Natural Disaster and Conflict Dynamics in Civil War

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Abstract

With ongoing climate change and rising frequency of extreme weather events, understanding the impact of natural disasters on conflict becomes increasingly urgent. This thesis explores under what conditions and how natural disasters affect conflict dynamics during civil war. Across three papers, I argue that the impact of natural disasters is not unconditional but conditional on the strategic environment of civil war, which changes the vulnerabilities, incentives, and expectations of armed actors. I develop theory and empirical evidence for three strategic environments: (1) the spatial configuration of territorial control, (2) the role of third-party commercial actors, and (3) armed actors' anticipation of future disasters. This thesis contributes to the climate–conflict literature by demonstrating conditions under which natural disasters affect conflict dynamics. It also contributes to broader theories of conflict escalation and de-escalation, particularly debates on the impact of exogenous negative shocks and commitment problems on conflict. Furthermore, it introduces a new typology of spatial configurations of territorial control, develops a new measure of rainfall-related disasters, and includes a previously under-explored actor – a third-party commercial actor – into the study of civil war under sudden and negative shocks. It also develops a new empirical design to estimate the causal impact of disaster anticipation on conflict.

In the first paper, *The Impact of Natural Disasters on Ongoing Civil War: The Role of Spatial Configuration of Territorial Control*, I examine how the pre-disaster territorial configurations moderate post-disaster conflict dynamics by introducing a typology of spatial configurations of territorial control and developing a satellite-based, rainfall-related disaster measure. Using the ongoing civil war between the New People's Army (NPA) and the Philippine government as an empirical context, I demonstrate theoretically and empirically that, depending on the type of pre-disaster territorial configurations, disasters trigger different armed actors' strategies and thus lead to different outcomes regarding shifts of territorial control and battle-related violence. The second paper, *Buying Stabil-*

ity After Sudden and Negative Shocks: How Extractive Companies Dampen Post-Disaster Conflict, analyzes how third-party extractive companies moderate post-disaster conflict dynamics. Using fine-grained geospatial data on different types of natural disasters and resource extraction sites in Myanmar, I show that the presence of companies operating capital-intensive resource extraction sites dampens post-disaster violence, motivated by their incentives to stabilize local conditions in order to restore operations and revenue quickly. In support of my argument, I also demonstrate empirically that this mitigating effect fades as extraction licenses approach expiration. In the third paper, *Anticipation, Commitment Problems and Civil War Violence: Evidence from Normal vs. Flash Droughts*, coauthored with Dr. Mahmoud Osman (NASA), we examine the impact of anticipation of upcoming droughts on conflict, using the fine-grained geospatial data on droughts and conflict across Sub-Saharan Africa. We develop a credible design that isolates the causal effect of anticipation by contrasting two climatic shocks with comparable agro-economic impact but different levels of predictability: anticipated normal droughts and unanticipated flash droughts. The empirical results support the anticipation-based commitment problem by revealing that violence increases before normal drought but not before flash drought. Taken together, this thesis highlights the conditionality of the impact of natural disasters and has actionable policy implications for anticipating where and when increasingly extreme weather events are most likely to have destabilizing effects.

Declaration

I certify that the thesis presented for examination for the PhD degree at Durham University is solely my own work, except where it is clearly indicated that the work has been co-authored with another person. In such cases, the extent of my contribution and that of any other author is clearly identified. The copyright of this thesis rests with the author. Quotation from it is permitted, provided that full acknowledgment is made. The thesis may not be reproduced without my prior written consent.

Structure of the thesis

This thesis is structured as a three-paper thesis. Chapter 1 constitutes the first paper, Chapter 2 the second paper, and Chapter 3 the third paper. Chapters are organized to form a coherent research agenda that addresses the central research questions of the thesis. Tables and figures are numbered by chapter (e.g. Table 1.1, Figure 2.3), reflecting the three-paper structure. I declare that this thesis consists of 56,316 words.

Statement of co-authored work

I confirm that Chapter 3 of this thesis was co-authored with Dr. Mahmoud Osman. I contributed to the overall argument of the chapter, the theory, research design, conflict data collection, empirical analysis, and the drafting of the full chapter. Dr. Mahmoud Osman contributed by providing guidance on the selection and interpretation of appropriate drought measures and by supplying the flash drought data used in the analysis.

