generate one-sided violence in otherwise dyadic contests.³ A third strand of research, in political economy (e.g., Besley and Persson, 2009, 2011), has shown that the same structural factors can drive one- and two-sided violence, but explains selection into violence regimes rather than combatant behavior within them.⁴

We argue that two conditions produce persistent coercive asymmetry: targeting selectivity and outside options. A combatant sustains unilateral punishment when its violence is selective enough to extract civilian compliance, or when it can afford to forgo that compliance entirely. How a combatant delivers punishment determines whether one-sided violence succeeds. When a combatant strikes selectively, targeting enemy collaborators and sparing bystanders, civilians can protect themselves by cooperating with the punishing side. When a combatant strikes indiscriminately, civilians absorb harm regardless of their loyalties, and cooperation offers no protection. Indiscriminate violence can still degrade the opponent’s support base, and combatants with robust outside options (e.g., external recruitment, resource rents, administrative depth) can afford the backlash that indiscriminate punishment provokes. A combatant turns to one-sided violence because it expects escalation to produce submission, or because it does not care if it produces defiance.

We formalize this logic as a stochastic game between two armed rivals competing for a

³The information-economy literature shows that selective coercion requires distinguishing combatants from noncombatants (Kalyvas, 2006) via ethnic ties (Lyall, 2010), service provision (Berman et al., 2011), organizational capacity (Hoover Green, 2016), or surveillance (Gohdes, 2020). The outside-options literature shows that access to lootable resources (Wood, 2010; Salehyan et al., 2014) or border sanctuaries (Blair, 2023) severs the disciplinary feedback loop that otherwise constrains violence. Neither literature examines what happens when two rivals facing the same population differ in these capacities.

⁴Besley and Persson (2009, 2011) model peace, (one-sided) repression, and (two-sided) civil war as ordered equilibrium outcomes of a common rent-seeking game. Rohner et al. (2025) classify countries into structural ideal-types to predict which form of violence predominates. This framework treats civilians as a passive labor pool with no strategic choices, and focuses on whether each party mobilizes, not how it allocates coercive effort once conflict has begun. Models of strategic interaction between perpetrators and civilian targets remain rare (Anderton and Brauer, 2021).

civilian population. From this model, we derive a scalar measure of *coercive asymmetry*, a combatant’s behavioral commitment to sustained one-sided violence. We show how to estimate this measure from any dyadic conflict panel data. Our measure goes beyond existing empirical efforts by capturing the dynamic, dyadic nature of coercive interaction.

Our empirical analysis proceeds in two stages. First, we estimate coercive asymmetry across a cross-national sample of armed conflicts, spanning civil wars, counterinsurgencies, occupations, and expeditionary wars. We show that our theoretical predictions hold across conflict types and are not artifacts of any single case. Second, we validate these patterns with micro-level data from a single, well-documented conflict — the Soviet campaign against the Ukrainian Insurgent Army (1944–1950) — where archival completeness reduces the reporting and misclassification concerns that affect media-generated event data.

We make three contributions to the literature on armed conflict. First, we derive the conditions under which two-sided conflicts produce persistent one-sided violence, by either armed actor. Second, we introduce a scalar measure of coercive asymmetry that researchers can estimate from standard dyadic conflict data, identifying which conflicts are relatively symmetric, which are asymmetric, and in whose direction. Third, we show that the same conflict can generate markedly different coercive regimes across space and time, tracking changes in the underlying strategic environment. Both empirical contributions rest on a two-stage design that pairs cross-national breadth with archival validation.

1 Theory of coercive asymmetry

Our theory applies to armed conflicts in which two organized combatants compete for the loyalty of a civilian population that neither side fully controls (Leites and Wolff, Jr., 1970; Kalyvas, 2006). The combatants may be states or non-state actors; the conflict may be an insurgency, civil war, military occupation, or anti-colonial uprising. The framework

requires contested sovereignty, where neither combatant holds a monopoly on legitimacy or violence in the contested area and civilians face real costs from defying either side. We call the two combatants Side 1 (incumbent; the actor exercising greater de facto administrative control at the outset) and Side 2 (challenger). We develop the argument qualitatively here, with footnotes connecting each claim to its formal counterpart in Appendix A1.

The conflict involves three sets of actors: a civilian population and two combatants (Sides 1 and 2). Each combatant’s campaign depends to some degree on civilian cooperation (e.g., intelligence, shelter, labor, material support) although the degree of dependence varies. To secure that cooperation, each combatant chooses whether to restrain or escalate punishment.⁵ The expected mechanism is coercive. By raising the cost of supporting the opponent, a combatant hopes to redirect civilian cooperation toward itself. Punishment reaches civilians through two channels. One is selective: the combatant identifies and targets supporters of the opposing side, making cooperation with that side more costly. The other is indiscriminate: violence inflicts collateral harm on nearby civilians regardless of their allegiances, raising the cost of being present rather than the cost of taking sides.⁶ Indiscriminate violence also hurts some enemy supporters, albeit by chance, at high cost to neutral civilians, and without the deterrent effect of selective violence.

Some combatants are less dependent on local support than others. Combatants with robust outside options (e.g., external recruits, resource rents, materiel sourced outside the contested locality) can absorb the cooperation loss that indiscriminate violence provokes.⁷ Combatants with fewer outside options face stronger incentives to keep civilians on side.

Civilians, in turn, are not a homogeneous mass. Each civilian occupies a distinct exposure profile. Some face greater risk from selective targeting, because their location,

⁵We denote punishment levels L (low) and H (high). Each combatant $k \in \{1, 2\}$ selects $\rho_k \in \{L, H\}$.

⁶Formally, combatant k hunts identified enemy cooperators at rate $\lambda_k \rho_k$, where $\lambda_k \in (0, 1)$ is selectivity and ρ_k is punishment level; collateral harm accrues at rate $(1 - \lambda_k) \rho_k$.

⁷We capture outside options with the parameter $c_k \geq 0$.

occupation, or social network marks them as potential collaborators. Others face greater risk from indiscriminate fire, perhaps because they live near infrastructure combatants routinely strike or lack the means to evacuate.⁸ Combatants know the population's general composition but not which specific civilians belong to which exposure category.

How civilians respond to punishment depends on how well that punishment discriminates.⁹ When Side 1's violence is selective enough to punish Side 2's collaborators, while sparing bystanders and loyalists, the relative costs of cooperating with Side 2 rise, and civilians protect themselves by cooperating with Side 1: submission. When Side 1's violence is too blunt to spare bystanders or loyalists, cooperation with the punishing side no longer offers protection, and civilians instead cooperate with Side 2: defiance.

Combatants' choices to escalate or restrain hinge on whether their targeting technologies are selective enough to make escalation pay.¹⁰ Each combatant has a selectivity threshold: a minimum share of violence that must befall enemy collaborators and not bystanders,

⁸We index civilian j 's exposure type by $\theta_j \in (0, 1)$: low θ_j denotes greater vulnerability to selective targeting, high θ_j to collateral damage. Combatants observe distribution $F(\theta | x_i)$, where x_i are observable locality characteristics, but cannot assign types to individuals before punishing (see Assumption 1).

⁹Civilian j chooses $e_j^* \in [0, 1]$, the share of cooperation directed toward Side 1, to maximize

$$U_j(e_j) = -[\theta_j(1 - \lambda_1)\rho_1 + (1 - \theta_j)\lambda_2\rho_2]e_j - [\theta_j(1 - \lambda_2)\rho_2 + (1 - \theta_j)\lambda_1\rho_1](1 - e_j) - \frac{\gamma}{2}(e_j - \frac{1}{2})^2, \quad (1)$$

where $\gamma > 0$ governs the curvature of civilian utility (equivalently, the cost of deviating from perfect neutrality $e_j = 1/2$). The first-order condition gives $\partial e_j^* / \partial \rho_1 = (\lambda_1 - \theta_j) / \gamma$, which is positive (submission) when $\lambda_1 > \theta_j$ and negative (defiance) when $\lambda_1 < \theta_j$ (see Proposition 1).

¹⁰Combatant k chooses $\rho_k \in \{L, H\}$ to maximize

$$U_k(\rho_k, \rho_{-k}) = b \cdot \mathbb{E}[e_k^*(\rho_k, \rho_{-k}, \theta_j) | x_i] + c_k(1 - \lambda_k)\rho_k - \kappa_k(\rho_k), \quad (2)$$

where $b > 0$ is the value of a unit of civilian cooperation, e_k^* is the share of cooperation that k receives, $c_k \geq 0$ is k 's outside option, and punishment cost $\kappa_k(\cdot)$ satisfies $\kappa_k(L) = 0$ and $\kappa_k(H) > 0$. Taking the difference $U_k(H, \cdot) - U_k(L, \cdot)$ and signing it yields a threshold. k strictly prefers escalation ($\rho_k = H$) when

$$\lambda_k > \lambda_k^\dagger \equiv \bar{\theta}_i + \gamma \frac{\kappa_k(H) - c_k(1 - \bar{\theta}_i)\Delta}{(b - c_k\gamma)\Delta}, \quad (3)$$

where $\bar{\theta}_i = \mathbb{E}[\theta_j | x_i]$ is mean civilian type in locality i , and $\Delta > 0$ is the punishment increment (see Theorem 1). The threshold falls as c_k rises: outside options expand the region where escalation dominates.

in order to deter cooperation with the enemy. A combatant whose selectivity meets or exceeds this threshold will always prefer to escalate, no matter what the opponent does. A less selective combatant will prefer to restrain, for the same reason.¹¹

When both sides fall short of their selectivity thresholds, mutual restraint follows. When both clear them, mutual escalation results. The more revealing cases are the asymmetric ones, where one side escalates and the other restrains: one-sided violence. These asymmetries arise not because either side miscalculates, but because they face the same population with different technologies and reach different conclusions about whether escalation pays.

