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Determinants of Peace Compliance in Personalist Authoritarian Regimes

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ABSTRACT

Why do some personalist authoritarian regimes comply with peace after initiating interstate conflict, while others do not? Although there is substantial research on why personalist leaders initiate conflicts, a significant gap remains in understanding the conditions under which personalist authoritarian regimes comply with peace. Based on rational choice and selectorate theory, this study analyzes 214 dyad-level conflict episodes involving autocratic initiators between 1960 and 2009. A linear probability model and a Cox proportional hazards model are used to test how sanctions, foreign aid, and military asymmetry shape peace compliance in personalist regimes relative to other autocracies. The analysis shows that durable, intense, elite-targeted sanctions are associated with a lower risk of conflict relapse in personalist regimes than in other autocracies. Contrary to expectations, higher post-conflict foreign aid might be associated with greater relapse risk, suggesting aid may be captured by patronage networks rather than reinforcing peace. Military asymmetry trends do not affect post-conflict compliance differently in personalist regimes. The findings suggest that peace compliance by personalist regimes is associated with how external pressure and resources affect the narrow elite coalitions that sustain personalist rule.

Key words: peace compliance, personalist authoritarian regimes, autocracies, sanctions, conflict relapse, selectorate theory, post-conflict, interstate conflict.

Word count: 15,164

THESIS INTRODUCTION

Global conflicts are on the rise: in 2024, there were 61 inter-state conflicts, the largest number since 1946 (Rustad, 2024). They were mostly driven by authoritarian aggression, contributing to the 18th year of decline in global freedom (Freedom House, 2024). Notably, personalist regimes accounted for 43% of non-democratic governments in 2014 (Geddes et al., 2014). Although most state-based disputes over the last 200 years were resolved through negotiation (Pillar, 1983) and peacefully (Blattman, 2022), negotiating with authoritarian states is harder. There is foundational knowledge of personalist regimes during and after conflict, but little research explaining why they comply with peace. A prominent example is a personalist authoritarian Russia, which has repeatedly participated in peace negotiations yet failed to stop military aggression against Ukraine and other states (Hromenko, 2023; Bohinska, 2022; Snegovaya & Petrov, 2022). Therefore, the **research question** of this paper is: under what conditions does a personalist authoritarian regime comply with peace?

My theoretical framework is based on a gap in understanding peace compliance among personalist states, and **my research is exploratory**. To narrow the scope of the analytical framework, I focus on selectorate theory, which explains how leaders make decisions that capture both public and elite interests. My **argument** is that personalist leaders make cost-benefit calculations when deciding whether to comply with peace. This depends not on formal institutions or norms, but on the selectorate's interest in avoiding conflict.

My theoretical expectations are:

TE1: Personalist regimes experiencing higher cumulative levels of elite-targeted sanctions during and after conflict are more likely to comply with peace than other autocracies. While external sanctions might increase peace compliance across authoritarian regimes, I argue that this is strongest in personalist regimes where resource distribution is concentrated around the leader. If sanctions impose disproportionate costs on elite supporters relative to the benefits the regime provides, elites are more likely to pressure the leader to alter policy. Unlike other autocracies with more institutionalized networks, personalist leaders perceive elite defection as a threat to regime stability and have a stronger incentive to comply with peace.

TE2: Personalist regimes that receive higher levels of aid after the end of the conflict are more likely to comply with peace than other autocracies. Third-party investment may increase the opportunity costs of renewed conflict and shift attitudes toward peace. Unlike in other autocracies, where resource distribution is more institutionalized, in personalist regimes, the selectorate's support is more closely tied to the leader's control over resources. Elites, benefiting from increased aid and investment, will pressure the leader to maintain compliance with the peace.

TE3: Personalist authoritarian regimes whose relative military capabilities improve after the end of the conflict are less likely to comply with peace than other autocracies. While improving military capabilities may increase peace violations across authoritarian regimes, I argue that this is strongest in personalist regimes where leaders exercise direct control over military resources with fewer institutional constraints. Personalist leaders in states with improved military capabilities face lower costs from renewed conflict and often need small battlefield victories to stay in power. Knowing they are militarily stronger than a rival, they will engage in revisionist behavior more often than other autocratic regimes.

To explore these theoretical expectations, I perform **quantitative analysis** using the R statistical programming language. I first formed a sample of personalist authoritarian regimes involved in an interstate conflict between 1960 and 2009 using the Geddes, Wright, and Frantz (GWF) and the Correlates of War Militarized Interstate Disputes dataset (MID) datasets. I then tested these assumptions using three datasets corresponding to theoretical expectations: 1) Global Sanctions Database (GSDB), 2) AidData, and 3) the Composite Index of National Capability (CINC). I performed robustness checks on the model using control variables, alternative measures of personalism, sample restrictions, and different time horizons (ranging from one to ten years after conflict termination).

In terms of **empirical contribution**, my research generates new data on conditions for peace compliance by personalist authoritarian regimes, revealing patterns in compliance and noncompliance and offering insights for international actors seeking stable peace. My research also contributes to negotiation **theory** by proposing a model for engaging personalist regimes in peace negotiations. This model might guide Ukraine's politicians and experts in identifying strategies for negotiations, increasing the likelihood of stable peace.

The paper is structured as follows: the **Introduction** presents the research question, main arguments, theoretical expectations, a description of the data and methods, and contributions to the existing research. The **Literature Review** section contains an academic discussion of personalist authoritarian regimes, identifies a knowledge gap, and justifies the exploratory research design of the study. The **Theoretical Framework** conceptualizes and operationalizes the study's key variables, outlines a theoretical approach grounded in selectorate theory, and presents the theoretical expectations. The **Research Design** outlines the methodological approach and justifies the data sources and methods that link the theoretical framework to the empirical analysis. The **Results** section summarizes the main findings. In the **Discussion**, I explain why some theoretical expectations are upheld and others are rejected, identify limitations, and suggest directions for future research. The **Policy Recommendations** chapter offers policy-relevant insights for designing durable peace with personalist states, with applications to the peace process between Ukraine and Russia. Lastly, the **Conclusion** section presents the study's key findings and highlights the limitations of the methodological approach.

LITERATURE REVIEW

Although authoritarian – and personalist – regimes have been extensively studied, there is a shortage of research on the mechanisms underlying instances of peace compliance by personalist autocracies. Personalist authoritarian leaders stay in power longer because power is concentrated in a single person's hands; therefore, they are less afraid of losing elite support if a promise is not kept (Hadenius & Teorell, 2006; Erzow & Franz, 2011). Personalist regimes, according to this logic, face lower audience costs and are less likely than democracies to honor international agreements. Blattman, Gehlbach, & Yu specifically note that under personalist regimes, the likelihood of civil war increases, although it depends on how scholars measure personalism (Blattman et al., 2025).

Scholars note that authoritarian regimes sign international agreements more often than democracies (Garriga 2009, Chyzh 2014), yet Chyzh (2014) finds that personalist regimes rarely comply with them. She says authoritarian leaders make strategic calculations before signing international treaties: they sign the agreement if it provides them with external recognition, but compliance is nevertheless rare. This builds on Geddes' (1999) idea: "leaders of personalist regimes also negotiate when under pressure from lenders or faced with widespread public protest, but the proportion who renege on the deals they make has been very high" (Geddes, 1999, p. 141). It aligns with the "stabilization hypothesis": "authoritarian incumbents deflect internal and external pressures by liberalising without fully submitting to the uncertainty of democracy" (Bernhard et al., 2020, p. 466). This theoretical framework explains non-compliance, but does not explain what would make compliance the survival-maximizing choice for personalist leaders.

Selectorate theory adds that nondemocratic leaders "have fewer supporters to satisfy", giving them greater freedom in foreign policy decisions and lower accountability for outcomes (Bueno de Mesquita & Siverson, 1995; Bueno de Mesquita et al., 2003, cited in Croco & Weeks, 2016, p. 580; Bueno de Mesquita & Smith, 2009). Overall, bilateral agreements between authoritarian states are more likely than those between states with divergent regimes, as authoritarian regimes face fewer domestic constraints on content (Garriga 2009).

There is substantial research on why regimes initiate wars and how war outcomes are related to the regime's survival. Copeland's (2000) dynamic differential theory shows that states choose highly risky policies, such as war, because leaders fear future decline and select the war strategy if they believe it best protects the state's long-term survival. This is particularly relevant to less constrained personalist regimes. However, Copeland's framework, like most of the literature, is focused on explaining the decision to fight, not the decision to stop.

If authoritarian leaders initiate wars and lose them, it rarely affects the authoritarian leader's tenure, especially in personalist regimes, where there are few *opportunities* and little

desire for the elite and public to punish the leader for the war consequences; the *vulnerability* and *culpability* of the leader is low (Croco & Weeks, 2016). It is also known that the longer a regime is in office, the less likely it is to pursue a peaceful resolution (Farber & Gowa, p. 126). Given the long tenure characteristic of personalist regimes, this might explain why they rarely comply with peace agreements. These studies, however, mostly explain why and when personalist regimes initiate or become involved in conflict. Still, a gap remains in identifying conditions under which personalist regimes honor peace after the interstate wars they initiated.

Research has examined how foreign aid influences the recurrence of insurgent violence mostly in civil conflict. One body of literature suggests that aid development programs reduce violence by “winning hearts and minds” of the people and increasing the opportunity costs of joining an insurgency (Beath et al., 2025). Studies on aid in Afghanistan and Iraq found improvements in economic outcomes, support for the government, and reductions in insurgent violence (*ibid*); a similar result was observed in the Philippines (Crost et al., 2014).

At the same time, the effect of aid on conflict is not uniform. While development aid in Afghanistan reduced violence in most regions, it increased insurgent violence near the Pakistani border, where insurgents did not depend on local populations (Beath et al., 2025). Shipments of U.S. food aid, on average, increase the incidence and duration of civil conflicts, as aid flows lower the pressure to reach a peace settlement; however, these findings did not apply to interstate conflicts (Nunn & Qian, 2014). Some scholars argue that foreign aid keeps unaccountable dictators in power, fueling corruption and prolonging the conflict (Moyo, 2009, in Tierney et al., 2011). Crost, Felter, and Johnston (2014) imply that different types of aid might lead to different effects, and “it is therefore important to understand how and why different types of aid affect conflict” (Crost et al., p. 1834).

Recent scholarship highlights contradictory effects of foreign aid, including on peace (Dreher et al., 2024, p. 10-11). The effects of foreign aid depend on aid sectors, donor motives, the recipient country’s context, and pre- or post-Cold War periods. Related scholarship on aid withdrawals and suspensions also shows mixed outcomes: attempts to penalize governments for corruption or human rights violations frequently fail to induce compliance and may strengthen authoritarian resistance (Cheeseman et al., 2024). In contrast, other scholars show that “aid conditionality entails reducing foreign revenue to dictatorships when they do not liberalize. The loss of aid, in this context, reduces the ability of incumbents to purchase political support” (Escribà-Folch & Wright, 2015, p. 251). They note that the presence of other donors diminishes the effectiveness of this threat. Nowadays, there is even greater concern that the willingness of “emerging” donors (such as Russia, India, Brazil, and China) to compensate for suspended or withdrawn aid may weaken pressure on recipient governments to alter their behavior (Cheeseman et al., 2024, p. 5). That is why the impacts of foreign aid are cautiously interpreted through conditional correlations rather than causal effects. In sum, the studies on the

effectiveness of foreign aid are ambiguous. There are more studies on the effects of aid in civil wars than in interstate conflicts. My research aims to fill this gap.

A similar argument can be made about sanctions. Sanctions are “collective action against a state considered to be violating international law” designed “to compel that state to conform” (Daoudi and Dajani, 1983, pp. 5-8 in Davis & Engerman, 2003, p. 187). The effect of sanctions is also mixed, depending on the political costs of compliance or noncompliance rather than economic costs. Countries are likely to comply with sanctions when the political costs of noncompliance exceed those of compliance; however, if compliance threatens greater political survival costs, resistance to sanctions becomes the optimal choice (Blanchard and Ripsman, 1999 in Hovi et al., 2005, p. 482).

There is a debate on the effectiveness of sanctions. One body of scholarship argues that sanctions can work depending on the goal, and the claim that sanctions do not work is wrong. For example, sanctions with “limited goals, such as the release of a political prisoner, succeeded half the time”, [...whereas] “attempts to change regimes (e.g., by destabilizing a particular leader or by encouraging an autocrat to democratize), to impair a foreign adversary’s military potential, or to otherwise change its policies in a major way succeeded in about 30 percent of those cases” (Hufbauer et al., 2007, p. 158-159). Escribà-Folch & Wright (2015) argue that sanctions are most likely to work in personalist regimes.

There is a body of literature stating that sanctions against authoritarian states rarely work (Peksen, 2009; Demarais, 2022), because they often fail to affect ruling elites directly due to targeting the general population. They may instead strengthen anti-sanction resentment within the population (Demarais, 2022). Peksen (2009) argues that extensive sanctions can actually consolidate authoritarian rule and encourage more repression. This argument could be applicable to personalist regimes where oppression is already very likely.

There is a tension between theoretical expectations and empirical data. The argument that elite-targeted sanctions should work should be plausible under the selectorate theory. If the winning coalition is small and dependent on the leader’s distribution of rents, disrupting that distribution should raise the cost of loyalty and create elite pressure for policy change. The evidence is conflicting. For example, currently, sanctions do not seem to work on Russia – despite years of sanctions, it continues the war against Ukraine. The problem might lie in the goal of these sanctions: the EU itself says the policy objective of its sanctions is “maximising pressure on Russia and using all tools available to diminish Russia’s ability to wage its illegal war of aggression” (Council of the European Union, n.d.), rather than changing Russia’s behavior or regime. This suggests that the conditions under which sanctions work in personalist regimes require more precise theoretical specification, and this study attempts to do so.

Another body of literature within rational choice theory explains how military asymmetry and power shifts incentivize states to initiate conflict. For example, power transition theory predicts that war is likely when a rising power attempts to change the world order through attacking a dominant power (Organski & Kugler, 1980). Gilpin (1981) argues that states try to change the international system when they observe a rival state growing stronger and calculate that acting now is cheaper than waiting. Copeland (2000), through his dynamic-differentials theory, similarly argues that it is precisely the expectation of relative power decline that triggers risky strategies such as war. The military asymmetry, therefore, matters at the dyadic level. When one holds a military advantage over a rival, the costs of renewed aggression are comparatively lower. It is particularly relevant in personalist regimes, where military decisions are not constrained by party or institutional structures, and the leader exercises direct control over the use of force. Battlefield victories can also help legitimize rulers and prolong their political survival (Bueno de Mesquita et al., 2003). That is why a personalist leader who holds military advantage over a target faces lower costs of renewed conflict, fewer institutional constraints on action, and a political logic that rewards revisionist behavior. While the power transition and dynamic differentials literature explains why military asymmetry motivates conflict initiation, it does not address what happens after conflict ends: specifically, whether a personalist leader who retains military superiority following a war has any incentive to comply with peace. This is another analytical gap this study addresses.

Thus, across the broader literature, studies explain why authoritarian leaders fight, why some are more likely to reach agreements, and the conditions under which peace is reached without the conflict recurrence in civil wars. However, three specific gaps remain. First, while the aid literature establishes that post-conflict aid can reduce violence, it predicts so mostly in civil conflicts, but less in personalist interstate conflicts. Second, the effect of sanctions is also ambiguous, and it lacks a theoretical specification of its effectiveness for peace in a personalist regime after the war. Third, while power transition theory explains why military asymmetry motivates conflict, it does not address whether and how military advantage shapes a personalist leader's willingness to comply with peace after conflict has ended. When looking at literature about the Russian-initiated wars, authors tend to focus on how Russia created "negotiation traps" for countries like Ukraine, Moldova, and Georgia (Bohinska, 2022), or simply explain how to win over Russia and why it lost some of its wars, such as the Russian-Japanese one (Hromenko, 2023). They do not answer the question: under what conditions will a personalist authoritarian country comply with peace?