Statement of prior publication and public availability

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Introduction

Natural disasters have increasingly been framed as threat multipliers and have attracted growing attention from the public and policy-makers in the context of ongoing climate change. According to Intergovernmental Panel on Climate Change (IPCC)'s Sixth Assessment Synthesis Report (2023), while climate change does not drive all types of natural disasters, it is expected to increase the frequency and intensity of several major extreme weather events, including heatwaves, heavy precipitation, droughts, and severe storms. As a result, many countries are increasingly exposed to natural disasters not as rare shocks, but as recurring events, amplifying the policy relevance of understanding how natural disasters interact with armed conflicts.

These concerns are particularly salient because the countries most vulnerable to natural disasters are often also those most vulnerable to armed conflicts. According to the World Risk Report (2025), more than half of the ten countries ranked highest in overall disaster risk, including the Philippines, India, Indonesia, Colombia, Myanmar, Mozambique, Pakistan, Somalia, and Yemen, are characterized by political instability or ongoing armed conflicts. This overlap has further raised attention to the potential security implications of climate change and natural disasters.

Correspondingly, international institutions have increasingly recognized climate change as a security-relevant issue. Since the IPCC's Fifth Assessment Report (2014), climate change and particularly extreme weather events have been explicitly discussed as a factor that can increase the risk of violent conflicts. In its most recent sixth assessment cycle (2021-2023), the IPCC further emphasizes that climate extremes and disasters constitute an important pathway through which climate change may affect security outcomes.

Beyond scientific assessments, climate-related security concerns have become prominent in policy debates. The United Nations Security Council has acknowledged that climate

change can undermine stability, for example, noting in Resolution 2349 (2017), the adverse effects of climate change on security in the Lake Chad Basin. Similarly, North Atlantic Treaty Organization (NATO) (2021), European External Action Service (EEAS) (2022), and the White House (2014) have described climate change as a threat multiplier.

Despite growing international, policy, and public attention, empirical evidence on the relationship between natural disasters and armed conflicts, particularly in the context of ongoing civil war, remains contested. This is reflected in substantial real-world variation in the way conflict dynamics evolves after natural disasters. In some contexts, natural disasters appear to coincide with increased tensions and conflict. In others, similar disasters do not lead to increased violence and may even coincide with cooperation or progress toward peace. For example, in the Philippines, following super typhoon Haiyan (Yolanda) in 2013, the dynamics of armed conflicts differed markedly across affected regions. In the immediate aftermath of the disaster, the New People's Army (NPA) declared a temporary unilateral ceasefire (Philippine Daily Inquirer, 2014). However, at the same time, the violence did not subside uniformly. The clashes continued sporadically in parts of the affected regions, along with accusations that insurgent groups sought to exploit civilian grievances related to aid distribution. In parallel, government forces expanded their presence in disaster-affected areas, using relief and security operations to weaken the influence of the NPA (Walch, 2014, 2018). Taken together, the Haiyan case illustrates how a single disaster can simultaneously generate incentives for restraint, opportunities for strategic advantage, and various conflict responses across space.

Similar patterns of variation appear in flood and drought-affected contexts such as Pakistan and the Horn of Africa, where natural disasters are associated with increases in violence in some regions and periods, but with stabilization or reduced conflict in others (Ghorpade, 2020; Maystadt & Ecker, 2014). This real-world heterogeneity suggests that the impact of natural disasters on civil war is conditional. It thus motivates the central question of this thesis: under what conditions and through what mechanisms do natural disasters affect conflict dynamics during ongoing civil war? This thesis contributes to the climate–conflict literature by demonstrating conditions under which natural disasters affect conflict dynamics. It also contributes to broader theories of conflict escalation and de-escalation,

particularly debates on the impact of exogenous negative shocks and commitment problems on conflict. Furthermore, it introduces a new typology of spatial configurations of territorial control, develops a new measure of rainfall-related disasters, and includes a previously under-explored actor – a third-party commercial actor – into the study of civil war under sudden and negative shocks. It also develops a new empirical design to estimate the impact of anticipation about approaching climatic shocks on conflict.

Given growing concern about the security implications of climate change, the existing literature on climate, natural disasters, and conflict has expanded rapidly. This body of work, spanning research on climate-conflict links, natural disaster-conflict links, and conflict escalation and de-escalation, provides important insights into how natural disasters may affect conflict dynamics during ongoing civil wars.

Early contributions to the climate-conflict literature focused on identifying the effects of climate shocks, arguing that climate shocks increase conflict. A foundational study is Miguel et al. (2004), which uses rainfall variation as an instrumental variable for economic growth and shows that negative economic shocks increase the risk of civil conflict. This approach motivated a wave of subsequent literature linking temperature or precipitation deviations to higher levels of interpersonal and intergroup violence (Burke et al., 2009; Hsiang et al., 2011; O'Loughlin et al., 2012). Among those is Burke et al. (2009)'s analysis, which concludes that climatic deviations are associated with increased conflict risk across the continent of Africa.