Armed conflicts are not static. Front lines move, populations change, surveillance capacity evolves. As these conditions shift, so does each side's calculus about whether escalation pays. At any point in time and space, the two combatants' behavior falls into one of four patterns: mutual restraint, mutual escalation, or unilateral escalation by Side 1 or 2.¹² Which of these patterns will emerge in the future depends on which one currently prevails: the chances of mutual restraint tomorrow may be different for localities already experiencing mutual restraint as opposed to one-sided violence or mutual escalation.¹³

¹¹Aggregating over $F(\theta|x_i)$ shows that escalation attracts or repels cooperation depending only on λ_k relative to θ_i . The outside-options term involves only k 's own parameters. These properties make the game dominance-solvable with a unique Nash equilibrium (Theorem 1).

¹²We model these dynamics as a stochastic game in which combatants re-optimize punishment as the strategic environment shifts rather than executing a fixed plan. Within any period, each side acts independently of the other (Theorem 1), but across periods, the opponent's most recent action signals how conditions have changed. Combatants condition on this signal to update their assessment of the environment, not to react strategically within a period. This time-varying structure gives the model empirical traction: observed violence patterns carry recoverable information about which state prevails and why.

¹³The observed sequence of joint punishment profiles forms a first-order Markov chain, collecting transition probabilities in a 4×4 matrix $\mathbf{M} = [m_{a'a}]$, where $m_{a'a} = P(a' | a)$ and $\mathcal{A} = \{LL, LH, HL, HH\}$:

$$\mathbf{M} = \begin{matrix} & \begin{matrix} LL & LH & HL & HH \end{matrix} \\ \begin{matrix} LL \\ LH \\ HL \\ HH \end{matrix} & \begin{pmatrix} m_{LL,LL} & m_{LL,LH} & m_{LL,HL} & m_{LL,HH} \\ m_{LH,LL} & m_{LH,LH} & m_{LH,HL} & m_{LH,HH} \\ m_{HL,LL} & m_{HL,LH} & m_{HL,HL} & m_{HL,HH} \\ m_{HH,LL} & m_{HH,LH} & m_{HH,HL} & m_{HH,HH} \end{pmatrix} \end{matrix} \quad (4)$$

Each combatant's behavior reduces to two probabilities: the chance it escalates when the opponent restrained last period, and the chance it restrains when the opponent escalated last period. Together, these determine how durable each form of one-sided violence is. We define *coercive asymmetry* (ω) as how much more persistent one-sided violence by Side 1 is than one-sided violence by Side 2.¹⁴ When ω is positive, Side 1 escalates persistently while Side 2 restrains. When ω is negative, Side 2 escalates while Side 1 restrains. When ω is zero, neither form of one-sided violence dominates. Positive ω does not imply that Side 2 uses no force, only that it is less likely to initiate or sustain unilateral escalation.

So where and when should we expect one-sided violence, and by which side? Our theory yields several testable predictions about how ω varies across localities and conflicts.¹⁵ Where Side 1's violence is more selective than Side 2's, escalation generates a larger net gain in civilian support for Side 1, and Side 1 becomes more likely to use one-sided violence; ω rises. Where Side 1 has better outside options, it escalates to degrade the opponent's base through attrition, even if doing so alienates the population; again, ω rises. Estimating ω and how it varies with observable proxies for these parameters is our main empirical test.

We reduce \mathbf{M} 's 12 free parameters to four by restricting each combatant to condition its current action only on the opponent's *previous* action (i.e., adaptation to observed enemy behavior, [Smith et al. 2000](#)). For each k , let $p_k \equiv P(\rho_{kt} = L \mid \rho_{-k,t-1} = L)$ and $q_k \equiv P(\rho_{kt} = L \mid \rho_{-k,t-1} = H)$. Under this restriction, current actions are conditionally independent and all $m_{a'a}$ reduce to products of p_k and q_k (Proposition 2):

$$\mathbf{M} = \begin{matrix} & \begin{matrix} LL & LH & HL & HH \end{matrix} \\ \begin{matrix} LL \\ LH \\ HL \\ HH \end{matrix} & \begin{pmatrix} p_1 p_2 & q_1 p_2 & p_1 q_2 & q_1 q_2 \\ p_1(1-p_2) & q_1(1-p_2) & p_1(1-q_2) & q_1(1-q_2) \\ (1-p_1)p_2 & (1-q_1)p_2 & (1-p_1)q_2 & (1-q_1)q_2 \\ (1-p_1)(1-p_2) & (1-q_1)(1-p_2) & (1-p_1)(1-q_2) & (1-q_1)(1-q_2) \end{pmatrix} \end{matrix} \quad (5)$$

¹⁴Formally, $\omega_1 \equiv m_{HL,HL} - m_{LH,LH} = (1-p_1)q_2 - q_1(1-p_2)$ (Proposition 2). ω_2 reverses the roles of the two sides. For brevity, we drop the subscript and write $\omega \equiv \omega_1$ unless we state otherwise.

¹⁵These follow from Corollary 1 and comparative statics on λ_k^\dagger .

2 Estimation

We empirically estimate coercive asymmetry in two steps. First, we model the joint sequence of combatant decisions using dyadic conflict data. Second, we recover predicted transition probabilities from that model, and assemble them into an estimate of $\hat{\omega}$.

We observe the two combatants' decisions to escalate or restrain as pairs of binary outcomes across localities and time periods.¹⁶ We assume that these decisions are potentially correlated, in that unobserved local and time-specific shocks affecting both sides (e.g., unit rotations, agricultural cycles, population flows) might simultaneously push both combatants toward escalation or restraint. If we ignore this dependence and estimate each combatant's behavior in isolation, we could misstate the uncertainty around $\hat{\omega}$.¹⁷

In our empirical model, each combatant's propensity to escalate depends on three inputs: the opponent's most recent action, its own most recent action, and an interaction between the two.¹⁸ The opponent's past action raises or lowers the baseline propensity to escalate directly. It also moderates how much each side's own past behavior carries forward. An opponent's escalation can either reinforce or dampen a combatant's own momentum.¹⁹

¹⁶We index combatants $k = 1, 2$, localities i , and periods t . We define $Y_{k,it} = 1$ if k escalates and $Y_{k,it} = 0$ otherwise. Observed outcomes emerge from latent indices via $Y_{k,it} = \mathbf{1}[Y_{k,it}^* \geq 0]$. Violence that available measurement instruments cannot detect goes unrecorded. We treat this observation mechanism as the boundary between restraint and escalation: the former is empirically invisible, the latter is not.

¹⁷We allow for this by modeling the two latent indices as jointly normal:

$$\begin{bmatrix} Y_{1,it}^* \\ Y_{2,it}^* \end{bmatrix} \sim N_2 \left(\begin{bmatrix} \mu_{1,it} \\ \mu_{2,it} \end{bmatrix}, \begin{bmatrix} 1 & r \\ r & 1 \end{bmatrix} \right),$$

where r is a scalar cross-equation correlation parameter. We test whether the dependence is statistically negligible with likelihood ratio tests (LRT) (Appendix A2).

¹⁸The estimating equations take the form, suppressing locality subscript i ,

$$\begin{bmatrix} \mu_{1,t} \\ \mu_{2,t} \end{bmatrix} = \begin{bmatrix} y_{2,t-1}\zeta_1 + y_{1,t-1}(\alpha_1 + y_{2,t-1}\phi_1) + \mathbf{x}_{1,t}\beta_1 \\ y_{1,t-1}\zeta_2 + y_{2,t-1}(\alpha_2 + y_{1,t-1}\phi_2) + \mathbf{x}_{2,t}\beta_2 \end{bmatrix}, \quad (6)$$

where $y_{k,t-1}$ is combatant k 's lagged outcome, $\mathbf{x}_{k,t}$ are vectors of covariates including pre-conflict locality attributes, regional, year, and month fixed effects; $\alpha_k, \beta_k, \zeta_k, \phi_k$ are regression coefficients.

¹⁹In eq. (6), the effect of $y_{1,t-1}$ on μ_1 is α_1 if Side 2 restrained in $t-1$, and $\alpha_1 + \phi_1$ if it escalated.

We estimate this system by bivariate probit, controlling for pre-conflict local attributes, with corrections for unobserved locality-specific and period-specific confounders.²⁰

We recover $\hat{\omega}$ from the fitted model’s predicted transition probabilities. For each combatant, we compute two sets of predicted probabilities: the probability of escalation after the opponent restrained last period, and the probability of restraint after the opponent escalated.²¹ Our estimate $\hat{\omega}$ is the difference between the probability that Side 1 sustains the former configuration (unilateral escalation) and the probability that Side 1 sustains the latter (unilateral restraint).²² We obtain confidence intervals by parametric bootstrap.²³

To test the comparative statics predictions, we apply the same procedure under counterfactual covariate values, shifting each variable from its observed minimum to its maximum while holding all others at their median or modal values. We report the resulting contrast $\Delta\hat{\omega}$ with a 95% bootstrap confidence interval. A contrast whose sign matches the theoretical prediction confirms the comparative static. We apply this procedure in both empirical applications below, differing only in data sources and covariate specifications.

3 Application I: Cross-National Evidence

We estimate coercive asymmetry across a broad cross-national sample to assess whether the theoretical predictions hold beyond any single conflict type, region, or data source.