Stepping from that research puzzle, my research question is: **under what conditions (X) does a personalist authoritarian regime comply with peace (Y)?** Given an identified gap in understanding peace compliance among personalist states, **my research is exploratory.**

THEORETICAL FRAMEWORK

The theoretical paradigm chosen for this study is **rational choice theory** (Griggs, 2007). It aligns with previous research on authoritarian regime behavior, allowing the development of new theoretical expectations on peace compliance by building on existing knowledge and a consistent framework. If a political system approach were taken, the research would risk being too broad and focused on authoritarian states in general. Secondly, the rational choice theory approach focuses on regime leaders and examines how they make decisions through **cost-benefit calculations aimed at power maximization**. It surpasses institutional theory, as institutions in personalist regimes are often weak and serve the leader's interests. As Farber & Gowa (1995) noted, even peace between democracies is best explained not through *checks and balances* or *norm-based* approaches, but by their *interests* in avoiding conflict, highlighting the prevalence of rational choice theory in conflict resolution studies.

To further narrow the scope of the analytical framework, I focus on selectorate theory, which explains how leaders make decisions that capture both public and elite interests. The role of the domestic audience is reflected in much literature on the topic (Geddes, 1993; Farber & Gowa, 1995; Hadenius & Teorell, 2006; Bueno de Mesquita & Smith, 2009; Erzow & Franz, 2011; Chyzh, 2014; Croco & Weeks, 2016). The classic selectorate theory says that political leaders aim to maximize their stay in power, which depends on the support of a winning coalition – a sample of people from the leader's electorate (Bueno de Mesquita & Smith, 2009). The smaller the winning coalition, the more advantageous (cheaper) it is for the leader to provide it with private benefits while avoiding policies that benefit the public. In exchange for concessions in certain policies, the winning coalition provides the leader with its support. This theory, however, has limitations, such as focusing on cost-benefit analysis, but diminishing ideological factors.

My theoretical framework is based on an identified gap in understanding peace compliance conditions for personalist states. Considering the main goal of a personalist leader is political survival, my theoretical expectations on peace compliance are based on whether complying with peace helps the personalist leader to preserve power. See the structure of the mechanisms and theoretical expectations below, in Figure 1.

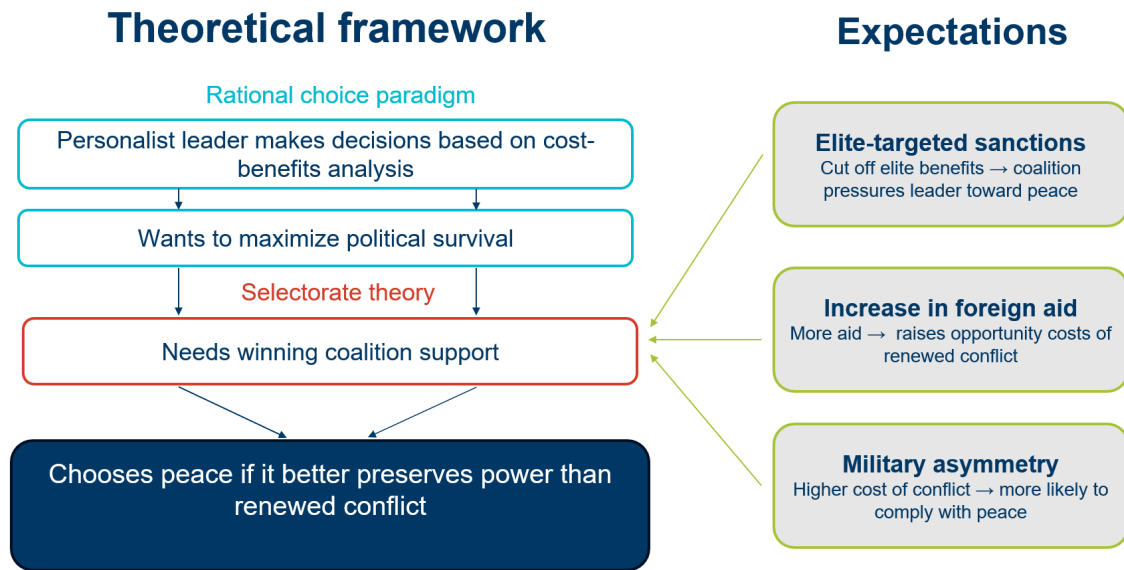


Figure 1. Theoretical framework of the analysis¹

Theoretical expectations

TE1. *Personalist regimes experiencing higher cumulative levels of elite-targeted sanctions during and after conflict are more likely to comply with peace than other autocracies.*

Involvement in a conflict affects the domestic audience in any regime type, often provoking a sense of unity – a phenomenon known as “rally around the flag” (Mueller, 1970). Recognizing this effect, autocratic leaders often initiate conflicts to sustain their power (Weeks, 2014). Personalist regimes maintain elite loyalty through a combination of patronage and coercion: elites depend on access to rents and posts, while defection from a leader can bring severe punishment (Ezrow & Frantz, 2011, pp. 57-58, 117). This logic is especially strong where ruling networks are clientelistic, and elites’ positions depend on the leader, making them likely to resist change (Hadenius & Teorell, 2006, pp. 3-4). Some studies show that support for autocrats might decline after a conflict initiation (Hale & Lenton, 2024; Croco & Weeks, 2016). I expect that high dissatisfaction among the domestic audience can nudge a personalist leader to honor peace in the face of a threat to overthrow the regime. Therefore, if the attacked party, along with its allies, manages to cut off the benefits of the winning coalition in a personalist regime, peace compliance is more likely there than in other authoritarian regimes. Building on Chyzh’s (2014) argument about strategic calculations, I argue that what matters are cumulative sanctions targeting the elite networks surrounding a leader, such as financial and travel sanctions. They can reduce elites’ access to wealth, mobility, and international economic opportunities, thereby weakening the benefits the elite derive from continued conflict. Even

¹ This illustration was created by the author using Microsoft PowerPoint

though elite-targeted sanctions impose costs in institutionalized autocracies, they put greater pressure on personalist regimes, where the selectorate network is narrow.

TE2. *Personalist regimes that receive higher levels of aid after the end of the conflict are more likely to comply with peace than other autocracies*

In selectorate theory, the winning coalition is the key audience ensuring the leader's survival. Post-conflict aid may affect this audience by increasing the material and political benefits associated with the peace period. Aid directed toward the economic and production sectors, for example, may create new opportunities for elites surrounding the leader, raising the costs of renewed conflict and making peace more attractive. In the case of renewed war, these benefits disappear. Moreover, aid could shift preferences of the broader domestic audience by "winning their hearts and minds" (Beath et al., 2025). Elites who benefit from post-conflict aid will have incentives to pressure the leader to preserve peace to maintain continued access to these resources. It makes peace compliance rational for a leader's political survival, as he needs the selectorate's support to sustain his power. This mechanism may be especially important in personalist regimes, where access to resources is concentrated around the leader and ruling coalition rather than institutionalized through formal structures, as in other autocracies.

TE3. *Personalist authoritarian regimes whose relative military capabilities improve after the end of the conflict are less likely to comply with peace than other autocracies.*

Looking at the rational choice paradigm, regimes with stronger military capabilities are more likely to initiate conflicts. Scholars argue that dynamic shifts in relative power, rather than static military capabilities alone, shape incentives for conflict initiation (Copeland, 2000; Giplin, 1981). When a personalist regime improves its relative military position after conflict termination, renewed conflict may appear less costly and more politically beneficial than compliance with peace. This may be especially relevant for personalist regimes, as political power is concentrated in the hands of a single leader and his narrow inner circle. As a result, personalist leaders might be more willing to take military action. They face fewer institutional and political costs than leaders in other autocratic regimes (where decision-making is more collective). On the contrary, when personalist leaders are militarily weaker, peace compliance becomes more likely, assuming they are rational actors who might lose the selectorate's support. Unlike more institutionalized autocracies, where institutional structures can absorb the costs of military failures, personalist leaders bear these costs more directly, making a renewed aggression under military disadvantage a less rational choice.

CONCEPTUALIZATION AND OPERATIONALIZATION

Conflict

Before **conceptualizing** my dependent and independent variables, I define **conflict** as high-intensity interstate violence rather than full-scale war. Following Farber & Gowa (1995), I distinguish between “general wars” (only two, WWI and WWII) and other forms of interstate violence. These researchers refer to general wars as “a clash that involves “one or more system members” and that leads “to a minimum of 1,000 battle fatalities among all of the system members involved.” A member of the international system is defined as a belligerent only if it either committed 1,000 troops to battle or suffered at least 100 casualties” (Small & Singer, 1982, p. 55, quoted in Farber’s & Gowa’s, 1995, p. 131). While this definition identifies full-scale interstate wars, it captures only the most extreme forms of military confrontation. However, battle deaths in interstate wars declined after WWII and especially after the Cold War (Lacina, Gleditsch & Russett, 2006). In addition, lower levels of violence between parties can still be classified as an interstate conflict. Therefore, I broaden my definition of conflict to “use of force” and “war”.

I **measure conflict** through the participant-level data from Version 5 of the Militarized Interstate Dispute (MID) data gathered by the Correlates of War (COW) project, which “provides information about conflicts in which one or more states threaten, display, or use force against one or more other states between 1816 and 2014” (Palmer et al, 2020). This data is used in well-established research in my field (Farber & Gowa, 1995; Croco & Weeks, 2016). I take data from 1960 to 2009 on inter-state disputes. The selection of this time period is solidified by Farber’s & Gowa’s (1995) notion that there was no difference between war likelihood among democracies and other regime types until after WWII; additionally, data on wars between WWI and WWII is unreliable, as these disputes are likely to be parts of big “general wars”.

Within the MID dataset, conflict is operationalized through hostility levels such as “use of force” or “war”. “Use of force” includes such actions as blockade, occupation of a territory, seizure, attack, clash, declaration of war, and use of chemical, biological, and radiological weapons. “War” is defined as the initiation of an interstate war or joining an ongoing interstate war. This analysis captures a broader spectrum of force usage in interstate conflict and provides a larger number of cases for quantitative analysis.

Peace compliance

I **conceptualize** peace compliance, my dependent variable, as the absence of renewed high-hostility interstate violence by the original conflict initiator against the same target within a dyad for 5 years after the end of the conflict. I intentionally limit my definition to the absence of relapse. Accounting for other factors, such as compromises on goals, risks subjective interpretation. By contrast, the absence of renewed violence provides observable cases. I

introduce an agency restriction, specifying the initiating state as the actor under analysis because recurrence of violence initiated by the target state or a third party does not constitute non-compliance by the original initiator. A case is coded as noncompliance only when the same initiator renews high-hostility violence against the same target.

For measuring post-conflict compliance, scholars argue that a two-year (Werner, 1999) or five-year (Walter, 2002) window is a good measure, as it captures whether peace has become stable. Considering the relatively narrow timeframe of my analysis, short time frames are not the most suitable as they shrink the sample size of already rare high-level interstate violence. That is why I use a five-year window as the primary measure of peace compliance. I also test my findings across different compliance windows (ranging from 1 to 5 years).

I do not use an indefinite “ever relapse” horizon as the main model because over time, regime type, leadership, international environment, and other factors change. This might break causal chains between original personalist leader calculus and later conflicts. My approach is also consistent with selectorate theory: elite pressure is expected to operate when the costs and benefits of renewed conflict matter to the current leader and his winning coalition, not across an unlimited period in which the relevant actors may no longer be in power.

I therefore **operationalize peace compliance** as the absence of recurrent high hostility (“use of force” or “war”) within the same initiator-target dyad within 5 years following the termination of the focal conflict episode. Positive coefficients indicate a higher probability of conflict relapse, while negative coefficients indicate greater peace compliance.

Personalist regimes

There are debates in the literature regarding the conceptualization of **personalist regimes**. The discussion revolves around the “classic” typology of personalist, military, and single-party regimes, developed by Geddes (1999), who introduced a personalist regime as a type of its own. While some scholars support this typology (Erzow & Franz, 2011; Croco & Weeks, 2016), others argue that each authoritarian regime exhibits a degree of personalism (Wahman, Teorell, & Hadenius, 2013; Chyzh, 2014). I conceptualize personalist regimes as a distinct regime type, different from other authoritarian forms. Geddes defined personalist regimes as those where “access to office and the fruits of office depend much more on the discretion of an individual leader. The leader may be an officer and may have created a party to support himself. Still, neither the military nor the party exercises independent decision-making power insulated from the whims of the ruler” (Geddes, 1999, pp. 121-122). Erzow and Franz say personalist regimes are those where “access to political office and influence over policy are controlled by a single individual” (Erzow & Franz, 2011, p. 15).

It is worth noting that personalist regimes are often called “patrimonial”, which Weber defined as “pre-modern societies like Imperial China, late Roman Empire and Tsarist Russia

and constructed ideal typical patrimonialism as a state-society nexus consisting of personal hierarchical relationships based on loyalty and mutual services between rulers and ruled and without a strict distinction between public and private (Weber, 1972, pp. 580–653)...” (Weber, 1972 in Becker & Vasileva, 2017, p. 86). Contemporary scholarship similarly emphasizes neopatrimonial features in personalist regimes, such as patron-client networks and informal rule centered around the leader. The concepts are often used interchangeably. This study uses Geddes’ definition because it distinguishes personalist regimes from other forms of authoritarianism.

When it comes to operationalizing autocracy and personalism, the timeframe for available data is, on average, from 1960 to 2014 (Blattman et al., 2025). Since I build on Geddes’ definition of a personalist regime, I will use the Geddes, Wright, and Franz (GWF) dataset for baseline **operationalization**. It captures authoritarian regimes worldwide between 1946 and 2010 and categorizes them as military, party, personalist, and monarchy regimes (Geddes et al., 2014). I use personalism as a dichotomous variable, with all personalist and personal-hybrid regimes coded as 1 and all non-personalist regimes coded as 0.

However, there is significant disagreement over how to measure personalism, and I acknowledge this as a limitation. Blattman, Gehlbach, and Yu (2025) note that the measurement of personalism differentiates across datasets. They present evidence that six personalism measures – GWF Categorical Personalism, GWF Latent Personalism, G&S Power Consolidation, Polity Executive Constraints, V-Dem Presidentialism, and Veto Players – are weakly correlated with each other, with pairwise correlations ranging from 0.12 to 0.46. This is much lower than the correlations among the eight autocracy measures the authors use, which range from 0.73 to 0.92. This suggests that personalism measures are not capturing the same concept, but measure different dimensions of personalism. In particular, the authors criticize the GWF 2014 dataset for “a substantial number of coding gaps in large countries that cannot be accounted for by political transitions or stability” (Blattman et al, 2025, p. 10).

To account for that, I will run alternative models that replace the GWF categorical personalist regime indicator with **two other operationalizations**: the GWF latent personalism measure (Geddes et al., 2018) and a time-varying measure of power consolidation from Gandhi and Sumner (G&S) (2019). The GWF latent personalism measure captures leaders’ connection to coercive institutions. I will run it as a binary variable dichotomized at the mean, and as a continuous variable. I also run the G&S measure as both a binary and a continuous variable. This approach follows the researchers’ recommendation to assess whether results are sensitive to alternative operationalizations of personalist rule.

Tested conditions of peace compliance by a personalist regime

I assume that, if the following conditions are present, they are associated with higher personalist regime compliance with peace relative to non-personalist regimes.

The first condition is cumulative elite-targeted sanctions imposed during and after the conflict, particularly financial sanctions and travel bans. I will use the Global Sanctions Database to measure that, which gathers “all bilateral, multilateral, and plurilateral sanctions in the world during the 1950–2016 period across three dimensions: type, political objective, and extent of success” (Felbermayr et al., 2020, p. 1).