This early positive effects wave has triggered sustained criticism, particularly regarding its reliance on reduced-form estimates that treat climate stress as having unconditional effects on conflict (e.g., see review articles, including Salehyan (2014); Buhaug et al. (2014); Selby (2014); Burke et al. (2015); von Uexkull and Buhaug (2021)). A prominent challenge is the Buhaug (2010) reanalysis of African civil war data, which challenged Burke et al. (2009)'s argument that climate change directly drives conflict. Related critiques emerged from the "adaptation and conditionality" strand, arguing that climatic stress influences conflict only under specific political and socio-economic conditions, including economic vulnerability, weak political institutions, and limited adaptive capacity (Bernauer et al.,

2012; Koubi et al., 2010, 2012; Theisen et al., 2013). Together, those critiques shift the debate away from whether climate stress increases conflict towards when, how, and under what conditions it does so.

In response to these critiques, the more recent literature has increasingly focused on identifying the mechanisms that link climatic shocks to conflict outcomes. The proposed pathways include livelihood shocks (Maystadt & Ecker, 2014; von Uexkull et al., 2016), food insecurity (Bellemare, 2015), and displacement (Abel et al., 2019; Reuveny, 2007)[1]. They suggest that climate stress should be understood as a risk factor rather than a primary driver of armed actors and highlight the need to better understand the mechanisms (Burke et al., 2015; Mach et al., 2019; von Uexkull & Buhaug, 2021).

While much of the climate–conflict literature examines gradual climatic variability, another body of literature investigates natural disasters as distinct forms of environmental shocks. The increasing frequency and severity of climatic disasters are one of the most important implications of climate change (IPCC, 2018). Similarly to the broader climate-conflict debate, this literature increasingly emphasizes conditional effects and causal mechanisms rather than unconditional effects.

Empirical findings in the literature on natural disasters and conflict remain mixed. Some studies find that natural disasters exacerbate armed conflict (Berrebi & Ostwald, 2011b; Kikuta, 2019; Nel & Righarts, 2008), while others report null effects or even pacifying effects (Bergholt & Lujala, 2012; Omelicheva, 2011; Petrova, 2021; Slettebak, 2012). This divergence has reinforced calls to move beyond average effects and examine the mechanisms and contextual conditions through which disasters affect conflict dynamics (Ide et al., 2020; von Uexkull & Buhaug, 2021).

One strand of the literature emphasizes grievance-based mechanisms. Drawing on environmental security perspectives, scholars argue that natural disasters can intensify competition over scarce resources, undermine livelihoods, and increase dissatisfaction with government performance, thus increasing conflict risk (Homer-Dixon, 1999). Disasters

[1]See the review synthesis on those mechanisms in von Uexkull and Buhaug (2021).

may also generate psychological stress and frustration that turn into unrest or violence (Anderson & Bushman, 2003; Flores & Smith, 2013). Additionally, inadequate or unequal disaster responses can deepen perceptions of injustice and marginalization, particularly when governments are seen as failing to protect vulnerable populations (Hendrix, 2013).

A second set of explanations focuses on opportunity and capacity-based mechanisms. Natural disasters can alter the balance of capacities among conflict actors by damaging infrastructure, disrupting economic activity, and straining state finances. Several studies argue that such disasters weaken government capacity, creating windows of opportunity for rebel groups to intensify violence, expand territorial influence, or prolong conflict (Berrebi & Ostwald, 2011a; Eastin, 2016; Nardulli et al., 2015). Other work emphasizes that disasters can also affect insurgent organizations by disrupting supply lines, limiting mobility, and constraining recruitment, which shifts conflict dynamics in favor of the government (Kreutz, 2012; Tominaga & Lee, 2021; Walch, 2018).

In contrast to conflict-amplifying accounts, a third strand highlights social cohesion and pacifying mechanisms. Sociological theories of disaster response emphasize the emergence of a community of sufferers, in which shared exposure to hardship fosters solidarity and temporarily reduces preexisting social divisions (Fritz, 1996). Empirical studies drawing on this perspective suggest that disasters can suppress violence by encouraging cooperation among affected populations or shifting attention to survival and recovery (Kang & Skidmore, 2018; Slettebak, 2012). Relatedly, the literature on disaster diplomacy argues that humanitarian responses, both domestic and international, can facilitate short-term cooperation or build trust between rivals (Kelman, 2012).