²⁰To partial out time-invariant heterogeneity and period-specific shocks, we augment eq. (6) with locality- and period-level time-means of each side’s lagged behavior (Mundlak, 1978; Wooldridge, 2010). We use Mundlak-corrected specifications for general inference about $\hat{\omega}$, and the base interactive specification for comparative statics tests on time-invariant covariates. We use LRT’s to select between the fully interactive model and restricted variants (e.g., independent: $\zeta_k = \phi_k = 0$; additive: $\phi_k = 0$; Appendix A2).

²¹For combatant k : $(1 - \hat{p}_k) \equiv \hat{P}(Y_k = 1 \mid y_{-k,t-1} = 0)$, the predicted probability of escalation when the opponent restrained, and $\hat{q}_k \equiv \hat{P}(Y_k = 0 \mid y_{-k,t-1} = 1)$, the probability of restraint when the opponent escalated. The diagonal entries of transition matrix $\hat{\mathbf{M}}$ are $\hat{m}_{HL,HL} = (1 - \hat{p}_1)\hat{q}_2$, $\hat{m}_{LH,LH} = \hat{q}_1(1 - \hat{p}_2)$.

²²Formally, $\hat{\omega} \equiv \hat{m}_{HL,HL} - \hat{m}_{LH,LH} = (1 - \hat{p}_1)\hat{q}_2 - \hat{q}_1(1 - \hat{p}_2)$.

²³Since $\hat{\omega}$ combines predictions from both equations in (6), propagating uncertainty through the system requires joint simulation. We draw from the joint coefficient distribution and recompute $\hat{\omega}$ at each draw.

We draw on two primary event datasets that jointly maximize the number of conflict-spell observations meeting our scope conditions (i.e., dyadic violence in contested territories): the Uppsala Conflict Data Program’s Georeferenced Event Dataset (UCDP-GED; [Sundberg and Melander, 2013](#)) and the Armed Conflict Location and Event Data project (ACLED; [Raleigh et al., 2010](#)). We harmonize UCDP-GED and ACLED into consistent actor categories and spatial units through the xSub data integration platform ([Zhukov et al., 2019](#)), ensuring that the threshold between restraint and escalation carries the same operational meaning across cases. We restrict the sample to two-sided armed conflicts in which combatants directed violence against each other and civilians, and exclude periods of non-violent contention and residual post-conflict violence. Within each country, we partition observations into discrete conflict spells: contiguous intervals during which the primary belligerent dyad remained relatively stable.²⁴ This procedure yields 136 spells in 50 countries across Africa, Asia, Europe, the Middle East, and the Americas (Appendix A2).

Our spatial unit of observation is the second-order administrative division (generically, “district”). Our temporal unit is the calendar month. Throughout, we assign armed formations fighting on the side of incumbent or occupation authorities to Side 1, and their armed opponents (including state and non-state actors) to Side 2. Covariates include road density, log distance to provincial capital, forest cover, elevation, number of local ethnic groups, petroleum presence, log population (1990 baseline), built-up land area, and fixed effects for province, year and month. For each spell, we fit the same model specification, recover combatants’ predicted choices across counterfactual behavioral scenarios, and assemble them into a coercive asymmetry score with bootstrapped confidence intervals.²⁵

²⁴While [Davenport and Appel \(2022\)](#) delineate spells by violence intensity, our spell boundaries correspond to discontinuities in dyadic composition, due to regime change, state collapse, or the entry/exit of a dominant armed actor (e.g., foreign intervention).

²⁵For each source-spell combination, we estimate eq. (6) with the two-way Mundlak correction. We recover transition probabilities \hat{p}_k , \hat{q}_k by averaging predicted marginal probabilities over the observed covariate distribution under counterfactual opponent behavior, and compute $\hat{\omega} = (1 - \hat{p}_1)\hat{q}_2 - \hat{q}_1(1 - \hat{p}_2)$.

3.1 Patterns of coercive asymmetry across conflicts

Figure 1 reports coercive asymmetry estimates and 95% confidence intervals for 136 conflict spells across 50 countries.²⁶ A positive $\hat{\omega}$ indicates that Side 1’s coercion is self-reinforcing: the asymmetric state where incumbents escalate and the opposition restrains is more persistent than the reverse. A negative estimate indicates that the opposition sustains unilateral escalation more effectively than incumbents. Null estimates indicate coercive symmetry.

The distribution of $\hat{\omega}$ spans a wide range, from strongly negative to strongly positive, with considerable heterogeneity within and across conflict types. 27 (20%) of the $\hat{\omega}$ estimates are positive and significant at the 95% confidence level, and 25 (18%) are negative and significant.²⁷ The remaining 84 (62%) are indistinguishable from zero.

The estimates in Figure 1 reveal two distinct types of sign variation in $\hat{\omega}$: shifts across conflict spells within the same country, and disagreements over the same spell across data sources. Within-country sign reversals often track documented shocks to the structural parameters that govern $\hat{\omega}$, particularly shifts in relative selectivity or outside options of Sides 1 and 2 following events like external military intervention or the loss of material support. Afghanistan is a clear example: $\hat{\omega}$ is negative in both pre-2001 spells (1992–1996, 1996–2001), reflecting the persistent coercive advantage of opposition forces over a fragmented central government. The estimates turn sharply positive following the U.S.-led intervention in October 2001, when external support enabled the new Afghan government (and coalition forces) to sustain operations even where civilian cooperation was scarce.

We draw 95% parametric bootstrap CI’s from the joint posterior of the bivariate probit coefficient vector.

²⁶78% of likelihood ratio tests reject independent-probit in favor of joint estimation (Appendix A2).

²⁷Confidence intervals for $\hat{\omega}$ are wider when the point estimate is negative than when it is positive, and the same asymmetry carries over to the contrast estimates in Figure 2. This is a mechanical consequence of the data-generating process, not a modeling artifact. Negative $\hat{\omega}$ arises when the opposition sustains unilateral escalation more reliably than the government, a configuration in which \hat{p}_1 conditions on HL periods that are rare by construction. The scarcity of observations in this cell inflates bootstrap variance for \hat{p}_1 , producing a noisier estimate of the $HL \rightarrow HL$ transition and a wider bootstrap distribution for $\hat{\omega}$.

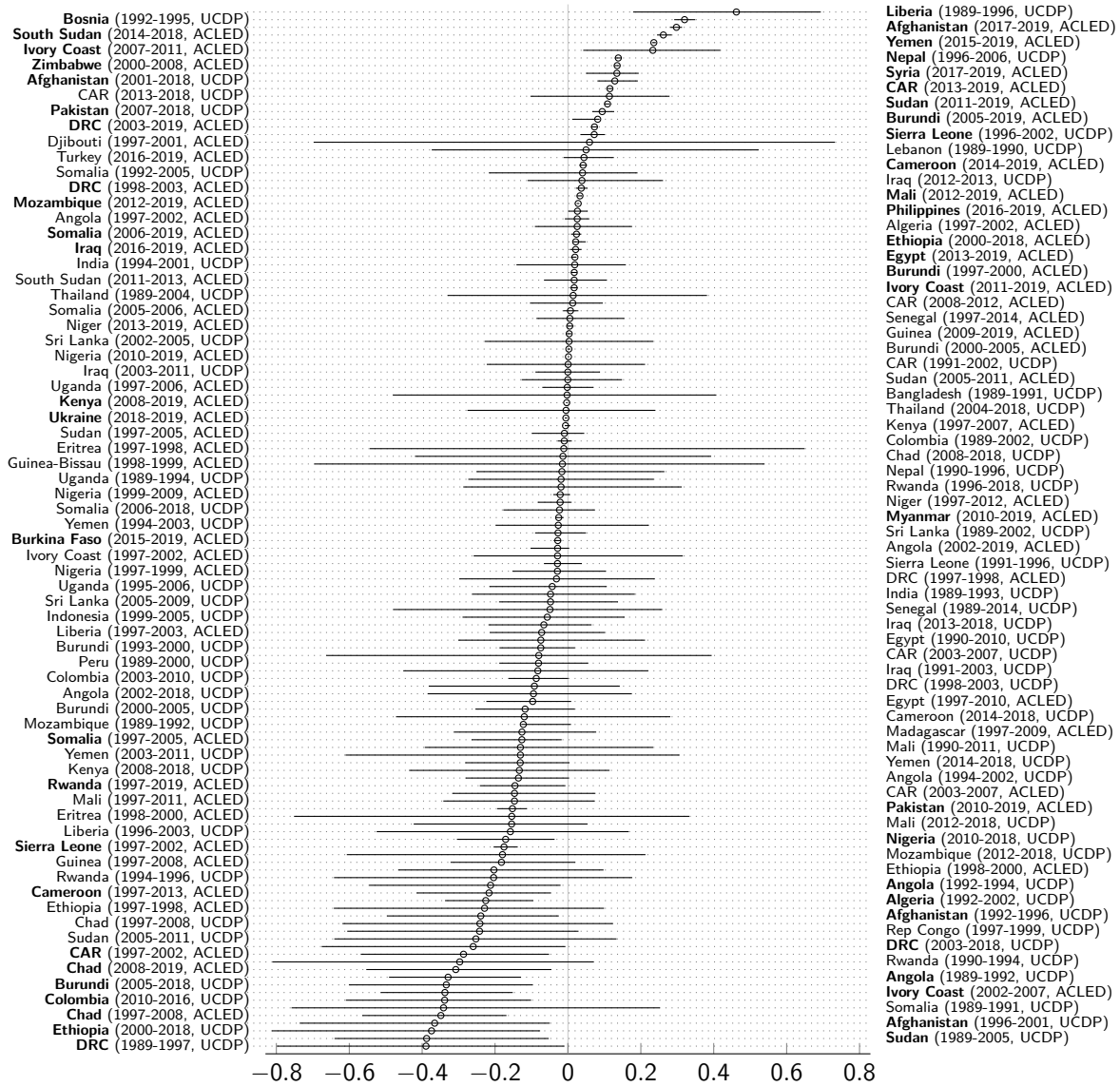


Figure 1: **Cross-national estimates of coercive asymmetry.** Point estimates and 95% bootstrap confidence intervals for $\hat{\omega}$. Bold labels indicate significance at 95% level. Of 136 estimates: 47 (35%) are positive, 89 (65%) are negative, including 27 (20%) positive and significant, 25 (18%) negative and significant.