The second is the foreign aid directed to the state that initiated the conflict. For that, I use the AidData dataset, which “includes commitment information for over 1.5 million development finance activities funded between 1947 and 2013” (Tierney et al., 2011; AidData, 2017). The primary aid measure is the logged level of post-conflict aid normalized by GDP, capturing the economic importance of aid relative to the recipient economy’s size. As an exploratory test, I will identify what type of aid matters most.

The final condition is post-conflict military asymmetry between the initiator and the target, measured dynamically rather than as a static balance. I examine whether higher ratios of military post-conflict capabilities are associated with lower peace compliance in personalist regimes compared to other autocracies. To do that, I use Version 6.0 of the National Material Capabilities (NMC) data gathered by the COW Project. The NMC includes information on the military, industrial, and demographic capabilities of each state, which is summed into the Composite Index of National Capability (CINC) (Signer et al., 1972; Signer, 1988).

RESEARCH DESIGN

I use quantitative research to compare conflict episodes across time and estimate whether differences in sanctions, aid, and military asymmetry are associated with conflict relapse. Researchers emphasize that quantitative methods help identify the most relevant cases for studying nominal variables and then focus on qualitative research of these cases (Collier et al., 2003, p. 6). This is consistent with exploratory sequential mixed-methods, when the researcher begins with quantitative analysis and then builds on those findings through qualitative research to provide a deeper explanation (Creswell & Creswell, 2018, p. 52). My findings could serve as a baseline for future qualitative research.

Data operationalization

My **dependent variable** (Y, **Peace compliance**) is operationalized in two ways.

The first is **binary**, where not relapsing into conflict within five years of the episode's end (*#relapse_5yr*) is coded as 0, and 1 if the initiating state re-initiates high-hostility aggression (levels 4 or 5) against the same target within 5 years.

The second approach is **continuous**, measuring the number of days between the end of the previous conflict episode and the start of the next one (*#time_to_relapse_days*), which serves as the time-to-event variable in the Cox proportional hazards model. This variable is right-censored at five years following conflict termination.

My independent variables are:

- 1) **Personalist regimes** (*#initiator_personalist*), a **binary variable** (0 or 1), are defined through the GWF autocratic regimes dataset. I restrict my sample to autocratic initiators of the conflict and compare personalist autocracies to other autocracies. If I were to broaden the sample to personalist regimes against democracies, these groups would have too different baseline characteristics, whereas authoritarian regimes are more similar to each other. To define initiator states, I rely on the “side A” variable in the MID 5.0 dataset. Sometimes side A (treated as the “initiator”) might be coded as such because it responded to non-codable militarized actions by side B. This cannot be fully resolved; this error is common in large-n datasets. It is assumed that it applies equally across all regimes and is unlikely to cause systematic bias.
- 2) **Elite sanctions intensity** (*#elite_sanctions_intensity*), measured as a **continuous variable**, captures the cumulative pressure of elite-targeted sanctions on the initiating state during the conflict and for five years after its end. Sanctions cumulative intensity is measured through the following mechanism: within each year of the conflict and post-conflict window, financial and travel restrictions each contribute 0.5 score per

initiator. Each case is weighted by sender tier: intergovernmental organizations (UN, EU, African Union, OAS, etc.) receive weight 2.0 because their broad membership makes evasion through third countries difficult; coalitions of three or more named states receive weight 1.5; bilateral senders receive weight 1.0. These scores are summed across all sanctioning cases into a single cumulative intensity score.

- 3) **Aid** (*#aid_gdp_post_log*) is measured as the **logged level** of post-conflict aid normalized by GDP, calculated as $\log(1 + \text{mean aid/GDP})$ averaged over the five years following conflict termination. Aid data are drawn from AidData and GDP data from the World Bank World Development Indicators. This measure captures the economic significance of aid flows relative to the recipient economy's size, reflecting how much elites risk losing if renewed conflict prompts donors to withdraw assistance.
- 4) **Military capabilities trend** (*#milasym_trend_5yr*), a **continuous** variable measuring the trajectory of the initiator's military position relative to the target over the 5 years following conflict termination. It is estimated as the slope coefficient from a regression of the logged CINC ratio on years one through five after the conflict. I use NMC data to measure it.

I also control for a set of factors that may influence the likelihood of compliance with peace. The **controls** are:

- 1) **Episode duration** (*#episode_duration_days*), measured in days of the conflict duration.
- 2) **Democratization** (*#dem_change*), to account for political transformations that might have occurred during or in the aftermath of the conflict. The democracy change is calculated as the difference between the mean polyarchy score over the five years following conflict termination and the mean over the five years preceding conflict onset. I use the V-Dem v2x_polyarchy indicator, which ranges from 0 (least democratic) to 1 (most democratic).
- 3) **GDP per capita** (*#log_gdp_pc_pre*), as recommended by Garriga's (2009) research design about regime types and bilateral treaty formalization. I take numerical GDP per capita data for each country from the World Bank. The reference period for the World Bank data is 1960-present, which justifies the timeframe of my study. Countries with higher levels of economic development (higher GDP per capita) may face higher opportunity costs from non-compliance, as renewed conflict can undermine trade and investment. GDP per capita is averaged over the five years preceding conflict onset to capture pre-conflict economic conditions.
- 4) **Sharing a border** (*#shared_border*) is a **dichotomous** variable, which takes the value of 1 if the two states are contiguous and 0 otherwise. Contiguous states might re-initiate

conflict more often, as geographic proximity creates more territorial disputes and more opportunities for invasion.

- 5) **Regime duration** (*#regime_duration*) is a **continuous** numerical variable. It is measured through the number of years the autocratic leader had been in power at the time the conflict episode ended. The variable is constructed from the Geddes, Wright, and Frantz (GWF) Autocratic Regimes dataset, which I also use to measure personalism.
- 6) **Prior conflicts** (*#prior_conflicts*), operationalized as a **continuous** variable counting how many militarized confrontations between the initiating state and the target state occurred before the current conflict episode. The variable is constructed from the MID dataset. I include disputes that started between 1946 and 2009 with a hostility level of 4 or 5. I account for the pre-1960 period to capture any potential conflicts that occurred before my sample analysis. I focus only on the post-World War II period (after 1946) for this variable.
- 7) **Cold War** (*#cold_war*), since the literature argues that the nature of the war changed after the Cold War. It is operationalized as a binary indicator coded 1 for conflict episodes initiated before 1990 and 0 for episodes initiated from 1990 onward.
- 8) **Military asymmetry ratio** (*#milasym_ratio*), a **continuous** variable measured as the logged ratio of the initiator's CINC score to the target's CINC score at the time of conflict onset. It is constructed from the National Material Capabilities (NMC) dataset. I add it to control for baseline differences in military capability between the initiator and target at the start of the conflict episode. Including the static capability ratio ensures that the interaction in TE3 captures dynamic post-conflict shifts in relative power from the onset of the conflict, rather than baseline military advantage.

Population and unit of analysis

The general population consists of all interstate militarized disputes involving autocratic regimes between 1960 and 2009. I created a categorical variable, *#initiator_personalist*, in which all personalist regime types (*party-personal*, *party-personal-military*, *personal*) were coded as "1", and other autocratic regimes were coded as "0".

I restrict the end date to 2009 because the MID dataset ends in 2014, providing at least 5 years after the end of the conflict to ensure consistent overlap across all datasets used in the analysis. Some countries were coded as personalists during multiple periods, which required accounting for regime changes over time rather than treating countries as static cases. That is why I expanded regime types into a *country-year* format for further analysis.

The conflict data were drawn from the Militarized Interstate Disputes (MID) v5.0 dataset from the Correlates of War (COW) project. I used the **participant-level dataset**, which allows identification of individual country involvement in disputes and facilitates merging with the country-year regime data. I restricted the analysis to **high-intensity interstate conflict**, corresponding to hostility levels 4 (use of force) and 5 (war) in the COW coding scheme.

Country names were harmonized across two datasets. The MID participant-level data were merged with the GWF country-year regime data, matching conflict participation to the regime classification for each year. For each conflict dyad, consecutive days of high-intensity conflict were treated as a single conflict spell; if the gap between the end of one conflict spell and the start of the next exceeds 1 day, a new episode is coded. I also recorded whether a subsequent episode occurred within the same dyad, which state initiated it, and the number of days between the end of one episode and the start of the next.

The unit of analysis is the dyadic **conflict episode**, and the initiator is identified using Side A coding from the MID dataset. The sample includes only episodes in which the initiator is an autocratic regime in the GWF dataset.

Quantitative data analysis

I used R for quantitative data analysis. The quantitative approach allows for the comparison of many conflict episodes across different times. It estimates whether differences in the duration and type of sanction, aid flows, and military asymmetry are associated with lower conflict relapse. It also allows testing relationships between variables using interactions, which might reveal joint significance for the dependent variable and for controlling for other variables. The limitation of the approach is potential omitted variable bias, data coding error, and measurement error. I account for that by using control variables, and my research should be treated as exploratory rather than confirmatory.

Chi-square test

My baseline is determining the relationship between my dependent variable (peace compliance, *#relapse_5yr*) and my main indicator variable (personalist regime, *#initiator_personalist*). For that, I perform a **chi-square test**, which is appropriate when both variables are binary. This test serves as a descriptive baseline to assess whether personalist and non-personalist autocracies differ in overall relapse rates before examining the conditional effects of sanctions, aid, and military asymmetry.

LPM with no controls

The first regression model is a Linear Probability Model (LPM) with peace compliance as the dependent, along with their interaction variable (*#relapse_5yr*), and independent

variables including *#initiator_personalist*, *#elite_sanctions_intensity*, *#aid_gdp_post_log*, and *#milasym_trend_5yr*; and the interaction of the latter three with personalism.

In the model, I test whether the interaction between personalist regimes and explanatory variables affects personalist regimes differently from other authoritarian states. The total effect of sanctions for personalist regimes equals $\beta_2 + \beta_5$, where β_2 is the effect of sanctions for *non-personalist autocracies*, and β_5 is the effect of sanctions for personalist regimes. The same applies to aid ($\beta_3 + \beta_6$) and military trends ($\beta_4 + \beta_7$). The error term is represented by u . The equation is as follows:

$$\begin{aligned} \text{Peace Compliance} = & \beta_0 + \beta_1 \text{Personalist Regime} + \beta_2 \text{Sanctions} + \beta_3 \text{Aid} + \beta_4 \text{MilAsymTrend} \\ & + \beta_5 (\text{Personalist} \times \text{Sanctions}) \\ & + \beta_6 (\text{Personalist} \times \text{Aid}) \\ & + \beta_7 (\text{Personalist} \times \text{MilAsymTrend}) \\ & + u \end{aligned}$$

The limitation of the model is that it predicts probability outside of the 1 or 0 range, and errors are heteroskedastic – they vary with x . To address this, I use robust standard errors (Huber-White sandwich estimators) when running a model rather than regular standard errors.

LPM with controls

The baseline model remains the same, but I now add more controls and cluster standard errors by dyad to account for non-independence of observations within dyads. I do not use fixed effects because the data are structured at the conflict-episode dyad level, not a panel. Fixed effects would absorb between-dyad variation, which is where the identifying variation in personalism, sanctions, and aid lies. The equation is:

$$\begin{aligned} \text{Peace Compliance} = & \beta_0 + \beta_1 \text{Personalist} + \beta_2 \text{Sanctions} + \beta_3 \text{Aid} + \beta_4 \text{MilAsymTrend} \\ & + \beta_5 (\text{Personalist} \times \text{Sanctions}) \\ & + \beta_6 (\text{Personalist} \times \text{Aid}) \\ & + \beta_7 (\text{Personalist} \times \text{MilAsymTrend}) \\ & + \beta_8 \text{ColdWar} \\ & + \beta_9 \text{EpisodeDuration} \\ & + \beta_{10} \text{DemChange} \\ & + \beta_{11} \text{GDPpc} \\ & + \beta_{12} \text{RegimeDuration} \\ & + \beta_{13} \text{SharedBorder} \\ & + \beta_{14} \text{Prior Conflicts} \\ & + \beta_{15} \text{MilitaryRatio} \\ & + u \end{aligned}$$

Cox proportional hazards survival model

I compare all LPM models to a Cox proportional hazards survival model, treating the number of days between the end of a conflict episode and the start of the next as the outcome variable. It captures *when* the relapse occurred rather than just *whether* relapse occurred, which matters because dyads differ in how quickly relapse follows conflict termination. The Cox model includes the same independent variables, interactions, and controls as the LPM model, and is right-censored at 5 years following conflict termination, consistent with the binary models. Observations where no relapse occurs within this window are treated as censored.

The Cox model estimates hazard ratios: a positive coefficient indicates a higher relapse hazard (less compliance), and a negative coefficient indicates a lower relapse hazard (greater compliance). The proportional hazards assumption is tested using Schoenfeld residuals. Independence of observations within dyads is addressed through dyad-clustered standard errors. The equation is:

$$\begin{aligned}
 h(t) = h_0(t) \cdot \exp(&\beta_1 \cdot \text{Personalist} + \beta_2 \cdot \text{Sanctions} + \beta_3 \cdot \text{Aid} + \beta_4 \cdot \text{MilAsymTrend} \\
 &+ \beta_5 \cdot (\text{Personalist} \times \text{Sanctions}) \\
 &+ \beta_6 \cdot (\text{Personalist} \times \text{Aid}) \\
 &+ \beta_7 \cdot (\text{Personalist} \times \text{MilAsymTrend}) \\
 &+ \beta_8 \cdot \text{ColdWar} \\
 &+ \beta_9 \cdot \text{EpisodeDuration} \\
 &+ \beta_{10} \cdot \text{DemChange} \\
 &+ \beta_{11} \cdot \text{GDPpc} \\
 &+ \beta_{12} \cdot \text{RegimeDuration} \\
 &+ \beta_{13} \cdot \text{SharedBorder} \\
 &+ \beta_{14} \cdot \text{PriorConflicts} \\
 &+ \beta_{15} \cdot \text{MilAsymRatio}
 \end{aligned}$$

Where $h(t)$ is the hazard of relapse at time t , given that it has not relapsed yet.

Robustness checks

Alternative personalism measure

I replaced the GWF categorical personalist regime indicator with **other operationalizations**: GWF latent personalism score (source) and time-varying measure of power consolidation from Gandhi and Sumner (G&S) (2020).

The GWF latent personalism score is tested both as a continuous score and as a binary measure dichotomized at the sample median of 0.427. Second, I use continuous scores, capturing gradations in personalism across the full range to avoid losing potentially important

information introduced by dichotomization. I also use the G&S power consolidation measure. Like Blattman, Gehlbach, and Yu (2025), I dichotomize it at 0 and convert it to a **binary** variable, where all values above 0 are coded as personalist states (*#level_personalist*). Besides binary testing, I also test it as a continuous variable.

Conservative Specification

A potential concern with the dyadic operationalization is that multilateral conflicts generate multiple observations from a single conflict episode, which undermines independence. To address this, I collapse all multilateral conflict episodes to a single observation per initiating state per conflict episode, retaining the dyad involving a shared border where one exists, and the alphabetically first target otherwise. Another concern is that 36 of 214 episodes in the main sample lasted only 1 day and were brief incidents, even though they had a hostility level of 4 or 5. I also excluded these episodes in my conservative model.

Alternative Sanctions and Aid Operationalisation

For sanctions, in addition to the primary elite sanctions intensity measure, I separately estimate models that distinguish between bilateral and multilateral elite sanctions. I also estimate a specification using a broader sanctions-intensity measure that incorporates trade, military, and other sanctions categories, in addition to elite-targeted sanctions. For aid, I estimate two alternative specifications: post-conflict aid per capita (logged) and the logged change between pre-conflict and post-conflict aid/GDP ratios.