Given the existence of these competing mechanisms, recent research has increasingly emphasized contextual moderators to explain heterogeneity in disaster-conflict outcomes. Existing studies examine how the effect of disasters varies with ethnic composition (Schleussner et al., 2016), local institutions and governance quality (Linke et al., 2018; Petrova, 2021), state capacity (Adam & Tsavou, 2022), post-disaster aid and humanitarian intervention (Juan et al., 2020), and disaster location (Nemeth & Lai, 2022; Rosvold & Buhaug, 2021).

Although existing literature identifies important contextual moderators, it remains comparatively undertheorized in explaining how disaster-induced shocks affect strategic interaction and bargaining among armed actors during civil war. Civil wars are often explained as a strategic interaction among actors whose behavior depends on expectations, incentives, and relative power. It is thus necessary to engage theories that explicitly model strategic interaction and bargaining during war to better explain why similar disasters lead to escalation in some contexts but restraint or de-escalation in others.

A central shift in the civil war literature has been a shift away from explaining conflict primarily in terms of onset or duration at the country-level (Collier et al., 2004; Fearon & Laitin, 2003) toward explaining within-war and subnational conflict variation (Kalyvas, 2006; Kalyvas & Kocher, 2007). This shift reflects a growing recognition that civil wars involve strategic interactions in which armed actors adapt their behavior in response to changing local conditions, rival actions, and information. Building on this perspective, many scholars conceptualize conflict escalation or de-escalation as the product of local strategic interaction, rather than aggregate structural pressures. Armed actors compete for territorial control, seek information from civilians, and adjust their violence in response to patterns of collaboration and defection (Balcells, 2017; Berman et al., 2011; Kalyvas, 2006; Kocher et al., 2011). Importantly, conflict escalation is not simply more violence, but often involves changes in the form and targets of violence, while de-escalation is also a strategic outcome rather than a passive decline in violence (Balcells & Stanton, 2021; Díaz Pabón & Duyvesteyn, 2023). Among those conflict outcomes, territorial control, which serves as a key goal for both belligerents in a civil war (Kalyvas, 2006; O'Sullivan, 1983), remains understudied in empirical research.

Strategic interaction becomes even more complex when civil wars involve more than two armed actors, where escalation and de-escalation are shaped by coordination problems, alliance dynamics, and internal fragmentation. Research shows that the entry of new actors or the split within armed groups can intensify violence through competition and infighting (Bakke et al., 2012; Cunningham, 2013; Fjelde & Nilsson, 2012). At the same time, fragmentation can also produce restraint when cooperation costs increase or when actors seek to consolidate territorial control and organizational authority (Christia, 2012;

Metelits, 2009; Pearlman & Cunningham, 2012). Those findings underscore that changes in conflict dynamics often occur from strategic interaction among several armed actors, responding to one another's moves, but with comparatively less attention to the role of non-armed actors such as commercial actors in shaping these interactions[2].

While the escalation and de-escalation literature shows how violence varies through local strategic interaction, bargaining theory provides a complementary theoretical explanation for why actors have incentives to escalate or restrain conflict during war. Rationalist accounts explain fighting as the result of bargaining failure driven by information asymmetries and commitment problems (Fearon, 1995; Powell, 2006). Information asymmetries, together with incentives to misrepresent private information about capabilities and resolve, generate uncertainty about opponents' strength that can prevent actors from identifying mutually acceptable settlements. At the same time, commitment problems may render even efficient agreements unenforceable, particularly when actors anticipate future shifts in relative power (Fearon, 1995; Powell, 2006; Spaniel, 2024). Furthermore, these problems are not limited to the war onset. As conflicts unfold, shifts in relative capabilities, uncertainty about opponents' strength, and doubts about credible commitment can change bargaining positions and affect conflict dynamics (Walter, 2009). Despite its theoretical prominence, the theory of bargaining during civil war remains difficult to test empirically[3].

Taken together, the literature on conflict escalation, de-escalation, and bargaining failure provides insights into how strategic interaction shapes conflict dynamics during ongoing civil war. This perspective is important for analyzing the impact of natural disasters, which constitute negative and sudden shocks that can alter the capacity, expectations, and relative bargaining positions of armed actors. By reshaping strategic environments, disasters may generate divergent conflict responses, depending on how actors anticipate and respond to one another.