The Central African Republic (CAR) traces a similar arc. $\hat{\omega}$ is significantly negative during the period of state fragmentation and overlapping rebel factions (1990–2002), drifts toward zero through the transitional period (2003–2007, 2008–2012), and turns significantly positive in the post-2013 spell. The latter shift coincides with the arrival of Russia’s Wagner Group forces in 2017–2018, which substantially expanded government-side outside options, supplying external personnel, materiel, and logistical capacity that insulated the government from dependence on local civilian cooperation.

Sudan shows a parallel trajectory: negative and significant in the early civil war period (1989–2005), near-zero during the Comprehensive Peace Agreement era (2005–2011), and significantly positive in the subsequent post-secession Darfur/South Kordofan period. Cameroon follows the same pattern at a smaller scale: negative during the Boko Haram spillover period in the Far North, turning positive after 2014, as government and paramilitary forces consolidated control over the Anglophone crisis.

A second source of sign variation is less common: disagreement between UCDP-GED and ACLED over the same spell. This pattern affects only five spells — Burundi post-2005, Democratic Republic of Congo post-2003, Ethiopia 2000–2018, Pakistan post-2007, Sierra Leone 1996–2002 — and likely reflects systematic differences in the two sources’ collection protocols rather than genuine uncertainty about conflict dynamics.²⁸ The rarity of such disagreement (five out of 136) testifies to the broad convergence of results across datasets.

²⁸UCDP-GED restricts coverage to events involving organized armed actors, requires documentation of fatalities, and draws primarily on global newswires (Sundberg and Melander, 2013). ACLED casts a broader net, collecting events from local and regional media alongside wire services, and includes non-fatal violence (Raleigh et al., 2010). Because these differences in sourcing and inclusion criteria do not inflate event counts symmetrically across both sides of a dyad, they can shift the relative magnitudes of \hat{p}_1 and \hat{q}_1 , and thus the sign of $\hat{\omega}$, in ways that are conflict-specific and difficult to predict from first principles.

3.2 Targeting selectivity and outside options

Our theory predicts that more selective targeting increases coercive asymmetry: the more accurately a combatant identifies enemy collaborators, the higher the return to sustained punishment.²⁹ We test this prediction by computing $\widehat{\omega}$ at the bottom and top quintiles of the selectivity distribution within each source-spell sample. We operationalize selectivity as a district-level time average of the share of Side 1’s violence directed at armed opponents rather than civilians, consistent with the theoretical role of selectivity as the share of punishment falling on identified enemy collaborators.³⁰

Figure 2 reports the estimated change in coercive asymmetry as selectivity moves from the bottom to the top of each conflict’s distribution, with 95% confidence intervals.³¹ Most cases (74%) show a positive contrast, consistent with our theoretical prediction. 53 (39%) of the estimated contrasts are positive and significant, and another 48 (35%) are positive but statistically indistinguishable from zero. No case shows a statistically significant negative contrast. Higher selectivity never reliably reduces government coercive asymmetry.

These results hold across all 136 source-spell samples, spanning five decades, four continents, and two data sources. They hold regardless of whether government coercive asymmetry is itself positive or negative. Even where the opposition is generally more persistent, greater government selectivity narrows the gap and sometimes reverses it. In 16 cases where $\widehat{\omega} < 0$, the high-selectivity estimate turns positive, and 11 of these reversals are statistically significant (e.g., Iraq, Somalia, Myanmar).

Our theory also predicts that combatants with robust outside options (e.g., external sponsors, resource rents, forces drawn from outside the locality) can sustain unilateral

²⁹Corollary 1(i) establishes $\partial\omega/\partial\lambda_1 > 0$: greater selectivity by Side 1 raises $\widehat{\omega}$.

³⁰In xSub, we compute $\bar{\lambda}_{1i} = \frac{1}{T} \sum_t \frac{\text{DYAD_A_B}_{it}}{\text{INITIATOR_SIDE}_{it}}$, where DYAD_A_B_{it} counts Side 1-initiated events targeting Side 2 in location i at t and $\text{INITIATOR_SIDE}_{it}$ counts all Side 1-initiated events in i, t .

³¹Formally, the contrast is $\Delta\widehat{\omega} = \widehat{\omega}^{\text{hi}} - \widehat{\omega}^{\text{lo}}$, where $\widehat{\omega}^{\text{lo}}$ and $\widehat{\omega}^{\text{hi}}$ are expected values of $\widehat{\omega}$ when $\bar{\lambda}_{1i}$ is at its observed minimum and maximum, respectively, holding all other covariates at their medians.

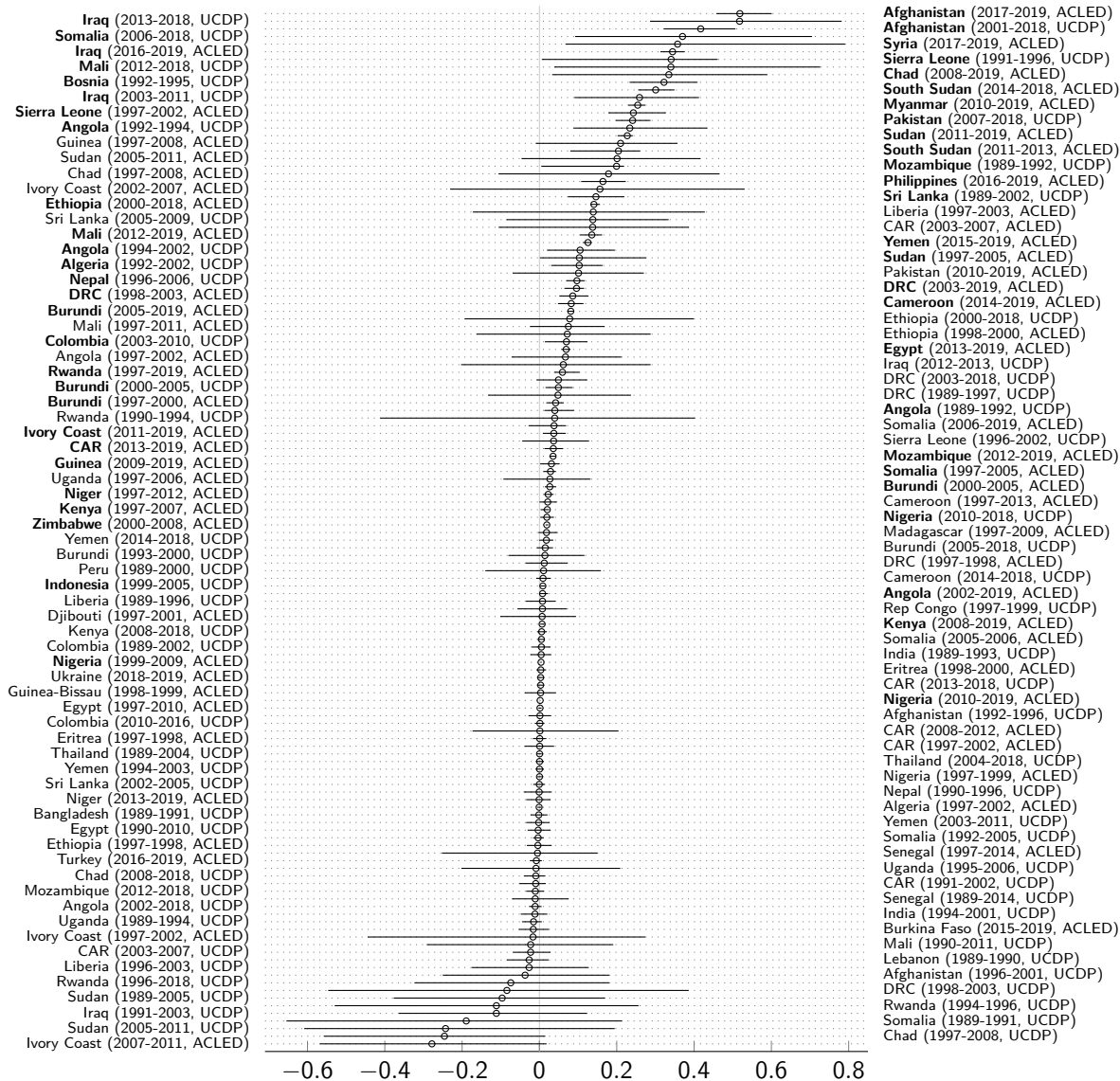


Figure 2: **Coercive asymmetry rises with selectivity of violence.** Point estimates and 95% bootstrap confidence intervals for $\Delta\hat{\omega} = \hat{\omega}^{\text{hi}} - \hat{\omega}^{\text{lo}}$, where $\hat{\omega}^{\text{hi}}, \hat{\omega}^{\text{lo}}$ are average values of $\hat{\omega}$ for localities in the highest versus lowest quintiles of $\bar{\lambda}_1$. Bold labels indicate significance at 95% level. Of 136 estimates: 101 (74%) are positive, 35 (26%) are negative, including 53 (39%) positive and significant, 0 (0%) negative and significant.

escalation even without selective targeting, because they depend less on local civilian cooperation to keep fighting.³² We test this prediction with two complementary approaches.