Aid Sectors and Conservative Specification

As part of my exploratory analysis, I test what type of aid (if any) drives the results. I sorted aid data into sectors and ran separate models for the following types of aid:

- social services (education and health);
- governance (government and civil society, including conflict prevention and resolution, peace and security);
- economic and productive sectors (economic infrastructure and services; production sector; industry, mining, construction);
- humanitarian aid;
- budget/debt relief (coded as “commodity aid and general program assistance”);
- and other multisector aid (including infrastructure and services, environmental protection, women, land management, and urban development).

Such classification is based on the AidData coding scheme. I ran a model estimating the effect of these different types of aid on conflict relapse on my conservative sample ($N = 152$).

Pre-conflict Aid and Conservative Specification

One concern with aid effects is that it may be directed toward more stable regimes where relapse is unlikely, creating endogenous effects. As a robustness check, I estimate models using only pre-conflict aid levels on a restricted sample that excludes episodes in which the same dyad had experienced a prior high-intensity conflict within five years of the onset, drawing on conflict history from 1946 onward. The sanctions variable was excluded from this specification.

Alternative Time Horizons

To test whether findings are sensitive to the choice of compliance window, I re-estimate the primary model across cumulative post-conflict windows from Year 1 through Year 5. This tests whether the effects of sanctions and aid on personalist regime compliance emerge gradually or are stable across the post-conflict period. The analysis uses the full sample ($N = 214$) with the same controls and dyad-clustered standard errors as the primary model.

Uncensored Relapse

As an additional check, I re-estimate the primary LPM, replacing the five-year binary outcome with an uncensored relapse indicator that captures any subsequent conflict initiation by the same state against the same target, regardless of timing. This tests whether the main findings are sensitive to the choice of observation window.

Sensitivity Check on Conflict Outcome

As a final sensitivity check, conflict outcome is included as a control variable. Conflict outcome is not included in the main models because it may be post-treatment: sanctions and aid could affect both how conflicts end and whether conflict relapse occurs. Including outcome in the main specification could therefore obscure the mechanisms I study. Nevertheless, outcome controls are examined as a sensitivity check.

Where sample structures allow (e.g., in alternative time horizons, alternative independent variable measurements, or uncensored relapses), these models use the same baseline controls and dyad-clustered standard errors as the primary specification.

RESULTS

Descriptive statistics and balance table

Descriptive statistics for all variables are presented in Table 1. The analytical sample consists of 214 dyad-episodes of conflict. Observations were excluded when data were unavailable for any of the model variables (for example, military capability, GDP, or aid data). Conflict relapse occurs in 28% of episodes within five years, and personalist regimes account for 65% of initiators.

The elite sanctions intensity is right-skewed (mean = 6.86, median = 2.00), indicating that most cases face moderate sanctions pressure. The maximum corresponds to Myanmar (2004, 52) and Sudan (2004, 49.5), both of which are personalist regimes.

Post-conflict aid has a mean of 0.05, which indicates modest average aid inflows relative to GDP (about 5%), with considerable variation (SD = 0.08). The minimum of 0 corresponds to cases with no recorded aid flows (China across multiple episodes in the 1960s, Saudi Arabia, Portugal). The most aid was received by Mozambique following the 1983 conflict (aid equivalent to 56% of GDP, aid_gdp_post_log = 0.44), Honduras following 1976 (aid equivalent to 55% of GDP), and Somalia across several episodes in the mid-1970s (aid equivalent to 40-45% of GDP).

The trend in military asymmetry averages around 0.01, indicating that the military balance between former adversaries remains stable. The largest positive trend corresponds to Eritrea against Yemen (1995) and Iraq against Kuwait (1990 and 1991). The most negative trends correspond to Portugal against Tanzania in 1972, and Tajikistan against Afghanistan in 1993-1994.

64% of episodes occur during the Cold War. Democracy change averages near zero (0.02), reflecting the autocratic composition of the sample. Log GDP per capita averages 7.1 ($e^{7.1}=1.212$, approximately \$1,200 in constant 2000 USD), and is widely dispersed. Prior conflicts average 2.49 per dyad. Regime duration (how long the initiating regime had been in power at the time of conflict termination) averages at 16.8 years with large variation. Approximately 74% of country dyads share a border. The military asymmetry ratio at conflict onset averages 0.21, indicating that initiators were on average slightly stronger than their targets, though variation is large. The largest positive value corresponds to South Africa against Botswana (4.73), and the largest negative value corresponds to Panama (-6.16) and Libya (-4.43) against the US.

Table 1: Descriptive Statistics (N = 214)²

Variable	N	Mean	SD	Min	Median	Max
<i>Relapse Event</i>	214	0.28	0.45	0.00	0.00	1.00
<i>Personalist Regime</i>	214	0.65	0.48	0.00	1.00	1.00
<i>Elite Sanctions Intensity</i>	214	6.86	10.60	0.00	2.00	52.00
<i>Post-conflict Aid (log, % GDP)</i>	214	0.05	0.08	0.00	0.02	0.44
<i>Military Asymmetry Trend</i>	214	0.01	0.07	-0.27	0.00	0.25
<i>Military Asymmetry Ratio</i>	214	0.21	1.76	-6.16	0.26	4.73
<i>Episode Duration (days)</i>	214	303.58	582.06	1.00	145.50	4,677.00
<i>Cold War</i>	214	0.64	0.48	0.00	1.00	1.00
<i>Democracy Change</i>	214	0.02	0.08	-0.30	0.00	0.44
<i>Log GDP per Capita</i>	214	7.10	1.20	5.15	6.99	10.90
<i>Prior Conflicts</i>	214	2.49	3.40	0.00	1.00	18.00
<i>Regime Duration</i>	214	16.79	15.64	0.00	11.00	77.00
<i>Shared Border</i>	214	0.74	0.44	0.00	1.00	1.00

Unit of analysis: dyad-level conflict episode. Sample restricted to autocratic initiators, 1960-2009.

Table 2 shows a balanced difference in means between personalist and non-personalist autocratic initiators, so groups are plausible for comparison. The most observable characteristics, such as relapse rates, aid flows, episode duration, occurrence during the Cold War, GDP per capita, prior conflicts, and shared border coding, are similar.

However, the groups differ on three measures. Regime duration at episode onset differs substantially (23.9 vs 13.0 years), consistent with theory on authoritarian regimes: more institutionalized autocracies tend to survive longer, whereas personalist regimes depend on the longevity of one leader. The military asymmetry ratio differs (0.47 vs 0.08). This may reflect the composition of non-personalist autocracies: they may have stronger militaries, whereas personalist regimes keep the army less powerful to reduce the threat of a military coup against the leader. Lastly, personalist regimes are somewhat less likely during the Cold War (59% vs. 72%).

² Each table in my analysis is based on my calculations in R based on MID 5.0, GWF Autocratic Regimes, AidData Core Research Release v3.1, NMC v6.0, and other datasets. See Research Design for variable operationalization.

Table 2: Conflict Relapse and Regime Type Relations

	Non-Personalist (N=74)		Personalist (N=140)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
<i>Relapse Event</i>	0.27	0.45	0.29	0.45	0.02	0.06
<i>Elite Sanctions Intensity</i>	6.73	11.73	6.93	9.99	0.20	1.60
<i>Post-conflict Aid (log, % GDP)</i>	0.04	0.08	0.05	0.07	0.01	0.01
<i>Military Asymmetry Trend</i>	0.00	0.06	0.01	0.08	0.00	0.01
Military Asymmetry Ratio	0.47	1.73	0.08	1.77	-0.39	0.25
Episode Duration (days)	283.86	628.85	314.01	557.81	30.14	86.99
Cold War	0.72	0.45	0.59	0.49	-0.12	0.07
Democracy Change	0.02	0.08	0.02	0.08	-0.01	0.01
Log GDP per Capita	7.13	1.49	7.08	1.03	-0.05	0.19
Prior Conflicts	2.18	2.46	2.65	3.80	0.47	0.43
Regime Duration	23.89	20.20	13.04	10.93	-10.86	2.52
Shared Border	0.78	0.41	0.72	0.45	-0.06	0.06

Note: Diff. in Means = Personalist minus Non-Personalist.

Chi-square test results

Table 3 shows the baseline chi-square test results for personalist regime and conflict relapse. Among non-personalist regimes, 27.0% of episodes result in relapse within a 5-year window, compared to 28.6% among personalists. The difference is small and statistically insignificant (Chi-square=0.006, $p=0.94$), so personalist regimes do not re-initiate conflicts more than non-personalist. This motivates the interaction approach in the main models, which tests whether sanctions, aid, and military asymmetry affect relapse differently.

Table 3: Relapse by Regime Type (Chi-square = 0.006, $df = 1$, $p = 0.94$)

Regime Type	No Relapse	Relapse	Total	Relapse Rate
Non-Personalist (0)	54	20	74	27%
Personalist (1)	100	40	140	28.6%

LPM and Cox proportional hazard models results

Table 4 shows three models estimating the probability of conflict relapse. Model 1 is an LPM with the main variables of interest and their interactions with personalism; Model 2 adds controls and uses dyad-clustered standard errors. Model 3 is a Cox proportional hazards model that accounts for the time difference between conflict relapses within each dyad.

Table 4: Main Results. Personalism and Conflict Relapse

	LPM (No Controls)	LPM (Controls)	Cox PH (Controls)
Intercept	0.232*** (0.065)	0.129 (0.297)	
Personalist Regime	0.079 (0.087)	0.029 (0.089)	-0.055 (0.446)
Elite Sanctions Intensity	0.009 (0.006)	0.007* (0.003)	0.012 (0.017)
Post-conflict Aid (log, % GDP)	-0.537 (0.333)	-0.755† (0.389)	-8.259 (7.144)
Military Asymmetry Trend	-0.725 (0.768)	-0.668 (0.671)	-4.288 (4.721)
Personalist × Sanctions Intensity	-0.018** (0.007)	-0.014*** (0.004)	-0.051* (0.031)
Personalist × Aid (log, % GDP)	1.116 (0.703)	1.207* (0.581)	10.024 (7.266)
Personalist × Military Asym. Trend	1.065 (0.922)	0.895 (0.718)	6.048 (5.223)
Military Asymmetry Ratio		0.010 (0.017)	0.077 (0.102)
Episode Duration (days)		0.000 (0.000)	0.000 (0.000)
Cold War		-0.014 (0.069)	-0.142 (0.309)
Democracy Change		-0.418 (0.379)	-2.290 (2.052)
Log GDP per Capita		0.003 (0.039)	-0.060 (0.163)
Prior Conflicts		0.015 (0.010)	0.057† (0.034)
Regime Duration		-0.003 (0.003)	-0.014 (0.012)
Shared Border		0.178* (0.069)	1.188* (0.496)
N	214	214	214
R ²	0.068	0.140	
Adj. R ²	0.036	0.074	
Std. Errors	HC2	Clustered (dyad)	Clustered (dyad)

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All three models show consistency in the sanction interaction. The LPM without controls shows the sanctions interaction is significant at the 1% level ($\beta = -0.018$, $p = 0.007$). LPM with controls maintains this result ($\beta = -0.014$, $p < 0.001$), and the Cox model confirms it at the 5% level ($\beta = -0.051$, $p = 0.040$). The aid interaction is not significant in the no-controls model ($\beta = 1.116$, $p = 0.114$), but becomes significant at the 5% level once controls are added ($\beta = 1.207$, $p = 0.039$). It does not reach conventional significance in the Cox model ($p = 0.130$). Adding controls increases the share of explained variation from 4% to 7% (adjusted R^2). The interaction between the military asymmetry trend is not significant in any model. For the Cox model, the proportional hazards assumption was tested using Schoenfeld residuals. The global test confirms the assumption holds for the model as a whole ($p = 0.447$).

TE1. *Personalist regimes experiencing higher cumulative levels of elite-targeted sanctions during and after conflict are more likely to comply with peace than other autocracies.*

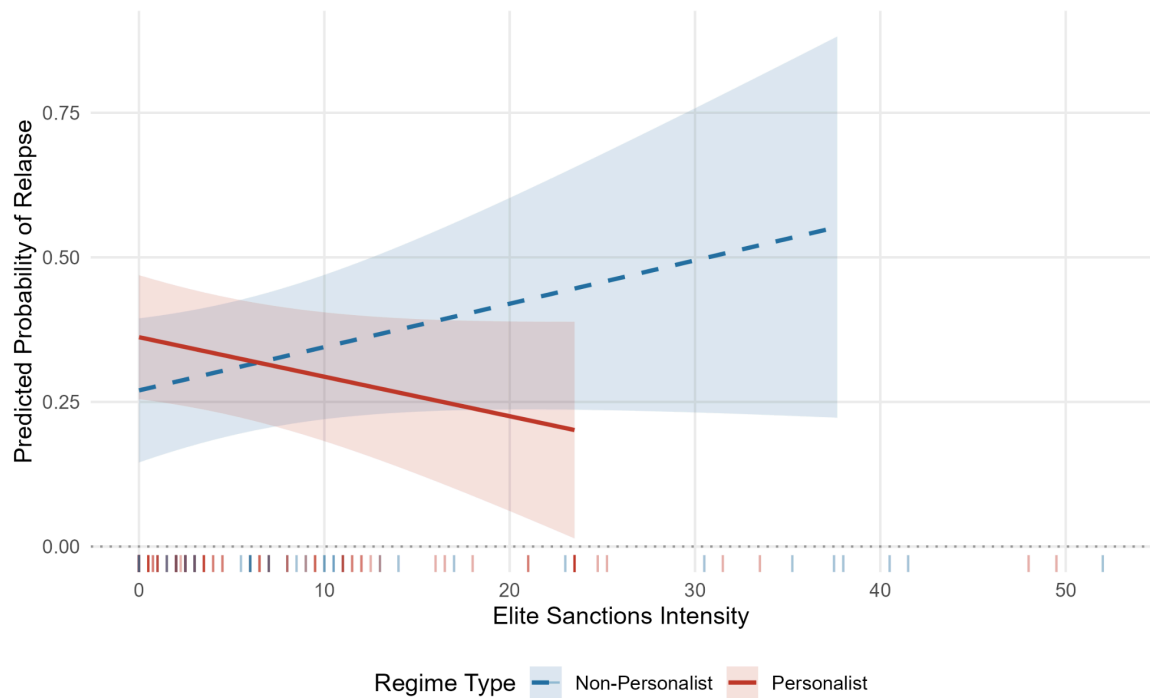
TE1 is supported. At the 1% significance level in the LPM model and at the 5% level in the Cox model, personalist regimes are more likely to comply with peace when elite-targeted sanctions are imposed. For non-personalist autocracies under the LPM model, each additional unit of sanctions intensity is associated with a 0.7 percentage point increase in the predicted probability of relapse, holding all other variables constant. The positive association with non-personalist regimes suggests that elite-targeted sanctions fail to deter conflict relapse in institutionalized autocracies and might even destabilize the post-conflict environment. This is consistent with selectorate theory: sanctions have weaker leverage because winning coalitions in party-based or military regimes are larger.

For personalist regimes, the interaction term reduces the predicted probability of relapse by 1.4 percentage points relative to non-personalist regimes. The net effect is:

$$\text{Net effect} = 0.007 + (-0.014) = -0.007$$

This means each additional unit of sanctions intensity is associated with a 0.7 percentage point decrease in relapse probability for personalist regimes. Figure 2 shows the difference: the red line for personalist regimes slopes downward as sanctions increase, while the blue line for non-personalist regimes slopes upward. Wide confidence intervals at high sanctions intensity levels reflect few observations in this range and should be interpreted with caution.

Figure 2. Predicted Relapse Probability by Elite Sanctions Intensity³



The Cox model confirms the LPM results and shows how quickly relapse is likely to occur after conflict ends. The calculations are:

$$\begin{aligned} \text{Hazard Ratio for non-personalist} &= e^{0.012} = 1.012 \\ (1.012 - 1) * 100\% &= 1.2\% \end{aligned}$$

For non-personalist regimes, each additional unit of elite sanctions intensity is associated with a 1.2% higher hazard of conflict relapse, meaning the risk of relapse at any moment is 1.2% higher compared to a non-personalist regime facing one fewer unit of sanctions intensity, conditional on not having relapsed yet. For personalist regimes, the interaction term is:

$$\begin{aligned} \text{Hazard Ratio for personalist} &= e^{(-0.051)} = 0.950 \\ (0.950 - 1) * 100\% &= -5\%. \end{aligned}$$

The interaction term indicates that sanctions reduce the hazard of relapse by 5% more in personalist regimes than in non-personalist regimes. The net effect of sanctions for personalist regimes is:

$$\text{Hazard Ratio} = e^{(\beta_{\text{sanctions}} + \beta_{\text{personalist}} \times \text{sanctions})}$$

³ Source: author's own calculations based on the Global Sanctions Database (Felbermayr et al., 2020), GWF Autocratic Regimes Dataset (Geddes, Wright & Frantz, 2014), and MID v5.0 (Palmer et al., 2020). Predictions generated from LPM with controls, holding all continuous variables at their mean and binary variables at their modal value. Shading represents 95% confidence intervals.

$$\text{Hazard Ratio} = e^{(0.012 + (-0.051))} = e^{-0.039} = 0.962$$

$$(0.962 - 1) * 100 = -3.8\%$$

In other words, while sanctions increase the relapse hazard by 1.2% for non-personalist regimes, they reduce it by 3.8% for personalist regimes.

TE2. *Personalist regimes that receive higher levels of aid after the end of the conflict are more likely to comply with peace than other autocracies*

TE2 is not supported in the expected direction in the main model. At the 5% significance level in the LPM, higher aid to personalist regimes is associated with increased probability of conflict relapse. The Cox model shows the same direction as the LPM but does not reach conventional significance.

The LPM model shows that for non-personalist regimes, the main effect of aid change is negative and significant ($\beta = -0.755$, $p = 0.054$). Each one-unit increase in logged post-conflict aid as % of GDP is associated with a 75.5 percentage point decrease in relapse probability, holding all other variables constant. The interaction term ($\beta = 1.207$, $p = 0.039$):

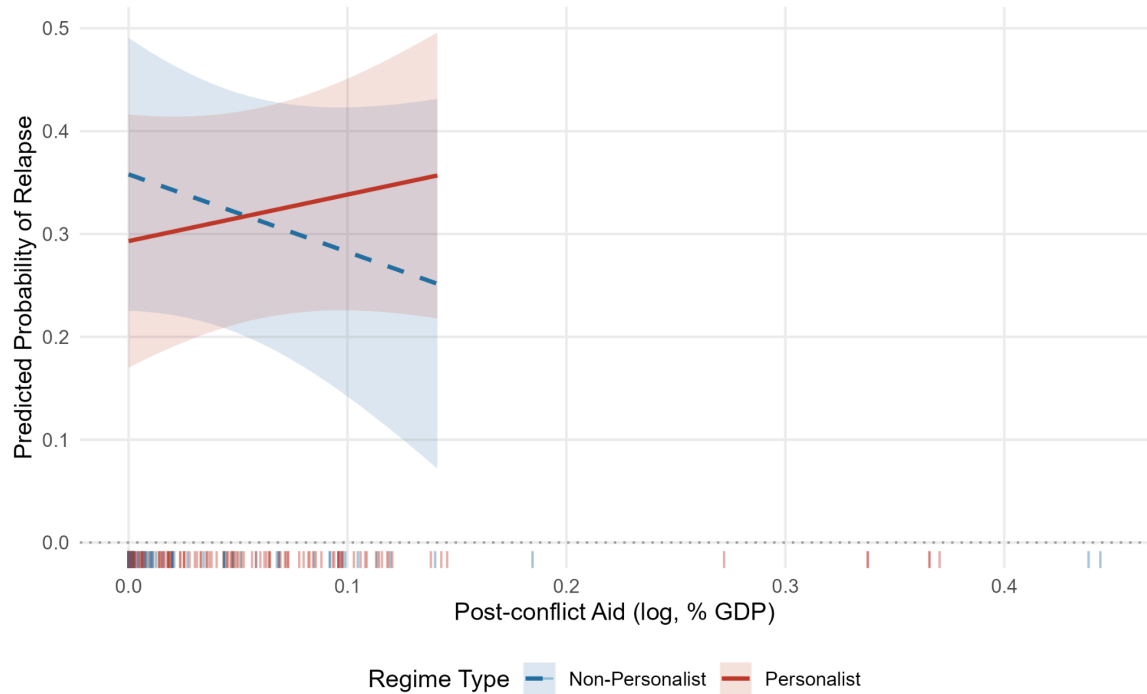
$$\text{Net effect} = -0.755 + 1.207 = 0.452$$

A one-unit increase in logged post-conflict aid is associated with a 45.2 percentage point increase in relapse probability for personalist regimes. Figure 3 illustrates it. The lines cross at low aid levels, after which the predicted relapse probability for personalist regimes continues to rise while it decreases for non-personalist regimes. Wide confidence intervals at higher aid values reflect few observations in this range and should be interpreted with caution.

These findings contradict the original prediction of TE2, which assumed that greater aid flows would raise the opportunity cost of conflict for elites, making them materially better off under peace and therefore less willing to support renewed conflict initiation. Instead, the findings suggest that leaders retain sufficient control over resource distribution to capture incoming aid and redirect it toward loyalty maintenance.

To understand what type of aid drives this, I analyzed which aid sector drives the effect, although it should be interpreted as exploratory. In the LPM, economic and productive sector aid is associated with a decrease in relapse probability for non-personalist regimes ($\beta = -3.135$, $p = 0.014$). In contrast, for personalist regimes, the interaction term is positive and statistically significant ($\beta = 3.799$, $p = 0.039$).

Figure 3. Predicted Relapse Probability by Post-Conflict Aid⁴



The significance of economic and productive-sector aid is consistent with selectorate theory. In personalist regimes, infrastructure and industry can become patronage instruments, as the leader can redirect aid to reward loyal elites, reducing their pressure for peace. No other aid sector reaches conventional significance, but humanitarian aid shows a marginal interaction effect ($\beta = 13.394$, $p = 0.054$). The direction of coefficients is broadly consistent across five of six aid types – main effects are negative for non-personalist regimes, and interaction terms are mostly positive for personalist regimes. These results are presented in Appendix 1.

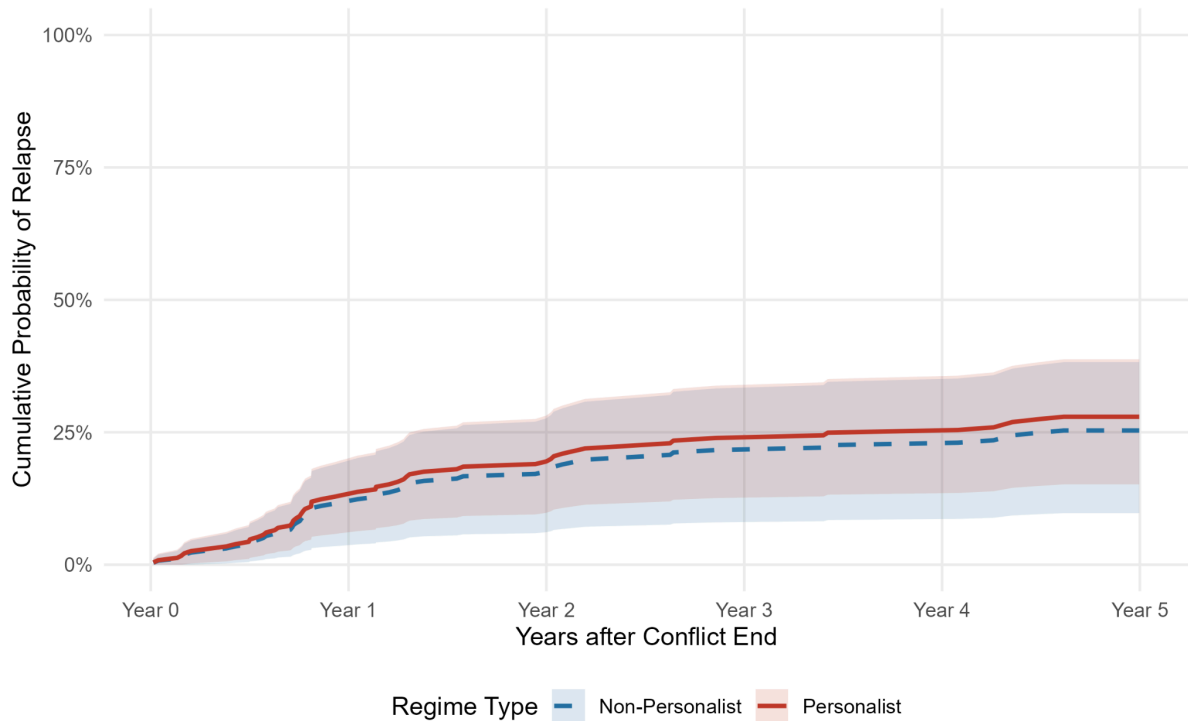
TE3. *Personalist authoritarian regimes whose relative military capabilities improve after the end of the conflict are less likely to comply with peace than other autocracies.*

TE3 is not supported. The interaction between personalism and trends in military asymmetry is not significant in any model. The main effect of the military asymmetry trend is also not significant. The military asymmetry ratio, a control variable capturing baseline capability differences at conflict onset, is likewise not significant. One possible explanation is that military capability trends measured through CINC scores may not capture perceptions of military advantage relevant to conflict recurrence.

⁴ *Source:* Author's own calculations. Predictions generated from LPM with controls, holding all continuous variables at their mean and binary variables at their modal value. Shading represents 95% confidence intervals.

Lastly, Figure 4 presents adjusted survival curves from the Cox model with all covariates held at sample means. Personalist regimes have a somewhat higher baseline probability of relapse over time, though this difference is not statistically significant.

Figure 4. Adjusted Survival Curves by Regime Type



Robustness checks results

Alternative personalism measure

Since some scholars criticize the GWF dataset for measurement error, I used alternative personalism measures as a robustness check. Inspired by Blattman, Gehlbach, and Yu (2025), I used GWF latent personalism measure (Geddes et al., 2018) and Gandhi and Sumner's (G&S) power consolidation measure (2020).

The **GWF latent personalism** sample has N=208 compared to N=214 in the main sample. The samples are comparable, with very similar means for sanctions and aid. The correlation between the GWF categorical measure and the latent binary measure is 0.405, which is among the highest, considering the concern about low correlation among different personalism measures.

The sanctions finding is robust. The interaction between sanctions intensity and the latent personalism measure is statistically significant under both binary ($\beta = -0.014$, $p = 0.005$) and continuous ($\beta = -0.025$, $p = 0.021$) operationalizations, with higher sanctions intensity

decreasing the relapse by personalist regimes compared to other autocracies. The aid interaction loses statistical significance in all GWF latent personalism models, though the direction remains consistent with the main results. This is likely due to differences in personalism measures and to low correlation between the datasets. The GWF autocratic measure classifies regime, but the latent measure is a time-varying, leader-level score that measures how unconstrained the leader is by the military and the party. See results in Appendix 2.

The **Gandhi and Sumner (G&S)** binary and continuous measures show no significant interactions for any of the variables of interest. This is expected given the very low correlation between GWF and G&S measures in the sample ($r=0.17$), the lowest pairwise correlation among personalism measures documented by Blattman et al. (2025). It suggests the two measures are classifying personalism very differently. The GWF captures observable regime structure while G&S captures internal power consolidation. For external actors who provide aid and decide on sanctioning states, it is harder to observe internal power consolidation, whereas the general regime structure is easier to classify.

Figure 5 shows that the majority of cases coded as personalists in the G&S dataset are just above the zero threshold, with 38.8% of personalist observations scoring below 0.3 out of 1. These near-threshold cases introduce measurement noise that likely attenuates the interaction effects. The results of the models are presented in Appendix 3.

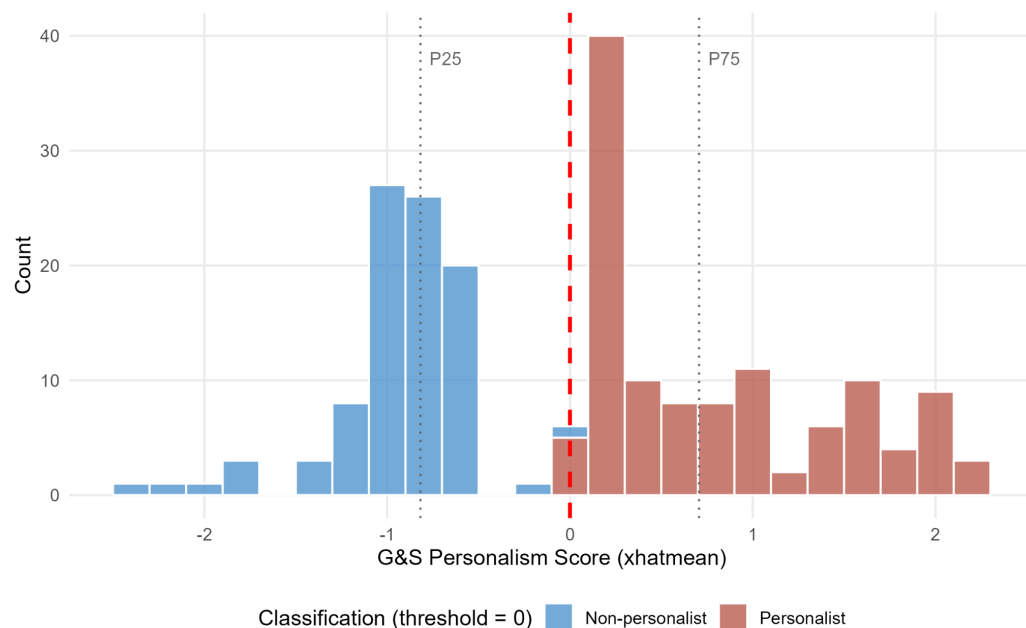


Figure 5. Distribution of Gandhi and Sumner (2020) Power Consolidation Score

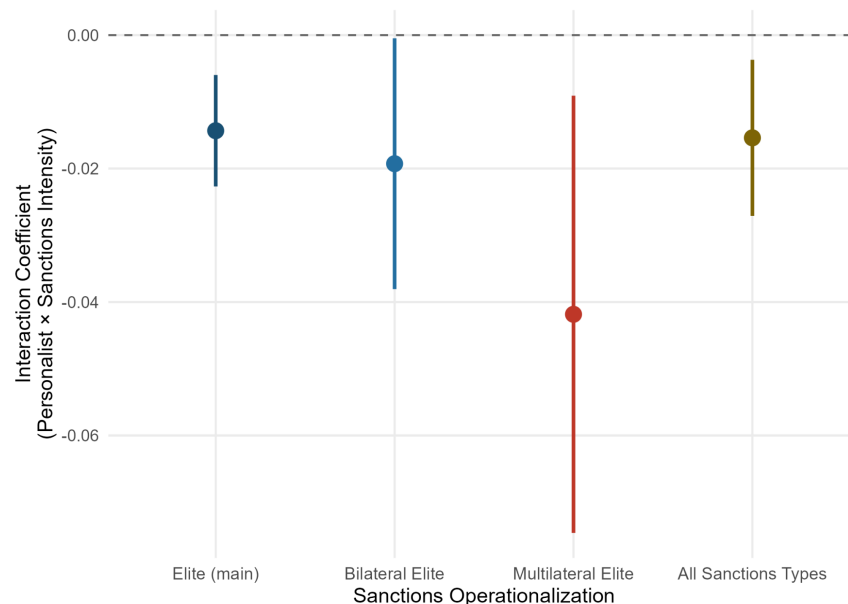
Conservative Specification

This produced a sample of 152 episodes with 52 relapse events. When collapsing multilateral episodes ($N = 188$), both the sanctions interaction ($\beta = -0.012$, $p = 0.007$) and aid interaction ($\beta = 1.340$, $p = 0.032$) are significant. After additionally excluding single-day episodes ($N = 152$), the aid interaction remains significant in the LPM ($\beta = 1.467$, $p = 0.032$), but marginal in Cox ($p = 0.094$). The interaction term for sanctions loses significance in both the LPM and Cox models. The analysis is therefore sensitive to excluding multi-actor conflicts and brief high-intensity conflicts. See results in Appendix 4.