First, we ask how often $\hat{\omega}$ is positive and significant even at low selectivity, a pattern that targeting precision alone cannot explain.³³ Of the 136 conflict spells, 16 (12%) show significantly positive coercive asymmetry even where government operations are mostly indiscriminate, consistent with outside options sustaining escalation independently of targeting capacity. Liberia’s first civil war (1989–1996) fits this pattern. Charles Taylor’s forces drew on timber and mineral revenues to sustain operations largely without relying on civilian cooperation. Ivory Coast’s post-election crisis (2007–2011) coincides with French *Licorne* force involvement that gave the government a coercive backstop. Yemen (2015–2019) reflects the Saudi-led coalition’s access to external materiel and logistical support. Bosnia (1992–1995) and CAR (post-2013) similarly point to external reinforcement as the operative mechanism. In each case, access to resources or forces from outside the locality sustained coercive asymmetry where selective targeting could not.

Second, we ask whether local conditions that proxy for outside options and escalation costs shift coercive asymmetry in the directions our theory predicts. For each spell, we shift each covariate from its observed minimum to maximum while holding all others at their medians, and report the resulting change in $\hat{\omega}$.³⁴ Table 1 summarizes the results.

Petroleum presence (i.e., a binary indicator of whether any petroleum deposit lies within the district, [Lujala et al. 2007](#)) shows the cleanest ratio of significantly positive to negative contrasts (6:1 across 73 spells), consistent with resource rents providing outside options that can sustain government escalation without selective targeting.

³²Corollary 1(iv) establishes $\partial\omega/\partial c_1 > 0$: greater outside options c_1 lower the selectivity threshold λ_1^\dagger , expanding the region in which Side 1 escalates.

³³Specifically, we evaluate $\hat{\omega}$ at the bottom quintile of the $\bar{\lambda}_{1i}$ distribution and identify cases where the 95% bootstrap CI excludes zero.

³⁴The estimation approach is identical to the one we describe in fn. 31.

Proximity to built-up areas and provincial capitals produce similar patterns. Such settings offer incumbents superior infrastructure, easier force projection, and established administrative presence, reducing the logistical costs of sustained escalation and providing resource flows that reduce government dependence on civilian support.³⁵

Ethnic co-habitation (i.e., the number of unique ethnic settlement zones overlapping each district, Weidmann et al. 2010) yields perhaps the most consistent results: 68% of spells show a positive contrast, with significantly positive cases outnumbering significantly negative ones by nearly 4:1. Where multiple ethnic groups’ territories overlap, civilians face higher coordination costs for collective resistance, making sustained defiance harder to organize and reinforcing the government’s coercive advantage.

Table 1: **Cross-national comparative statics.** Each cell reports the number of source-spell samples in which $\hat{\omega}$ increases or decreases when a covariate moves from its observed minimum to maximum, holding all other covariates at their medians. Sample sizes vary across covariates due to exclusion of cases with no within-spell variation or missing data on the covariate in question. 95% bootstrap confidence intervals determine significance.

Covariate	Spells	Countries	All contrasts		Significant at 95%	
			Positive	Negative	Positive	Negative
Number of ethnic groups	109	41	74 (68%)	35 (32%)	23 (21%)	6 (6%)
Built-up areas	111	43	66 (59%)	45 (41%)	30 (27%)	9 (8%)
Log population (1990)	111	43	64 (58%)	47 (42%)	16 (14%)	8 (7%)
Petroleum presence	73	29	42 (58%)	31 (42%)	12 (16%)	2 (3%)
Road density	111	43	60 (54%)	51 (46%)	10 (9%)	12 (11%)
Forest cover	111	43	56 (50%)	55 (50%)	14 (13%)	14 (13%)
Mean elevation	111	43	55 (50%)	56 (50%)	15 (14%)	17 (15%)
Log distance adm. center	111	43	46 (41%)	65 (59%)	9 (8%)	19 (17%)

³⁵Corollary 1(iii) establishes $\partial\omega/\partial\kappa_1(H) \leq 0$: lower escalation costs for Side 1 raise $\hat{\omega}$.

4 Application II: Soviet Union vs. Ukrainian insurgents

The cross-national results establish that coercive asymmetry tracks selectivity and outside options across a wide range of conflicts. We now test whether the same patterns hold under more demanding measurement conditions. A post-World War II conflict in western Ukraine meets our scope conditions directly: Soviet security forces and the Ukrainian Insurgent Army (UPA) competed for the loyalty of civilians across territories the USSR had recently annexed and did not fully control. We draw on declassified secret police and insurgent records covering Ukrainian conflict events from 1943 to 1955 (Zhukov, 2015; Rozenas et al., 2017).³⁶ These archival data are more comprehensive (i.e., less censoring of low-intensity incidents) than the media-based event data we used in cross-national analyses, improving estimation of transition probabilities that depend on sustained low-violence runs.

We aggregate to second-tier administrative units and restrict the sample to the spatio-temporal epicenter of the conflict: Ukraine’s eight newly-annexed western oblasts in 1944–1950 ($N = 20,076$ rayon-months).³⁷ Outcomes are binary rayon-month indicators: Soviet violence of any kind (Side 1) and UPA-initiated violence (Side 2). We proxy selectivity as the rayon-level share of violence the Soviet directed at insurgents rather than civilians, averaged over the sample period.³⁸ Combatant-specific covariates reflect each side’s structural constraints: administrative reach and state capacity for the Soviets, mobility and concealment for the UPA.³⁹ Both sets include oblast, year, and month fixed effects.

³⁶Data available at doi.org/10.1177/0022002713520590 under “Supplementary Material.”

³⁷Oblasts and rayons were, respectively, first- and second-tier administrative units in Soviet Ukraine.

³⁸We calculate $\bar{\lambda}_{1i} = \frac{1}{T} \sum_t \frac{\text{insurgent-targeted incidents}_{it}}{\text{all Soviet incidents}_{it}}$, where the numerator counts only operations the Soviets recorded as targeting UPA combatants in i, t (i.e., no family deportations, no civilian deaths).

³⁹For the Soviets, $\mathbf{x}_{1,t}$ includes railway access, number of village soviets (administrative depth), electrification, natural resources, agricultural land share, urbanization, rayon area, and pre-1944 Soviet partisan control. For the UPA, $\mathbf{x}_{2,t}$ substitutes forest cover (concealment) and border crossings to neighboring rayons for village soviets and partisan control. Village soviets and partisan control proxy Soviet administrative and intelligence infrastructure; forest cover and local crossings governed insurgent mobility — neither set of variables has a meaningful analogue for the other side.

4.1 Patterns of coercive asymmetry in Soviet Ukraine

Empirical estimates of coercive asymmetry are consistently positive and significant across all specifications, indicating that one-sided Soviet violence was more self-sustaining than one-sided violence by the UPA. In the best-fitting model, $\hat{\omega} = 0.2$ (95% CI: [0.16, 0.26]).⁴⁰

Figure 3 shows how estimates of Soviet coercive asymmetry $\hat{\omega}$ vary across quintiles of selectivity (i.e., share of violence directed only at insurgents). Soviet coercive asymmetry increases with selectivity, from $\hat{\omega} = 0.141$ in the lowest quintile to $\hat{\omega} = 0.251$ in the highest. Where Soviet forces discriminated more precisely between insurgents and civilians, their monopolization of violence became more durable.

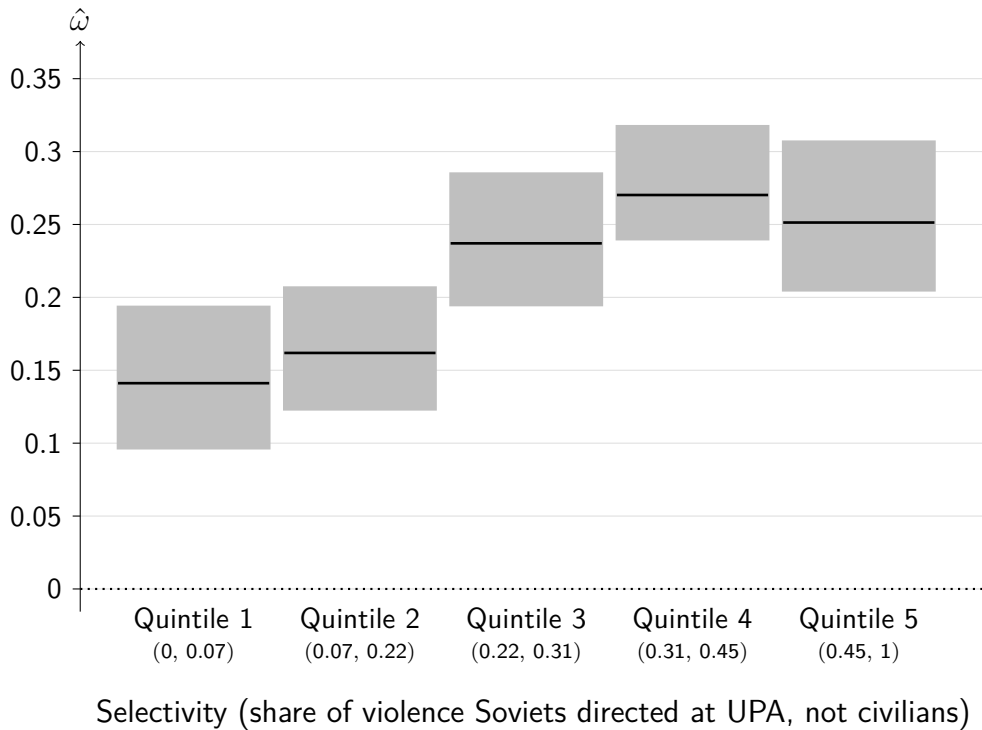


Figure 3: **Coercive asymmetry rises with Soviet selectivity.** Black lines are $\hat{\omega}$ point estimates, grey bars are 95% bootstrap CIs. Parentheses show selectivity range in quintile.

⁴⁰We use the two-way Mundlak specification for all $\hat{\omega}$ inference. Likelihood ratio tests confirm significant cross-equation correlation, validating joint estimation over independent equations (Appendix A2).

That $\hat{\omega}$ is positive and significant even in the lowest-selectivity quintile — where over 93 percent of Soviet violence fell on civilians (Figure 3) — points to outside options as an independent driver of Soviet escalation.⁴¹ Table 2 identifies the structural conditions behind this pattern.⁴² First, denser institutional footprints connected localities to state supply chains and personnel rotations, reducing civilian dependence. Our proxy for administrative depth, village soviet density, produces the largest contrast, $\Delta\hat{\omega} = 0.17$ (95% CI: 0.09, 0.26). Second, WWII-era partisan presence built supply networks and local intelligence that carried into post-WWII Soviet institutions, further insulating security forces from civilian dependence. Pre-1944 partisan control produces a positive contrast, $\Delta\hat{\omega} = 0.05$ (95% CI: 0.02, 0.08). Third, urban areas concentrated administrative and economic assets, sustaining Soviet escalation. Urban share yields a modest positive contrast, $\Delta\hat{\omega} = 0.02$ (95% CI: 0.003, 0.04).⁴³ The other covariates in Table 2 fall short of conventional significance, suggesting they capture underlying structural channels with considerable noise.

Table 2: **Comparative statics.** Change in $\hat{\omega}$ when each covariate moves from its observed minimum to maximum, holding all others at medians. 95% bootstrap CIs in parentheses.

Covariate	Range	$\Delta\hat{\omega}$ (95% CI)
Village soviets	7 → 82	0.166 (0.093, 0.263)
Partisan control (pre-1944)	0 → 1	0.045 (0.021, 0.075)
Urban share	0 → 1	0.021 (0.003, 0.040)
Area (1000 sq km)	0 → 1.8	0.077 (-0.012, 0.138)
Railway present	0 → 1	0.008 (-0.012, 0.022)
Electrification	0 → 1	0.034 (-0.013, 0.085)
Natural resources	0 → 3	0.010 (-0.028, 0.050)
Agricultural land	0 → 1	-0.005 (-0.034, 0.017)

⁴¹When $c_k > \kappa_k(H)/[\Delta(1 - \bar{\theta}_i)]$, outside options alone sustain escalation (Remark 1). The positive $\hat{\omega}$ at the lowest quintile of λ_1 suggests Soviet material resources satisfied this condition.

⁴²The contrast in Table 2 omit Mundlak corrections to enable inference on time-invariant covariates.

⁴³In addition to outside options (c_1), partisan control and urban density both potentially raise targeting selectivity (λ_1), making the two channels difficult to cleanly separate.

5 Threats to Inference

Our estimates clear the hardest inferential hurdle: the core patterns hold across a broad cross-national sample and survive replication in archival data where reporting and misclassification concerns are less acute. The most serious remaining concerns are more likely to attenuate our comparative static results than to generate them spuriously.

First, reporting bias is the most direct threat to inference. Violence against civilians may be more observable than violence against combatants, and larger events are more likely to enter both archival and media-based records than smaller ones. Either pattern can inflate the level estimate $\hat{\omega}$ if it makes Side 1's escalation more visible than Side 2's.⁴⁴ The contrast estimates $\Delta\hat{\omega}$ are less vulnerable: reporting asymmetries would need to vary systematically with selectivity to generate a spurious positive gradient. If anything, the most visible events tend to be large and indiscriminate, which would compress differences between high- and low-selectivity localities and attenuate $\Delta\hat{\omega}$ toward zero.

Second, binary coding of escalation imposes a visibility threshold: some escalatory behavior goes unrecorded, and some low-level violence appears as escalation.⁴⁵ Such misclassification adds noise to the transition probabilities we use to construct $\hat{\omega}$, pulling the level estimate toward zero. It also attenuates $\Delta\hat{\omega}$ by shrinking contrasts across localities.

Third, selectivity measures may be endogenous to local conditions that simultaneously sustain one-sided violence. Better-trained or more experienced units may both target more selectively and sustain escalation more effectively, so the positive association between selectivity and $\hat{\omega}$ may partly reflect unit quality rather than the effect of selectivity. This concern does not bias the level estimate $\hat{\omega}$, which we identify from observed transition patterns rather than from the selectivity proxy, but it can inflate $\Delta\hat{\omega}$ if localities with

⁴⁴In media-accessible settings, journalists may document government operations more thoroughly than insurgent ones, biasing $\hat{\omega}$ upward. In closed authoritarian settings, the reverse may hold.

⁴⁵Our observation mechanism assumes that low punishment is empirically invisible, and high punishment is not. This is a necessary simplification, not a claim that zeroes represent perfect peace.

higher-quality units sort into both higher selectivity and higher coercive asymmetry. We reduce this risk by proxying selectivity with time-averaged targeting shares rather than short-run tactical choices, and by conditioning on pre-conflict locality attributes.

Fourth, the cross-national analysis relies on imperfect proxies for outside options and escalation costs, and on datasets with inconsistent sourcing rules, media access, and visibility of violence. Noisy proxies are unlikely to bias $\hat{\omega}$ systematically, but they attenuate comparative statics by weakening the empirical stand-ins for our theoretical parameters. Source disagreement adds variance in the same direction, although UCDP-GED and ACLED agree in almost all overlapping spells. The Soviet-UPA application serves as a partial check on both concerns: the same theoretical gradients appear under archival measurement conditions where proxy noise and reporting gaps are substantially lower.

6 Conclusion

Combatants escalate unilaterally when and where their violence is selective enough to extract civilian compliance, or when outside options make compliance dispensable. We derived these conditions, measured their behavioral imprint from sequences of observed violence, and showed that coercive asymmetry shifts predictably across space and time.

Several extensions follow naturally from these contributions. First, future work could extend our theoretical framework to allow strategic investment in selectivity and outside options (e.g., surveillance infrastructure, patronage networks, alliances) or relax the dyadic structure to model asymmetry in multi-party conflicts. Second, $\hat{\omega}$ provides a common metric for phenomena the literature has treated as distinct (e.g., wartime repression, terrorism, mass atrocities), revealing them as points on a single continuum rather than categorically different objects of study. Researchers can now classify conflicts by which side holds the structural coercive advantage and why, sharpening downstream work on conflict duration,

civilian casualties, and settlement. Third, our comparative statics rest on observational variation. Identifying exogenous shocks to selectivity and outside options (e.g., foreign materiel influx, surveillance technology rollout) would move the study of one-sided violence from structural estimation of theoretical parameters toward credible causal identification.

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A1 Theoretical Appendix

Table A1: Model notation.

Symbol	Description
i, t, j, k	Indices for locality, time period, civilian, and combatant ($k \in \mathcal{K}$).
\mathcal{K}	Combatant set $\mathcal{K} = \{1, 2\}$. Side 1 is incumbent, 2 is challenger.
A_k	Action set for combatant k : $A_k = \{L, H\} = \{0, \Delta\}$.
ρ_k	Punishment level combatant k chooses; $\rho_k \in A_k$.
L, H, Δ	Low and high punishment; $L = 0, H = \Delta > 0$.
θ_j	Civilian j 's type (relative collateral vs. selective vulnerability).
$F(\theta x_i), f(\theta x_i)$	Type distribution and density in locality i , conditional on local observables x_i . Mean type in locality i is $\bar{\theta}_i = \mathbb{E}[\theta_j x_i]$.
e_j, e_j^*, e_k^*	Civilian j 's cooperation with Side 1 ($e_j \in [0, 1]$); e_j^* is optimal value; e_k^* is share that k receives ($e_1^* = e_j^*, e_2^* = 1 - e_j^*$).
\bar{e}_i	Mean cooperation with Side 1 in locality i : $\bar{e}_i = \mathbb{E}[e_j^* x_i]$.
λ_k	Selectivity of k 's violence.
λ_k^\dagger	Critical selectivity level at which H becomes optimal for k .
λ_i^*	Aggregate civilian threshold in locality i : $\lambda_i^* = \bar{\theta}_i$.
b	Value of a unit of civilian cooperation to each combatant.
$\kappa_k(\rho_k)$	Punishment cost for k ; $\kappa_k(L) = 0, \kappa_k(H) > 0$.
c_k	Combatant k 's outside options; $c_k \geq 0$.
γ	Curvature of civilian utility (cost of deviating from $e_j = 1/2$).
Φ_k	Cooperation premium of combatant k : $\Phi_k = \frac{\Delta}{\gamma}(\lambda_k - \bar{\theta}_i)$.
$U_j(e_j), U_k(\rho_k, \rho_{-k})$	Civilian j 's utility, combatant k 's utility.
S, s_{it}	State space; state of locality i at time t : $s_{it} = (\lambda_{it}, x_{it})$.
g, ε_{it}	Selectivity transition: $\lambda_{i,t+1} = g(\lambda_{it}, \rho_{1,it}, \rho_{2,it}, x_{it}) + \varepsilon_{i,t+1}$.
$v_k(s, \rho_k, \rho_{-k})$	Stage payoff to combatant k in state s under profile (ρ_k, ρ_{-k}) .
$\xi_k, V_k(s)$	Strategy and continuation value of combatant k at state s .
p_k, q_k	Reactive strategies: $p_k = P(L_t L_{t-1}^{-k}), q_k = P(L_t H_{t-1}^{-k})$.
δ	Common discount factor in the stochastic game.
$\mathcal{A} = A_1 \times A_2$	Set of observable punishment profiles: $\mathcal{A} = \{LL, LH, HL, HH\}$.
$M, m_{a'a}$	4×4 transition matrix over \mathcal{A} ; $m_{a'a} = P(a' a)$ for $a, a' \in \mathcal{A}$.
ω	Coercive asymmetry index: $\omega = m_{HL,HL} - m_{LH,LH}$.