Alternative Sanctions and Aid Operationalisation

The sanctions finding is robust across different operationalizations. The interaction remains significant under bilateral ($\beta = -0.019$, $p = 0.046$), multilateral ($\beta = -0.042$, $p = 0.013$), and all sanctions types ($\beta = -0.015$, $p = 0.011$). The effect is primarily driven by elite-targeted sanctions. Adding broader trade, military, and other types of sanctions barely changes the coefficient. Within elite sanctions, multilateral sanctions show the strongest effect ($\beta = -0.042$). This is consistent with my theoretical expectation that financial and travel sanctions on the elite matter most for personalist regime compliance. Figure 6 illustrates the interaction coefficient and shows that multilateral elite sanctions produce the largest effect. See results in Appendix 5.

Figure 6. *Personalist × Sanctions Interaction Coefficients Across Alternative Specifications*



The aid shows mixed results in alternative operationalizations. When using post-conflict aid per capita (logged) instead of aid as % of GDP, the main effect for non-personalist regimes is significant, but the interaction with personalism is not. When using the logged change

between pre- and post-conflict aid/GDP ratios, the interaction is marginally significant ($\beta = 1.718$, $p = 0.061$). The sanctions interaction remains significant in both alternative specifications ($p = 0.002$ and $p < 0.001$, respectively). See Appendix 6.

Aid Sectors and Conservative Specification

The main findings on economic aid are robust. The main effect is negative for non-personalist regimes ($\beta = -3.790$, $p = 0.020$), and the interaction for personalist regimes is positive and significant ($\beta = 4.285$, $p = 0.030$). The pattern of negative main effects and positive interactions is consistent across five of six sectors, suggesting that the destabilizing effect of aid on personalist regimes is not driven by a single sector. See results in Appendix 7.

Pre-conflict Aid and Conservative Specification

The interaction between pre-conflict aid levels and personalism is positive. Still, it does not reach statistical significance in either model, and the sample is small ($N = 82$; 18 relapses), limiting statistical power. This suggests that post-conflict, but not pre-conflict, interaction drives aid finding. Interestingly, the military asymmetry trend interaction is significant in this restricted sample, but with only 18 relapse events, this result is unreliable. See Appendix 8.

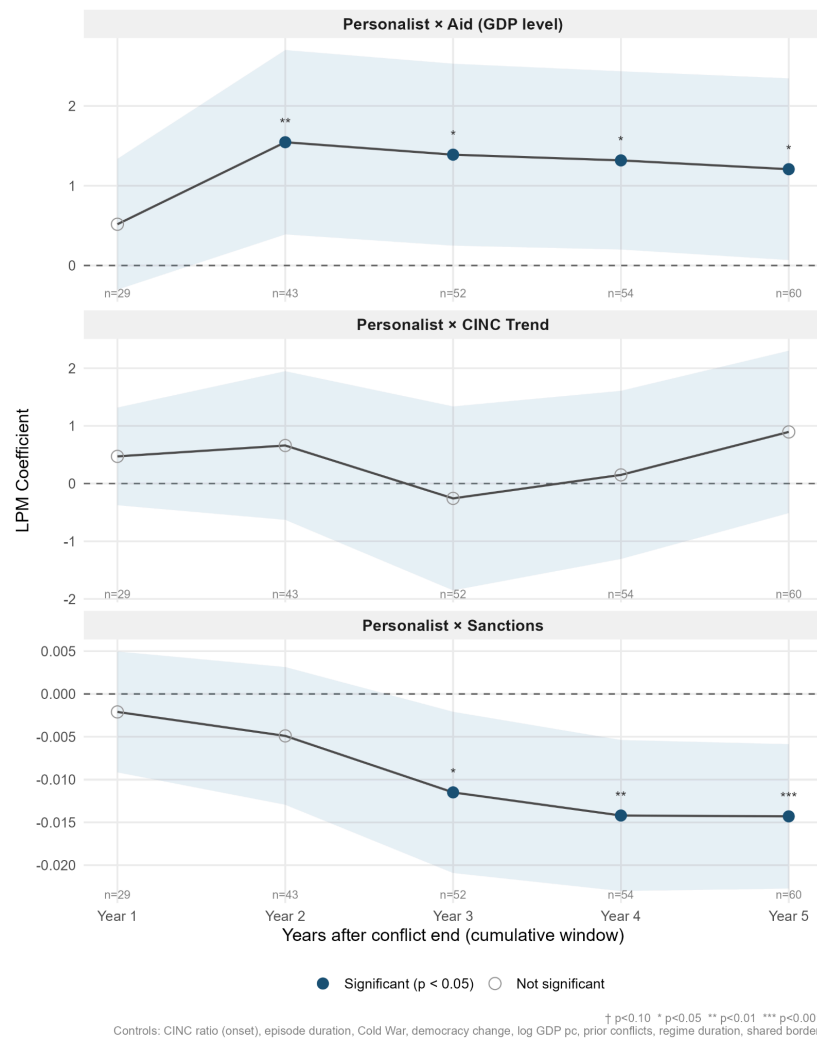
Alternative Time Horizons

I re-estimated the main model across post-conflict windows from year 1 to year 5 to see whether the findings are sensitive to the chosen timing. The sanctions interaction emerges only gradually: it is not significant in years 1 and 2 after conflict, then becomes significant at the 5% level in year 3 and at the 1% level in years 4 and 5. The aid interaction is strongest in year 2 ($\beta = 1.545$, $p = 0.006$). It remains significant through Year 5, although the coefficient slightly decreases. The military asymmetry trend is not significant in any year. See results in Appendix 9. See Figure 7 for results.

Uncensored Relapse

As the last robustness check, I re-estimated the primary LPM with an uncensored relapse, capturing any conflict initiation regardless of timing. The sanctions interaction remains significant at the 1% level. The sanction effect has strengthened over the years. The aid interaction is not significant, suggesting that the destabilizing aid effect is visible shortly after the conflict ended. The military asymmetry trend interaction shows marginal significance ($\beta = 1.620$, $p = 0.052$). Personalist regimes that improve their military position relative to the target may eventually re-initiate conflict, but not necessarily within five years. See Appendix 10 for results.

Figure 7. Interaction Effects Across Post-Conflict Years (Years 1-5)



Sensitivity Check on Conflict Outcome

As a final sensitivity check, I added conflict outcome as a control variable using all available categories from the MIDA 5.0 dataset, with stalemate as the reference category⁵. The sanctions interaction is unchanged ($\beta = -0.015$, $p < 0.001$) across all specifications. The aid interaction retains the same positive direction but loses conventional significance ($\beta = 0.984$, $p = 0.116$). Neither favorable nor unfavorable outcomes for the initiating state predict relapse relative to stalemate, suggesting that conflict outcomes do not drive compliance in this sample.

⁵ Conflict outcomes were coded from the initiator's (Side A) perspective using the MIDA 5.0 dataset. The following categories were available in the matched sample (N = 207): stalemate (N = 139, 67%), defeat (N = 25, 12%), compromise (N = 12, 6%), unclear (N = 11, 5%), victory (N = 10, 5%), yield loss (N = 4, 2%), released/transformed (N = 3, 1%), yield win (N = 1, <1%), and missing (N = 2, <1%). Due to small sample size for some, categories were collapsed into four groups for the regression: favorable outcomes (victory and yield win, N = 11), unfavorable outcomes (defeat and yield loss, N = 29), compromise (N = 12), and other (unclear, released, missing, N = 16). Seven episodes could not be matched to MIDA by date due to multi-dispute episode construction and are excluded from the sensitivity check. Stalemate is used as the reference category as it represents 67% of the cases.

DISCUSSION

Personalist regimes and conflict re-initiation

The idea for this paper was primarily driven by a research gap in understanding peace compliance among personalist states. There is evidence that military violence, such as riots or civil wars, is more likely to emerge under personal rule than under other autocracies (Blattman et al., 2025, p. 21). According to the literature, personalist regimes are more likely to renege on deals or international agreements (Geddes, 1999, p. 141; Chyzh, 2014). And since longer leader tenure is associated with a lower probability of peaceful resolution (Farber & Gowa, p. 126), given the long tenure of personalist leaders, the baseline assumption in my analysis was that personalist regimes are more likely than other autocracies to re-initiate military aggression.

My analysis shows this is not the case. Even though the probability coefficients for conflict relapse are higher for personalist regimes, there is no statistically significant difference in the likelihood of re-initiating the conflict within 5 years between any type of authoritarian state during 1960-2009. The null effect therefore suggests that conflict re-initiation cannot be attributed more to personalist regimes. This further suggests that simply replacing a personalist leader to produce durable peace will not work; other conditions must shape the broader selectorate's compliance logic as well. However, as Kendall-Taylor, Frantz, and Wright noted, "in 1980, personal regimes constituted 23% of all authoritarian states" but "that figure has risen to 40% of all authoritarian regimes" (Kendall-Taylor et al., 2017). It is possible that a research timeframe extended to 2020 — or even to the present — would show a different pattern, as personalist rule is becoming more common. Once the databases used in this research are updated, it is worth rechecking this baseline assumption.

Sanctions effect on the probability of conflict relapse

My results show that greater sanction intensity toward the elite is associated with a lower probability and hazard of conflict relapse in a personalist regime than in a non-personalist regime. Interestingly, in non-personalist authoritarian regimes, sanctions intensity is positively associated with relapse risk. These patterns, like foreign aid findings, suggest that sanctions do not affect all autocracies in the same way.

Sanctions matter not simply because they impose economic costs, but because they alter the political costs of defiance and compliance (Blanchard and Ripsman, 1999 in Hovi et al., 2005, p. 482). In personalist regimes, leaders depend heavily on a narrow elite coalition, and sanctions that restrict finance and travel may weaken the ruler's support base. The temporal dynamics analysis shows that this effect does not emerge immediately. The sanctions interaction is not significant in years 1 or 2 after conflict, but becomes significant from year 3 onward and strengthens through year 5 – it takes time for financial and travel restrictions to weaken the regime's ability to provide rents and patronage benefits.

These findings also explain variation in the literature on sanctions effectiveness in conflict prevention. In personalist regimes, all types of sanctions are associated with lower conflict relapse, with multilateral elite-targeted sanctions driving the strongest effect. In non-personalist regimes, sanctions are associated with higher relapse risk across most specifications, except multilateral sanctions, which show no significant association.

The sanctions analysis identifies associations rather than causal effects. Nevertheless, the consistent interaction pattern across models suggests that regime type conditions the relationship between sanctions and post-conflict peace. The key finding is that sanctions' impact depends on whether they can raise the political costs of noncompliance for actors whose support keeps the authoritarian regime in power. One limitation is that the sanctions interaction loses significance in the conservative specification that excludes single-day episodes, suggesting the effect may be relevant to deterring brief low-cost conflict rather than big wars.

The positive association between sanctions intensity and relapse risk in non-personalist autocracies is an unexpected finding. In a non-personalist regime, the winning coalition is broader, and the effect is more diffuse, so sanctions might not significantly disrupt the leader's network. It remains unclear why they increase the probability of relapse, and further study could explain this relationship.

This study is limited to interstate high-intensity conflict relapse among autocratic initiators between 1960 and 2009. Future research could examine more recent cases and lower-intensity conflicts. Finally, in this research, I measured the intensity of elite sanctions from the GSDB. This database does not capture whether the elites actually lost access to foreign assets, experienced financial disruptions, or were able to overcome travel restrictions. Future research could focus on individual-level sanctions within a winning coalition around a personalist leader and estimate whether more financial and travel sanctions against individuals in that coalition correlate with lower conflict reinitiation.

Foreign aid effects on the probability of conflict relapse

My findings suggest that in personalist regimes, more aid is associated with a higher probability of conflict relapse, though this association is sensitive to model specification and conflict outcome controls. The aid interaction is strongest at year 2 and slightly diminished through year 5. This suggests that the destabilizing effect of aid in personalist regimes happens quickly, which could be explained by the quick patronage capture of aid.

In contrast, in non-personalist authoritarian regimes, more aid is associated with a lower probability of relapse. A possible explanation is that in a personalist regime, public resources are distributed through patronage networks, and financial aid can be distributed among the elite to consolidate political control. It might serve as another means for the leader to "buy" the support of the selectorate. At the same time, non-personalist regimes may be less dependent on

personal networks and more capable of transforming aid into public goods. International actors might also have more opportunities to monitor how aid is distributed in non-personalist states.

Inspired by Crost et al. (2014) and Dreher et al. (2024), who suggested that different types of aid might drive distinct aid effects on conflict probability, I defined six aid sectors to examine how each is correlated with conflict relapse. The aid findings show that economic aid is likely to drive this result, and they hold in robustness checks. The destabilizing association of aid in personalist regimes is the strongest where aid is most politically usable. For example, economic and production aid might be redirected through patronage networks, and this aid might be used for rent-generating projects.

These findings align with the broader literature on ambiguous aid effects (Dreher et al., 2024). On the one hand, empirical evidence shows that aid might decrease insurgent violence (Beath et al., 2025; Crost et al., 2014). On the other hand, aid sometimes led to longer civil conflicts (Beath et al., 2025; Nunn & Qian, 2014). My results support these ambiguities and suggest that regime characteristics, time, and the aid sector may help explain when aid is associated with peace and when with renewed conflict. Some literature suggests that in all authoritarian settings, aid might fuel corruption and create incentives for prolonged conflict (Moyo, 2009, in Tierney et al., 2011). My results show that aid might be effective for peace-building in non-personalist states.

At the same time, these findings should be interpreted cautiously because of endogeneity. Like Nunn and Qian, I acknowledge the bias that comes from estimating effects of aid on conflict: the effect of aid might be biased upward as conflict increases demand for aid, and downward bias might occur since countries might selectively reduce aid to those states where conflict is more likely (Nunn & Qian, 2014, p. 1631). The pre-conflict aid robustness check produces a positive but insignificant interaction, which is expected given the reduced sample size. When aid is measured per capita rather than relative to GDP, the interaction also loses significance. It suggests that in personalist regimes, aid relative to the economy, rather than the amount each citizen would receive, matters more.

The aid finding, however, is sensitive to conflict outcome controls, as it loses significance once outcomes in conflict are accounted for. This is likely because aid and conflict outcome are correlated. Since unresolved conflicts are generally in more fragile countries, one explanation is that donors give more aid to those fragile unresolved conflicts. In other words, aid may not be causing relapse, but it may be flowing to places already at high risk of relapse.

To eliminate this bias and create causal mechanisms, an instrumental variable that affects aid but not conflict would need to be created. Following the design of Nunn and Qian's (2014), a possible solution is constructing the interaction term of how much each donor country had available aid for distribution in a given year (e.g., budget article for foreign aid for a given

country), and how much a recipient country has historically received from that donor. The analysis here could be limited to a single donor rather than multiple countries or agencies, as aid effects often depend on individual donors (Dreher et al., 2014). However, constructing an instrumental variable of this kind is beyond the scope of this study.

Military asymmetry effects on the probability of conflict relapse

The last theoretical expectation in my research was that personalist authoritarian regimes facing a favorable military asymmetry would be more likely to relapse into conflict, and those that are militarily inferior would be more likely to comply with peace. Military asymmetry was measured as a post-conflict trend, the slope of the logged CINC ratio over five years following conflict termination.

The expectation was based on rational-choice logic: a stronger initiator faces lower costs of renewed aggression. Personalist leaders, unconstrained by institutions, can renew high-intensity violence because institutional constraints are weaker. However, none of the main models support this expectation.

There are several possible explanations. First, I measured whether military asymmetry matters for conflict relapse, not conflict onset. It is likely that leaders who initiated a conflict for the first time accounted for the military asymmetry in their initial decision, and a study focusing on conflict onset might find statistically significant results. Second, the uncensored relapse model shows marginal significance, suggesting that military asymmetry may influence compliance over a longer horizon than 5 years. It is consistent with power-transition theory, which holds that power shifts occur slowly. Third, the operationalization of military capability might be too broad, as the CINC index aggregates information about “total population, urban population, iron and steel production, energy consumption, military personnel, and military expenditure of all state members” (The Correlates of War Project, n.d.).

The absence of a statistically significant interaction suggests that military asymmetry affects personalist and non-personalist autocracies similarly in their decisions about conflict relapse. This suggests that the aggregated military asymmetry is insufficient to explain post-conflict relapse by personalist regimes. For post-conflict relapse, other factors, such as ideology, might matter more.

Future research could test the correlation between military asymmetry and conflict onset rather than relapse. Additionally, measuring asymmetry at different times (before conflict, at the moment of conflict termination, and during the post-conflict period) might capture shifts in power and show different associations with conflict relapse. Military asymmetry could also be disaggregated beyond CINC index, such as nuclear capability, air and naval power, alliance membership, and external military support.

POLICY RECOMMENDATIONS

As mentioned at the beginning of this research, we are now in an era where global conflicts are on the rise and global freedom has been in decline for almost two decades (Obermeier & 2024; Freedom House, 2024). Most conflicts are driven by authoritarian aggression (Freedom House, 2024), among which the share of personalist regimes has been growing over the last few decades, now accounting for almost half of all authoritarian regimes (Geddes et al., 2014; Kendall-Taylor et al., 2017). What conditions might ensure stability and prevent authoritarian initiators from launching another high-intensity military aggression against the same state? The findings suggest that post-conflict peacebuilding efforts should not treat authoritarian regimes as a single category. However, these recommendations should be interpreted cautiously. The analysis identifies conditional correlations rather than causal effects, and contemporary cases such as Russia may differ from historical cases included in the dataset. Nevertheless, the findings identify recurring patterns relevant for post-conflict policy design.

Use of sanctions

These recommendations are relevant to the following **actors**: the European Union, the Office of Foreign Assets Control (OFAC) of the US Department of the Treasury, coalition governments, the UN Security Council, and individual governments.

First, after the end of the conflict initiated by a personalist regime, the elite-targeted sanctions should serve as an enforcement mechanism of long-term peace. The findings on sanctions suggest that long-lasting, elite-targeted measures are more effective at promoting peace compliance in personalist regimes than in non-personalist ones.

Second, sanctions should be maintained after the formal end of the conflict as part of a compliance mechanism. The duration of sanctions imposed on the personalist initiator matters: the longer elite-targeted measures remain in place, the higher the probability that the conflict won't relapse. That is why actors who implement sanctions should resist pressure from sanctioning states to lift measures at the early stages of the conflict's end. This research does not directly study how the presence of specific conditions under which sanctions will be lifted affects sanctions effectiveness. At the same time, for the elite, it is important to understand when and under which conditions sanctions will end. If the condition is the absence of renewed military aggression, the elite gains an incentive to pressure the leader toward compliance.

Third, sanctions coalitions should be broad and intense from the conflict onset. The application of financial and travel restrictions by a wide multilateral coalition of states produces stronger correlations with peace compliance. The UN Security Council, EU Council, OFAC, and other coalition governments should therefore coordinate such efforts and minimize "sanctions exclusions" allowing targeted elites to avoid pressure.

Post-conflict aid delivery

These recommendations are relevant to the following **actors** which deliver **non-humanitarian aid**: World Bank, International Monetary Fund (IMF), the Organisation for Economic Co-operation and Development's Development Assistance Committee (OECD-DAC), US Department of State, European Commission; and for actors that deliver **humanitarian aid**: United Nations agencies (OCHA, WFP, UNHCR, UNICEF), the International Red Cross and Red Crescent Movement, and international NGOs.

First, post-conflict aid delivery to personalist regimes should be approached with caution. Contrary to the original theoretical expectation, the results provide tentative evidence that higher levels of post-conflict foreign aid are associated with a higher probability of relapse in personalist countries. Therefore, external actors should avoid assuming that foreign aid assistance will automatically have pacifying effects in personalist settings.

Second, when aid is provided to personalist regimes post-conflict, its delivery should bypass patronage structures as much as possible. The findings do not imply that foreign assistance to personalist states should be abandoned, but they suggest that the way and sector of delivery matter. For example, aid to the economic and production sectors is vulnerable to political capture. Donors should therefore closely monitor aid distribution.

Third, aid assistance to authoritarian states in post-conflict settings should be differentiated by regime type. In non-personalist authoritarian regimes, aid is associated with a lower risk of conflict relapse. For personalist states, the effect is the opposite. When delivering aid in personalist regimes, a higher share of aid funding should be directed toward monitoring and developing stricter procedures for recipient selection than in non-personalist regimes.

Military capability

These recommendations are relevant to individual governments forming coalitions with states targeted in conflict, target countries, NATO and its allies, and peace mediators.

First, building up the military capacity of the attacked state may be valuable, but it should not substitute other forms of political and economic pressure that might threaten the personalist regime's survival. The null result suggests that military asymmetry, even in favor of the target state, is insufficient to explain conflict relapse by personalist states in the short term.

Second, international actors should closely monitor the military capabilities growth of former personalist initiators and maintain arms embargo enforcement beyond the immediate post-conflict period. The uncensored model shows that under the observation window beyond 5 years, improving military capabilities become marginally associated with higher relapse probability in personalist regimes.

Application to Ukraine-Russia peace negotiations

This research was guided by the ongoing peace negotiations between Ukraine and Russia. Under the personalist rule of Vladimir Putin, Russia initiated military aggression against Ukraine in 2014 and a full-scale invasion of Ukraine in 2022. During this period, multiple peace attempts failed as Russia repeatedly violated them. While the findings of this research are bound by a timeframe ending in 2009, the theoretical mechanisms identified offer insights for designing future peace.

First, sanctions imposed on Russia following its military aggression should be maintained after any formal ceasefire or peace agreement. Ukraine's partners and Ukraine itself should resist pressure to lift sanctions at the early stages of peace or immediately after the formal conflict ends. Instead, financial, travel, and trade sanctions should be maintained as an enforcement mechanism for compliance, as cumulative elite-targeted sanctions intensity over the post-conflict period shows a stronger correlation with personalist regime compliance with peace, with effects emerging from year 3 onward.

Second, individual governments and organizations should be careful with aid delivery to Russia following any peace agreement, as foreign aid may undermine rather than prolong peace. The World Bank, the IMF, OECD-DAC donors, and individual governments should not assume that post-conflict economic assistance will have a pacifying effect on Russia. They should avoid assuming that more aid, especially economic aid, will produce stable peace. During aid delivery, there should be heightened monitoring of how funds are distributed and whether they bypass the patronage networks of the relevant regime.

Finally, the null result on military asymmetry shows that, for personalist regimes, the military asymmetry in the target's favor does not matter when making decisions about conflict relapse. This suggests that increasing Ukrainian military capacity, while strategically important for deterrence, should not be treated as a substitute for political and economic pressure on the Russian elite. Putin, facing battlefield disadvantage, may still calculate that renewed aggression serves his domestic survival needs.

I acknowledge that Russia is an extreme case of a personalist regime: it has nuclear weapons, a large territory with natural resources, it is a permanent UN Security Council member, and at the moment of writing this chapter, it is the most sanctioned country in the world. This limits the generalization of the findings. Still, the correlational mechanisms identified here – vulnerability to intense, elite-targeted sanctions, the destabilizing potential of aid to peace and stability, and the limited deterrent effect of military asymmetry – are relevant to the logic of personalist survival that Russia shares with personalist regimes.

CONCLUSIONS

This research analyzed the conditions under which personalist authoritarian regimes comply with peace after interstate conflict. I argued that peace compliance in personalist regimes depends on whether it maximizes the leader's political survival, which in turn depends on winning coalition support. The study tested whether elite-targeted sanctions, post-conflict aid, and military asymmetry affect the likelihood that personalist regimes re-initiate conflict.

To test these assumptions, I employed a quantitative analysis. The analysis drew on a dataset of 214 dyad-level conflict episodes involving autocratic initiators between 1960 and 2009, combining data from the GWF Autocratic Regimes dataset, the Correlates of War MID project, the Global Sanctions Database, AidData, and the National Material Capabilities dataset. I used linear probability and Cox proportional hazards models to compare how each condition relates to conflict relapse in personalist versus non-personalist authoritarian regimes.

The results show that authoritarian regimes should not be treated as a single category in post-conflict analysis, because regime type conditions how sanctions and aid relate to peace and stability. First, the sanctions expectation is supported: higher elite-targeted sanctions intensity is associated with a lower risk of conflict relapse in personalist regimes relative to other autocracies. These findings indicate that sanctions matter when they raise the political costs of renewed aggression for the narrow elite coalition sustaining personalist rulers, which supports the argument that personalist leaders are more likely to comply with peace when the benefits sustaining elite loyalty are constrained. Interestingly, the findings suggest that in non-personalist regimes, elite-targeted sanctions increase the probability of conflict relapse. However, the reasons for this relationship remain unclear.

Second, the aid expectation is reversed. The findings provide suggestive evidence that higher levels of post-conflict aid in personalist regimes are associated with a greater probability of conflict relapse, though this relationship is sensitive to conflict outcome controls and alternative operationalizations. In contrast, in non-personalist autocracies, aid is associated with a lower risk of relapse. The sectoral analysis further suggests that this pattern is driven by economic and productive-sector aid. These results imply that, in personalist regimes, foreign assistance may be absorbed into patronage networks and used to reinforce elite loyalty rather than to stabilize peace. Moreover, aid may unintentionally reinforce the same patronage structures that make renewed conflict politically sustainable for the personalist regime.

Third, the theoretical expectation on military asymmetry is not supported. Although stronger military capabilities may matter more broadly for conflict initiation, the analysis does not show that military asymmetry explains post-conflict relapse differently in personalist regimes than in other autocracies. This implies that aggregate military capability alone is insufficient to explain post-conflict peace compliance by personalist regimes.

These findings make several important contributions. Empirically, the analysis adds to the limited literature on peace compliance in interstate conflicts involving personalist authoritarian regimes. Theoretically, the study supports the claim that personalist leaders respond to pressures affecting their selectorate and political survival. However, it also shows that these pressures do not always operate as expected. While elite-targeted sanctions appear to be associated with lower conflict relapse, aid may instead reinforce patronage structures that make renewed conflict more politically sustainable for ruling elites.

These findings point to several implications for international actors in post-conflict settings and countries targeted by personalist aggression. First, higher-intensity elite-targeted sanctions are associated with lower conflict relapse in personalist regimes, so international actors should maintain them and resist lifting them early. Since effects emerge from year 3 onward, early sanctions removal is risky. Second, higher aid to personalist autocracies is associated with increased risk of conflict relapse, though this finding is sensitive to specifications. This nevertheless indicates that donors should strengthen monitoring in personalist contexts. Third, investing in the armed forces of the attacked country alone is insufficient to deter a personalist initiator. It should be complemented by political and economic pressure against the winning coalition that threatens the regime's survival. Additionally, marginal evidence signals that military advantage may drive relapse in the long term, so arms embargo enforcement should extend beyond the immediate post-conflict period.

These findings should be interpreted cautiously: this exploratory study is limited to a small number of high-intensity interstate conflict relapses among autocratic initiators between 1960 and 2009. The measurement of personalism remains contested across datasets, and some robustness checks show sensitivity to alternative operationalizations, especially for aid. In addition, the sanctions measure captures the intensity of elite-targeted sanctions but cannot directly observe whether elites actually lost access to assets, mobility, or other benefits. Likewise, the military asymmetry variable is based on a broad aggregate index that may not capture the forms of power most relevant for post-conflict bargaining and coercion.

Future research could extend the analysis in several directions. First, it should examine more recent cases, given the growing prevalence of personalist rule. Second, it could focus on creating aid-related and donor-specific instrumental variables to identify causal mechanisms of aid effects on peace compliance. Third, it could examine whether elite-targeted sanctions actually targeted the winning coalition in personalist regimes, and how conditions for lifting sanctions affect their effectiveness in promoting peace compliance. Fourth, it should complement the quantitative findings with qualitative case studies to better trace how leaders, elites, and external actors interact in post-conflict periods. Overall, this research suggests that durable peace with personalist regimes depends on targeting the narrow elite networks sustaining personalist rule.

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APPENDICES

Appendix 1: Aid Sector Models (N = 214, Main Sample)

Aid Sector	N	Main Effect (β)	Main Effect (p)	Interaction (β)	Interaction (p)
Social Services	214	4.246	0.272	0.235	0.968
Governance	214	4.870	0.498	10.325	0.546
Economic	214	-3.135	0.014	3.799	0.039
Humanitarian	214	-5.965	0.128	13.394	0.054
Budget/Debt Relief	214	-1.320	0.459	1.562	0.453
Other Multisector	214	13.494	0.140	-7.086	0.450

Main Effect: association between the aid sector and relapse in non-personalist regimes.

Interaction = differential effect for personalist regimes relative to non-personalist regimes.

DV: conflict relapse within a 5-year window. LPM with dyad-clustered standard errors.

Sample: main analytical sample, dyad-level conflict episodes with autocratic initiators, 1960-2009.

Data: AidData (Tierney et al., 2011). * $p < 0.05$, † $p < 0.10$.

Appendix 2: GWF Latent Personalism Models

	LPM — Latent Binary	LPM — Latent Continuous
Intercept	0.182 (0.299)	0.155 (0.308)
Latent Personalism (binary)	0.006 (0.116)	
Latent Personalism (continuous)		0.041 (0.206)
Elite Sanctions Intensity	0.007* (0.003)	0.011* (0.004)
Post-conflict Aid (log, % GDP)	-0.263 (0.474)	-0.598 (0.524)
Military Asymmetry Trend	-0.223 (0.637)	-0.236 (0.820)
Latent (binary) $\times$ Sanctions	-0.014** (0.005)	
Latent (binary) $\times$ Aid	0.326 (0.740)	
Latent (binary) $\times$ Mil. Trend	0.459 (0.770)	
Latent (cont.) $\times$ Sanctions		-0.025*

		(0.011)
Latent (cont.) × Aid		1.388
		(1.701)
Latent (cont.) × Mil. Trend		0.533
		(1.287)
Military Asymmetry Ratio	0.013	0.013
	(0.018)	(0.017)
Episode Duration (days)	0.000	0.000
	(0.000)	(0.000)
Cold War	-0.013	-0.034
	(0.073)	(0.074)
Democracy Change	-0.372	-0.433
	(0.342)	(0.370)
Log GDP per Capita	0.000	0.006
	(0.042)	(0.041)
Prior Conflicts	0.018†	0.020†
	(0.010)	(0.010)
Regime Duration	-0.003	-0.004
	(0.003)	(0.003)
Shared Border	0.158*	0.145*
	(0.069)	(0.067)
N	208	208
R ²	0.127	0.133
Adj. R ²	0.059	0.066
† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001		

Dyad-clustered standard errors in parentheses.

DV: conflict relapse within 5-year window.

Binary measure dichotomized at sample median (0.427).

Data: Geddes, Wright & Frantz (2018).