Two combatants ($k \in \{1, 2\}$) simultaneously choose $\rho_k \in \{L, H\} = \{0, \Delta\}$ in each locality i and period t . Each civilian j draws type $\theta_j \sim F(\theta|x_i)$, with $\bar{\theta}_i \equiv \mathbb{E}[\theta_j|x_i]$. θ_j is private information, but combatants observe $F(\theta|x_i)$.

Assumption 1 (Civilian utility). Civilian j chooses $e_j \in [0, 1]$ to maximize

$$U_j(e_j) = -[\theta_j(1-\lambda_1)\rho_1 + (1-\theta_j)\lambda_2\rho_2]e_j - [\theta_j(1-\lambda_2)\rho_2 + (1-\theta_j)\lambda_1\rho_1](1-e_j) - \frac{\gamma}{2}\left(e_j - \frac{1}{2}\right)^2. \quad (7)$$

Assumption 2 (Combatant utilities). Combatant k chooses $\rho_k \in \{L, H\}$ to maximize

$$U_k(\rho_k, \rho_{-k}) = b \cdot \mathbb{E}[e_k^* | x_i] + c_k(1 - \lambda_k)\rho_k - \kappa_k(\rho_k), \quad (8)$$

where $b > 0$, $c_k \geq 0$, $\kappa_k(L) = 0$, $\kappa_k(H) > 0$. Note that c_k does not enter civilian utility.

Proposition 1 (Civilian response). *Fix locality i .*

(a) (*Interior solution.*) If $\gamma > 2|(\lambda_1 - \theta_j)\rho_1 - (\lambda_2 - \theta_j)\rho_2|$, j has a unique best response:

$$e_j^* = \frac{1}{2} + \frac{(\lambda_1 - \theta_j)\rho_1 - (\lambda_2 - \theta_j)\rho_2}{\gamma}. \quad (9)$$

(b) (*Defiance and submission.*)

$$\frac{\partial e_j^*}{\partial \rho_1} = \frac{\lambda_1 - \theta_j}{\gamma}, \quad \frac{\partial e_j^*}{\partial \rho_2} = -\frac{\lambda_2 - \theta_j}{\gamma}. \quad (10)$$

Civilian j defies Side 1 iff $\lambda_1 < \theta_j$; submits iff $\lambda_1 > \theta_j$. Symmetrically for Side 2.

(c) (*Aggregation.*)

$$\bar{e}_i(\rho_1, \rho_2) = \frac{1}{2} + \frac{(\lambda_1 - \bar{\theta}_i)\rho_1 - (\lambda_2 - \bar{\theta}_i)\rho_2}{\gamma}. \quad (11)$$

Define $\lambda_i^* \equiv \bar{\theta}_i$. The mean response \bar{e}_i rises with ρ_1 iff $\lambda_1 > \lambda_i^*$, falls iff $\lambda_1 < \lambda_i^*$.

Proof. Part (a). The FOC $\partial U_j / \partial e_j = 0$ gives $-[(\theta_j - \lambda_1)\rho_1 - (\theta_j - \lambda_2)\rho_2] - \gamma(e_j - \frac{1}{2}) = 0$, which solves to (9). Strict concavity ($\partial^2 U_j / \partial e_j^2 = -\gamma < 0$) confirms uniqueness.

Part (b). Differentiate (9) directly to obtain (10).

Part (c). Take expectations of (9) over $\theta \sim F(\theta|x_i)$ to obtain (11); the sign of $\partial \bar{e}_i / \partial \rho_1 = (\lambda_1 - \bar{\theta}_i) / \gamma$ follows immediately. \square

Define the *cooperation premium*:

$$\Phi_k \equiv \frac{\Delta}{\gamma}(\lambda_k - \bar{\theta}_i). \quad (12)$$

Mean cooperation with k under each profile is $\mathbb{E}[e_k^*|H, L] = \frac{1}{2} + \Phi_k$, $\mathbb{E}[e_k^*|L, H] = \frac{1}{2} - \Phi_{-k}$, $\mathbb{E}[e_k^*|H, H] = \frac{1}{2} + \Phi_k - \Phi_{-k}$, $\mathbb{E}[e_k^*|L, L] = \frac{1}{2}$. Since $\rho_k \in \{0, \Delta\}$, net gain from escalating is

$$U_k(H, \cdot) - U_k(L, \cdot) = b\Phi_k + c_k(1 - \lambda_k)\Delta - \kappa_k(H), \quad (13)$$

independent of the opponent's action and strictly increasing in λ_k (since $c_k < b/\gamma$).

Theorem 1 (Equilibrium regimes). *Fix locality i with $\bar{\theta}_i$, (λ_1, λ_2) , $\kappa_k(H) > 0$, and $c_k \geq 0$ with $c_k < b/\gamma$ for each k . Define*

$$\lambda_k^\dagger \equiv \bar{\theta}_i + \frac{\gamma[\kappa_k(H) - c_k(1 - \bar{\theta}_i)\Delta]}{(b - c_k\gamma)\Delta}. \quad (14)$$

The game is dominance-solvable with unique Nash equilibrium:

- (i) (H, H) if $\lambda_1 > \lambda_1^\dagger$ and $\lambda_2 > \lambda_2^\dagger$ (mutual escalation);
- (ii) (L, L) if $\lambda_1 < \lambda_1^\dagger$ and $\lambda_2 < \lambda_2^\dagger$ (mutual restraint);
- (iii) (H, L) if $\lambda_1 > \lambda_1^\dagger$ and $\lambda_2 < \lambda_2^\dagger$ (asymmetric escalation by Side 1);
- (iv) (L, H) if $\lambda_1 < \lambda_1^\dagger$ and $\lambda_2 > \lambda_2^\dagger$ (asymmetric escalation by Side 2).

When $\kappa_1(H) = \kappa_2(H)$ and $c_1 = c_2$, the thresholds coincide and only regimes (i)–(ii) arise.

Proof. Substituting $\Phi_k = \Delta(\lambda_k - \bar{\theta}_i)/\gamma$ into (13):

$$U_k(H, \cdot) - U_k(L, \cdot) = \Delta\lambda_k\left(\frac{b}{\gamma} - c_k\right) + \Delta\left(c_k - \frac{b\bar{\theta}_i}{\gamma}\right) - \kappa_k(H).$$

Since $c_k < b/\gamma$, the coefficient on λ_k is positive. Setting equal to zero and solving yields (14). $\rho_k = H$ strictly dominates iff $\lambda_k > \lambda_k^\dagger$; $\rho_k = L$ strictly dominates iff $\lambda_k < \lambda_k^\dagger$. Since (13) is independent of opponent's action, each combatant has a strictly dominant action, making the game dominance-solvable. Crossing the two independent conditions yields (i)–(iv); symmetry of thresholds under $\kappa_1(H) = \kappa_2(H)$, $c_1 = c_2$ rules out asymmetric profiles. \square

Remark 1 (Outside options expand the region in which escalation dominates). The threshold λ_k^\dagger is strictly decreasing in c_k . When $c_k > \kappa_k(H)/[\Delta(1 - \bar{\theta}_i)]$, λ_k^\dagger falls below $\bar{\theta}_i$, and any

combatant k with $\lambda_k \in (\lambda_k^\dagger, \bar{\theta}_i)$ escalates despite a negative cooperation premium ($\Phi_k < 0$). The outside-option term $c_k(1 - \lambda_k)\Delta$ covers both the cooperation loss and $\kappa_k(H)$.

Suppose each k conditions its current action on the opponent's previous action. Define

$$p_k \equiv P(\rho_{kt} = L \mid \rho_{-k,t-1} = L), \quad q_k \equiv P(\rho_{kt} = L \mid \rho_{-k,t-1} = H). \quad (15)$$

At any fixed state $s_{it} = (\lambda_{it}, x_{it})$, Theorem 1 implies $p_k = q_k$. But $p_k \neq q_k$ can hold in the time series because λ_{it} evolves stochastically. The opponent's lagged action proxies for the unobserved current state, and the reactive strategies encode equilibrium belief-updating.

Proposition 2 (Coercive asymmetry index).

(a) (*Transition matrix.*) Under the independence restriction,

$$m_{HL,HL} = (1 - p_1)q_2, \quad m_{LH,LH} = q_1(1 - p_2). \quad (16)$$

(b) (*Coercive asymmetry index.*)

$$\omega \equiv m_{HL,HL} - m_{LH,LH} = (1 - p_1)q_2 - q_1(1 - p_2). \quad (17)$$

(c) (*Structural decomposition.*) Under the Markov perfect equilibrium (MPE) of the stochastic game,

$$\omega = \omega(\lambda_1, \lambda_2, c_1, c_2, \kappa_1(H), \kappa_2(H), \bar{\theta}_i, b, \gamma, g). \quad (18)$$

$\omega > 0$ iff Side 1 escalates ($\lambda_1 > \lambda_1^\dagger$) and Side 2 restrains ($\lambda_2 < \lambda_2^\dagger$); $\omega < 0$ iff Side 1 restrains and Side 2 escalates; $\omega = 0$ iff $\lambda_k > \lambda_k^\dagger \forall k$ or $\lambda_k < \lambda_k^\dagger \forall k$.