† p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 3: Gandhi & Sumner Personalism Models

	LPM — G&S Binary	LPM — G&S Continuous
Intercept	0.338	0.488
	(0.332)	(0.382)
G&S Personalism (binary)	0.125	
	(0.092)	
G&S Personalism (continuous)		0.055
		(0.046)
Elite Sanctions Intensity	0.003	-0.001
	(0.004)	(0.003)
Post-conflict Aid (log, % GDP)	-0.255	-0.284
	(0.457)	(0.408)
Military Asymmetry Trend	0.058	0.230

	(0.470)	(0.419)
G&S (binary) × Sanctions	-0.006	
	(0.004)	
G&S (binary) × Aid	0.009	
	(0.685)	
G&S (binary) × Mil. Trend	0.153	
	(0.764)	
G&S (cont.) × Sanctions		-0.001
		(0.002)
G&S (cont.) × Aid		0.305
		(0.544)
G&S (cont.) × Mil. Trend		0.472
		(0.462)
Military Asymmetry Ratio	0.014	0.019
	(0.017)	(0.017)
Episode Duration (days)	0.000	0.000
	(0.000)	(0.000)
Cold War	0.038	0.024
	(0.070)	(0.071)
Democracy Change	-0.075	-0.133
	(0.327)	(0.315)
Log GDP per Capita	-0.045	-0.053
	(0.047)	(0.050)
Prior Conflicts	0.016†	0.014
	(0.009)	(0.009)
Regime Duration	-0.001	-0.001
	(0.003)	(0.003)
Shared Border	0.179*	0.179*
	(0.072)	(0.072)
N	208	208
R ²	0.108	0.118
Adj. R ²	0.039	0.049

† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Dyad-clustered standard errors in parentheses.

DV: conflict relapse within a 5-year window.

Binary measure dichotomized at zero (xhatmean > 0 = personalist).

Data: Gandhi & Sumner (2020).

† p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 4: Conservative Specification Models

	LPM — Collapsed (N=188)	LPM — Conservative (N=152)	Cox PH — Conservative (N=152)
Intercept	0.206 (0.333)	0.402 (0.415)	

Personalist Regime	0.015 (0.093)	0.045 (0.111)	-0.023 (0.504)
Elite Sanctions Intensity	0.007* (0.003)	0.005 (0.004)	-0.000 (0.021)
Post-conflict Aid (log, % GDP)	-0.781† (0.397)	-0.919* (0.404)	-13.465 (10.281)
Military Asymmetry Trend	-0.686 (0.685)	-1.064 (0.889)	-7.983 (6.921)
Personalist × Sanctions Intensity	-0.012** (0.004)	-0.009 (0.006)	-0.015 (0.031)
Personalist × Aid (log, % GDP)	1.340* (0.621)	1.467* (0.677)	14.498† (10.342)
Personalist × Military Asym. Trend	0.776 (0.725)	1.296 (0.923)	9.269 (7.270)
Military Asymmetry Ratio	0.013 (0.022)	0.010 (0.029)	0.062 (0.113)
Episode Duration (days)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Cold War	-0.022 (0.075)	0.006 (0.088)	-0.083 (0.341)
Democracy Change	-0.440 (0.395)	-0.713 (0.478)	-3.316 (2.518)
Log GDP per Capita	-0.005 (0.043)	-0.027 (0.051)	-0.223 (0.178)
Prior Conflicts	0.016 (0.010)	0.019 (0.013)	0.067† (0.036)
Regime Duration	-0.003 (0.003)	-0.001 (0.004)	-0.007 (0.014)
Shared Border	0.163† (0.088)	0.077 (0.132)	0.401 (0.579)
N	188	152	152
R ²	0.107	0.122	

Adj. R² 0.030 0.025

† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Dyad-clustered standard errors in parentheses.

DV: conflict relapse within 5-year window.

Collapsed: one observation per initiating state per episode.

Conservative: collapsed + excluding single-day episodes.

† p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 5: Alternative Sanctions Operationalization

	Bilateral Elite	Multilateral Elite	All Types
Intercept	0.053 (0.316)	0.269 (0.292)	0.097 (0.299)
Personalist Regime	0.013 (0.088)	-0.009 (0.086)	0.034 (0.094)
Sanctions (Bilateral Elite)	0.015* (0.006)		
Sanctions (Multilateral Elite)		0.019 (0.015)	
Sanctions (All Types)			0.010* (0.005)
Post-conflict Aid (log, % GDP)	-0.690† (0.408)	-0.919* (0.396)	-0.670† (0.402)
Military Asymmetry Trend	-0.636 (0.662)	-0.746 (0.685)	-0.523 (0.678)
Personalist × Sanctions (Bilateral)	-0.019* (0.010)		
Personalist × Sanctions (Multilateral)		-0.042* (0.017)	
Personalist × Sanctions (All Types)			-0.015* (0.006)
Personalist × Aid (log, % GDP)	1.289* (0.582)	1.307* (0.589)	1.147† (0.595)
Personalist × Military Asym. Trend	0.817 (0.703)	1.001 (0.746)	0.708 (0.742)
Military Asymmetry Ratio	0.005 (0.017)	0.013 (0.018)	0.008 (0.017)
Episode Duration (days)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Cold War	0.017 (0.065)	-0.039 (0.074)	-0.016 (0.069)
Democracy Change	-0.416	-0.396	-0.413

	(0.383)	(0.365)	(0.384)
Log GDP per Capita	0.005	-0.008	0.007
	(0.041)	(0.039)	(0.040)
Prior Conflicts	0.016†	0.016†	0.017†
	(0.010)	(0.010)	(0.010)
Regime Duration	-0.002	-0.003	-0.003
	(0.003)	(0.003)	(0.003)
Shared Border	0.199**	0.157*	0.169*
	(0.068)	(0.071)	(0.069)
N	214	214	214
R ²	0.133	0.138	0.139
Adj. R ²	0.067	0.073	0.073

† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Dyad-clustered standard errors in parentheses.

DV: conflict relapse within 5-year window. N = 214.

Main model uses elite sanctions intensity (financial + travel, sender-weighted).

† p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 6: Alternative Aid Operationalization

	Aid per Capita (log)	Aid/GDP Change (log)
Intercept	0.246	0.109
	(0.280)	(0.282)
Personalist Regime	-0.141	0.078
	(0.168)	(0.083)
Elite Sanctions Intensity	0.005†	0.008*
	(0.003)	(0.003)
Aid per Capita (log)	-0.070*	
	(0.032)	
Aid/GDP Change (log)		-1.143†
		(0.624)
Military Asymmetry Trend	-0.846	-0.591
	(0.759)	(0.702)
Personalist × Sanctions Intensity	-0.013**	-0.015***
	(0.004)	(0.004)
Personalist × Aid per Capita (log)	0.077	
	(0.048)	
Personalist × Aid/GDP Change (log)		1.718†
		(0.913)
Personalist × Military Asym. Trend	1.085	0.844
	(0.792)	(0.744)
Military Asymmetry Ratio	0.007	0.011
	(0.016)	(0.018)
Episode Duration (days)	0.000	0.000
	(0.000)	(0.000)

Cold War	-0.022 (0.067)	-0.011 (0.072)
Democracy Change	-0.284 (0.404)	-0.397 (0.376)
Log GDP per Capita	0.011 (0.038)	-0.000 (0.038)
Prior Conflicts	0.015 (0.009)	0.015 (0.010)
Regime Duration	-0.004 (0.003)	-0.002 (0.003)
Shared Border	0.205** (0.065)	0.182* (0.070)
N	214	214
R ²	0.149	0.137
Adj. R ²	0.085	0.072
† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001		

Dyad-clustered standard errors in parentheses.

DV: conflict relapse within 5-year window. N = 214.

Main model uses post-conflict aid as % of GDP (logged).

Aid per capita: logged mean post-conflict aid per capita over 5 years.

Aid/GDP change: logged difference between post- and pre-conflict aid/GDP ratios.

† p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 7: Aid Sector Models (N = 152, Conservative Sample)

Aid Sector	N	Main Effect (β)	Main Effect (p)	Interaction (β)	Interaction (p)
Social Services	152	4.513	0.261	0.827	0.893
Governance	152	2.522	0.906	11.623	0.681
Economic	152	-3.790	0.020	4.285	0.030
Humanitarian	152	-1.956	0.784	9.583	0.287
Budget/Debt Relief	152	-0.816	0.648	1.836	0.472
Other Multisector	152	9.202	0.403	-0.608	0.956

Main Effect = association between aid sector and relapse for non-personalist regimes.

Interaction = differential effect for personalist regimes relative to non-personalist.

DV: conflict relapse within 5-year window. LPM with dyad-clustered standard errors.

Sample: conservative specification (collapsed multilateral, excluding single-day episodes).

Data: AidData (Tierney et al., 2011). † p < 0.10, * p < 0.05.

Appendix 8: Pre-conflict Aid Endogeneity Check

	LPM — Pre-conflict Aid	Cox PH — Pre-conflict Aid
Intercept	0.054 (0.446)	
Personalist Regime	-0.029 (0.116)	-0.221 (0.794)
Pre-conflict Aid (log, % GDP)	-1.019 (0.699)	-13.668† (10.602)
Military Asymmetry Ratio	0.006 (0.025)	0.031 (0.170)
Military Asymmetry Trend	-2.932* (1.274)	-20.778† (9.210)
Personalist × Pre-conflict Aid	1.591 (1.402)	15.691† (11.917)
Personalist × Military Asym. Trend	3.723** (1.337)	27.970* (10.143)
Episode Duration (days)	-0.000 (0.000)	-0.000 (0.001)
Cold War	0.018 (0.111)	-0.441 (0.600)
Democracy Change	-0.417 (0.423)	-2.693 (3.309)
Log GDP per Capita	0.011 (0.052)	0.152 (0.290)
Prior Conflicts	0.072*** (0.021)	0.196** (0.084)
Regime Duration	-0.001 (0.004)	-0.024 (0.021)
Shared Border	0.070 (0.121)	1.198 (0.921)
N	82	82
R ²	0.187	
Adj. R ²	0.032	
† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001		

Dyad-clustered standard errors in parentheses.

DV: conflict relapse within 5-year window.

Sample restricted to episodes with no prior high-intensity conflict in the 5 years before onset (N = 82, 18 relapse events).

Sanctions variable excluded from this specification.

† p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 9: Temporal Dynamics (Years 1-5, N = 214)

Year 1	Year 2	Year 3	Year 4	Year 5
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Personalist Regime	0.030 (0.071)	-0.008 (0.087)	-0.007 (0.095)	0.020 (0.091)	0.029 (0.089)
Sanctions Intensity	-0.000 (0.002)	0.003 (0.003)	0.007* (0.003)	0.010** (0.003)	0.007* (0.003)
Aid/GDP (log level)	-0.705* (0.326)	-0.880* (0.400)	-0.750* (0.356)	-0.698* (0.352)	-0.755† (0.389)
CINC Ratio (log, control)	-0.003 (0.015)	0.004 (0.017)	0.007 (0.017)	0.011 (0.017)	0.010 (0.017)
CINC Trend (5yr)	-0.243 (0.315)	-0.680 (0.592)	0.363 (0.735)	-0.103 (0.669)	-0.668 (0.671)
Personalist × Sanctions	-0.002 (0.004)	-0.005 (0.004)	-0.011* (0.005)	-0.014** (0.004)	-0.014*** (0.004)
Personalist × Aid	0.517 (0.418)	1.545** (0.589)	1.389* (0.582)	1.318* (0.570)	1.207* (0.581)
Personalist × CINC Trend	0.471 (0.431)	0.660 (0.657)	-0.257 (0.813)	0.150 (0.743)	0.895 (0.718)
Episode Duration	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Cold War	-0.049 (0.060)	-0.004 (0.060)	-0.034 (0.065)	-0.009 (0.065)	-0.014 (0.069)
Democracy Change	-0.138 (0.306)	-0.131 (0.320)	-0.208 (0.392)	-0.256 (0.383)	-0.418 (0.379)
Log GDP pc (pre)	-0.024 (0.024)	-0.015 (0.027)	0.011 (0.034)	0.015 (0.033)	0.003 (0.039)
Prior Conflicts	0.013* (0.006)	0.017* (0.009)	0.017† (0.009)	0.017† (0.009)	0.015 (0.010)
Regime Duration	-0.001 (0.002)	-0.003 (0.002)	-0.004 (0.003)	-0.003 (0.003)	-0.003 (0.003)
Shared Border	0.090 (0.066)	0.148* (0.062)	0.197** (0.067)	0.205** (0.065)	0.178* (0.069)
Intercept	0.255 (0.211)	0.195 (0.223)	0.045 (0.267)	-0.026 (0.254)	0.129 (0.297)
N	214	214	214	214	214
R ²	0.095	0.140	0.146	0.153	0.140
Adj. R ²	0.027	0.075	0.082	0.089	0.074

† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Dyad-clustered standard errors in parentheses.

DV = 1 if same initiator relapses within the indicated cumulative window.

milasym_ratio included as control only (no interaction).

Focal episodes end ≤ 2009, data through 2014.

† p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 10: Uncensored Relapse Model

	LPM — Uncensored Relapse
Intercept	0.444 (0.283)
Personalist Regime	0.078 (0.084)
Elite Sanctions Intensity	0.006* (0.003)
Post-conflict Aid (log, % GDP)	-1.013* (0.422)
Military Asymmetry Trend	-1.012 (0.705)
Personalist × Sanctions Intensity	-0.017*** (0.004)
Personalist × Aid (log, % GDP)	0.790 (0.604)
Personalist × Military Asym. Trend	1.620† (0.830)
Military Asymmetry Ratio	0.024 (0.017)
Episode Duration (days)	0.000 (0.000)
Cold War	0.030 (0.065)
Democracy Change	-0.465 (0.358)
Log GDP per Capita	-0.035 (0.036)
Prior Conflicts	0.001 (0.010)
Regime Duration	-0.003 (0.002)
Shared Border	0.253*** (0.068)
N	214
R ²	0.186
Adj. R ²	0.124
† p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001	

Dyad-clustered standard errors in parentheses.

DV: any subsequent conflict initiation by same initiator against same target, regardless of timing (uncensored). N = 214.

† p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix 11. Full Replication Code in R

Replication code is available at:

<https://github.com/KhrystynaDmytryshyn/dmytryshyn-thesis-replication>

Appendix 12. Disclosure of AI assistance

In accordance with academic integrity standards, I disclose my use of Artificial Intelligence (AI) tools. In this research, I used AI tools as a supportive research, formatting, and coding assistant. The tools I used include [Grammarly](#), [Citation Machine](#), [ChatGPT](#), [Claude](#), and [Gemini](#). All main parts of the research, including research design, theoretical framework, data operationalization, statistical analysis, interpretation of findings, and policy recommendations, are based on my decisions and interpretation.

I used Grammarly, a writing assistant software tool with AI functions, for proofreading. It helped identify grammar, spelling, and punctuation errors, and occasionally suggested clearer sentence constructions. I reviewed all suggested edits manually before incorporating them into the thesis. For citation formatting, I used the Citation Machine, which automatically extracts bibliographic information from a provided URL and generates APA-style citations. I manually reviewed each citation for accuracy. I also used ChatGPT for citation formatting, and asked it to review my final reference list for inconsistencies and duplicate citations before final submission. I also used ChatGPT to identify potential word count reduction across sections, however I made all the decisions on whether and how to accept them myself. Delegating these technical tasks to AI tools allowed me to dedicate greater attention to doing actual research.

I also used AI tools for coding. For example, I used Claude to help with different dataset mergings, implementing my ideas for operationalization (for example, doing sector analysis for aid, computing sanctions intensity, doing temporal analysis), and generating visualizations (figures 2-7), tables (1-4), and appendices formatted as Word tables. I reviewed, ran, and verified all code myself in RStudio, ensuring I understood the logic and confirmed that outputs were accurate before drawing any conclusions. I also used Gemini as a brainstorming assistant when considering robustness checks or alternative model specifications. No AI methodological recommendation was accepted without independent verification.

All analytical decisions, interpretations, and conclusions presented in this thesis are my own. AI coding assistance enabled me to implement a more rigorous analytical structure, particularly in dataset merging and robustness checks across multiple analytical dimensions. While AI tools handled much of the technical coding work, all substantive decisions, research design choices, and interpretations were made and verified by me.