Proof. Part (a). Under independence, $m_{a'a} = P(a'_1|a_2) \cdot P(a'_2|a_1)$. For $HL \rightarrow HL$: Side 1 sees L and plays H with probability $1 - p_1$; Side 2 sees H and plays L with probability q_2 ; so $m_{HL,HL} = (1 - p_1)q_2$. $m_{LH,LH} = q_1(1 - p_2)$ follows by symmetry.

Part (b). Subtract (16) directly.

Part (c). The MPE pins down $\xi_k : S \rightarrow A_k$, making (p_k, q_k) functions of structural primitives, establishing (18). For sign identification: when Side 1 escalates ($p_1 = q_1 = 0$) and Side 2 restrains ($p_2 = q_2 = 1$), $\omega = 1 \cdot 1 - 0 \cdot 0 = 1 > 0$. Reversing gives $\omega = 0 \cdot 0 - 1 \cdot 1 = -1 < 0$. Under symmetry, $p_k = q_k$ and $\omega = (1 - p_1)p_2 - p_1(1 - p_2) = p_2 - p_1 = 0$. \square

Remark 2 (MPE existence). The stochastic game — with state space S , action sets A_k , stage payoffs $v_k(s, \rho_k, \rho_{-k})$, transition kernel $P(s' \mid s, \rho_k, \rho_{-k})$, and discount factor $\delta \in (0, 1)$

— admits a stationary MPE, in which each k plays strategy $\xi_k : S \rightarrow A_k$ satisfying

$$V_k(s) = \max_{\rho_k \in A_k} \left\{ v_k(s, \rho_k, \xi_{-k}(s)) + \delta \int V_k(s') P(ds' | s, \rho_k, \xi_{-k}(s)) \right\}.$$

Existence follows from [Fink \(1964\)](#) for finite state and action spaces, and from [Mertens and Parthasarathy \(1987\)](#) in the general continuous-state case.

Corollary 1 (Comparative statics on ω).

(i) $\partial\omega/\partial\lambda_1 \geq 0$, $\partial\omega/\partial\lambda_2 \leq 0$.

(ii) $\partial\omega/\partial\bar{\theta}_i \geq 0$ as $\lambda_1 \geq \lambda_2$.

(iii) $\partial\omega/\partial\kappa_1(H) \leq 0$, $\partial\omega/\partial\kappa_2(H) \geq 0$.

(iv) $\partial\omega/\partial c_1 \geq 0$, $\partial\omega/\partial c_2 \leq 0$.

Proof. Parts (i)–(iii) follow from Proposition 2(c) and Theorem 1. For part (iv): from (14), $\partial\lambda_k^\dagger/\partial c_k < 0$ since both the numerator $\kappa_k(H) - c_k(1 - \bar{\theta}_i)\Delta$ and denominator $b - c_k\gamma$ decrease in c_k , with net derivative negative under $c_k < b/\gamma$. A lower λ_1^\dagger expands the region $\lambda_1 > \lambda_1^\dagger$, raises $m_{HL,HL}$ and ω . Symmetric argument for c_2 lowers λ_2^\dagger , raises $m_{LH,LH}$, reduces ω . \square

A2 Empirical Appendix

This appendix provides supporting documentation for both empirical applications. Table A2 lists the conflict spells we included in our cross-national estimation sample. After manual review, we removed spells with no active conflict, spells where the conflict ended before the sample period begins, and borderline cases. Table A3 summarizes model fit across these spells: each cell reports the percentage of spells in which a likelihood ratio test rejects the column model in favor of the row model at the 95% confidence level. Tables A4 and A5 present the full model comparison and $\hat{\omega}$ estimates for the Soviet-UPA application. Table A4 reports LRT statistics comparing each nested specification against its alternatives. Table A5 reports the corresponding $\hat{\omega}$ point estimates and 95% bootstrap confidence intervals alongside benchmarks from independent probit specifications.

Table A2: Conflict spells in the ACLED + UCDP-GED cross-national sample.

Country	Period	Country	Period	Country	Period	Country	Period
Afghanistan	(1989–1992) (1992–1996) (1996–2001) (2001–2019)	Colombia	(1989–2002) (2003–2010) (2010–2016) (2016–2018)	Ivory Coast	(1990–2002) (2002–2007) (2007–2011) (2011–2019)	Rwanda	(1990–1994) (1994–1994) (1994–1996) (1996–2019)
Albania	(1996–1997)	DRC	(1989–1997) (1997–1998) (1998–2003) (2003–2019)	Kenya	(1989–2007) (2008–2019)	Senegal	(1989–2014)
Algeria	(1990–1991) (1992–2002)			Lebanon	(1989–1990) (2005–2006)	Sierra Leone	(1990–1996) (1996–2002)
Angola	(1989–1992) (1992–1992) (1992–1994) (1994–2002) (2002–2019)	Djibouti	(1997–2001)	Liberia	(1989–1996) (1996–2003)	Somalia	(1989–1991) (1992–2005) (2005–2006)
		Egypt	(1990–2010) (2013–2013) (2013–2019)	Madagascar	(1990–2009)		(2006–2019)
Azerbaijan	(1987–1991) (1992–1994)	Eritrea	(1997–1998) (1998–2000)	Mali	(1990–2011) (2012–2019)	South Africa	(1990–1994)
				Mauritania	(1990–2007)	South Sudan	(2011–2013) (2014–2018)
Bangladesh	(1970–1971) (1972–1991)	Ethiopia	(1989–1991) (1991–1998) (1998–2000) (2000–2018)	Mozambique	(1989–1992) (2012–2019)	Spain	(1989–2010)
Bosnia	(1992–1995)			Myanmar	(2010–2019)	Sri Lanka	(1989–2002) (2002–2005)
Burkina Faso	(2015–2019)			Nepal	(1990–1996) (1996–2006)		(2005–2009)
Burundi	(1990–1993) (1993–2000) (2000–2005) (2005–2019)	Guatemala	(1990–1996)	Nicaragua	(1989–1990) (1989–2012) (2013–2019)	Sudan	(1989–2005) (2005–2011) (2011–2019)
		Guinea	(1991–2008) (2009–2019)	Niger	(1989–2012) (2013–2019)	Syria	(2017–2019)
CAR	(1990–2002) (2003–2007) (2008–2012) (2013–2019)	Guinea-Bissau	(1995–1998) (1998–1999)	Nigeria	(1990–1999) (1999–2009)	Thailand	(1989–2004) (2004–2019)
		Haiti	(1990–1994)			Turkey	(2016–2019)
		India	(1972–1984) (1985–1993) (1994–2001)	Pakistan	(1988–2001) (2007–2019)	Uganda	(1989–1994) (1995–2006)
Cambodia	(1989–1991) (1991–1997)	Indonesia	(1989–1998) (1999–2005)	Peru	(1989–2000)	Ukraine	(1987–2013) (2014–2019)
Cameroon	(1990–2013) (2014–2019)	Iraq	(1989–1990) (1990–1991) (1991–2003) (2003–2011) (2012–2013) (2013–2019)	Philippines	(2014–2019) (1989–2001) (2002–2014) (2014–2019)	Yemen	(1994–1994) (1994–2003) (2003–2011) (2011–2014) (2014–2019)
Chad	(1989–1990) (1991–1996) (1997–2008) (2008–2019)			Rep Congo	(1992–1997) (1997–1999)		(2014–2019) (2000–2008)

Table A3: **Cross-national bivariate probit model comparison.** LRT columns report percent of tests where χ^2 statistic is significant at 95% level.

Model	Likelihood Ratio Test				
	v. 2-probit	v. indep.	v. add.	v. inter.	v. Mundlak
Independent	78.4	—	—	—	—
Additive	77.6	61.9	—	—	—
Interactive	75.4	61.2	23.9	—	—
+ Mundlak	73.9	89.6	86.6	88.8	—
+ Mundlak (2-way)	73.1	88.8	86.6	86.6	26.1

Table A4: **Bivariate probit model comparisons (Soviet-UPA).** LRT columns report χ^2 statistic vs. named nested model. Significance: ** $p < 0.01$, * $p < 0.05$, † $p < 0.1$.

Model	Likelihood Ratio Test				
	v. 2-probit	v. indep.	v. add.	v. inter.	v. Mundlak
Independent	361.1**	—	—	—	—
Additive	374.1**	207.7**	—	—	—
Interactive	371.9**	227.9**	20.2**	—	—
+ Mundlak	320.0**	1379.8**	1172.1**	1151.9**	—
+ Mundlak (2-way)	343.2**	1497.7**	1290.0**	1269.8**	117.9**

Table A5: **Coercive asymmetry estimates (Soviet-UPA)**, bivariate probit. $\hat{\omega}$: coercive asymmetry. $\hat{\omega}_{2p}$: independent probit benchmark. 95% CIs from parametric bootstrap.

Model	Coercive Asymmetry		Fit	
	$\hat{\omega}$ (95% CI)	$\hat{\omega}_{2p}$ (95% CI)	N	AIC
Independent	0.10 (0.08, 0.12)	0.02 (-0.04, 0.08)	20076	27329
Additive	0.17 (0.14, 0.21)	0.02 (-0.04, 0.07)	20076	27125
Interactive	0.18 (0.14, 0.23)	0.02 (-0.04, 0.07)	20076	27109
+ Mundlak	0.17 (0.13, 0.21)	0.02 (-0.04, 0.07)	20076	25961
+ Mundlak (2-way)	0.20 (0.16, 0.26)	0.02 (-0.04, 0.07)	20076	25847