[2]See Chapter 2 for further discussion of the role of commercial actors in shaping strategic interactions between government and rebel groups.

[3]See Chapter 3 for further discussion of the limitations of existing empirical efforts to test the commitment problem.

Among the literature on natural disasters and conflict, the closest strand with clear strategic implications is the strand that emphasizes the opportunity and capacity mechanisms. In this strand, disasters shift material constraints and resources, enabling or constraining one side's ability to fight (Eastin, 2016; Tominaga & Lee, 2021). Although these accounts recognize that disasters affect strategic behavior, they largely conceptualize disaster effects in unilateral terms, focusing on how shocks alter what one actor can do, rather than how armed actors strategically respond to one another.

There are a few exceptions that explicitly study natural disasters within the framework of strategic interaction and bargaining. Kikuta (2019) conceptualizes post-disaster reconstruction as generating a commitment problem over future rents and increasing post-disaster violence. Nemeth and Lai (2022) show that natural disasters shape bargaining incentives for negotiations depending on which actors are affected. And Bas and McLean (2021) suggest how the risk of natural disasters can drive interstate war through a model of commitment problems.

Although these studies mark an important advance in studying the impact of natural disasters within a framework of strategic interaction, they remain limited in several aspects. First, while existing studies focus on various outcomes, such as battle-related violence, group-level negotiation outcomes, or interstate war, less attention has been paid to shifts in territorial control, a crucial but less-studied conflict outcome. Second, these accounts exclusively focus on the strategic interaction between armed groups but ignore the role of non-armed third parties whose interests and capabilities may shape post-disaster conflict dynamics. Third, while these studies frequently use commitment problems as the underlying theoretical mechanism driving post-disaster strategic interaction, this mechanism has been difficult to test credibly in empirical settings. Finally, much of the literature relies on disaster measures such as EM-DAT that are often too coarse to capture the localized strategic environments in which civil war unfolds, and that may introduce endogeneity through definitions based on human implications[4].

[4]A few exceptions exploit identification advantages from a single sudden and severe disaster event (see Kikuta (2019) as an example).

Taken together, the literature reviewed above points to several avenues for further development. Further progress can be made through more attention to the conditions under which natural disasters shape conflict dynamics, a more systematic theorizing of disaster impacts within the framework of strategic interaction and bargaining, a more credible empirical design to test the commitment problems, and the use of more precise, plausibly exogenous measures of disaster exposure.

My thesis contributes to these areas in four ways. First, it advances the understanding of the conditions under which natural disasters affect conflict dynamics by identifying three moderators, including the spatial configuration of territorial control, the presence of third-party commercial actors, and armed actors' anticipation of future disasters. Second, it extends existing strategic interaction frameworks by analyzing how disaster shocks affect the outcome of shifts in territorial control - a central but underexplored outcome in civil war study. It also incorporates non-armed third parties, specifically extractive commercial actors, whose economic interests and shock-absorbing capacities influence how armed groups adjust their strategies following disasters, into the framework of the strategic interaction during civil war. Third, it provides a more credible empirical design to test the anticipation-based commitment problem by using the variation in predictability of two otherwise similar climatic shocks - anticipated normal and unanticipated flash droughts. Fourth, it improves measurement and identification by constructing spatially disaggregated indicators of disaster exposure using satellite-based precipitation data. This measure generates plausibly exogenous variation in local disaster intensity and allows for a closer match between environmental disruptions and the localized strategic environments in which civil war dynamics unfold.

The first paper introduces the spatial configuration of territorial control as a moderator and examines how it moderates the impact of natural disasters on battle-related violence and the shift in territorial control. It suggests that pre-shock spatial configurations of territorial control shape both the vulnerabilities exposed by a disaster and the opportunities that they create for armed actors, and thus lead to different conflict outcomes. Using a staggered difference-in-differences design and focusing on the case of the ongoing civil war between the Philippine government and the NPA, this paper finds that the impact of nat-

ural disasters on both battle-related violence and shifts in territorial control varies across different pre-shock spatial configurations of territorial control. In doing so, this paper first specifies a new moderator of the spatial configuration of territorial control to explain the variation in the relationship between natural disasters and conflict dynamics. Also, this paper addresses the measurement limitation in existing empirical research on natural disasters by introducing a new measure of rainfall-related natural disasters that captures the localized variation in disasters more precisely than existing disaster datasets. This paper thus contributes to the debate on climate, natural disasters, and conflict. Additionally, this paper analyzes how natural disasters differently affect both shifts in territorial control and battle-related violence, and recognizes that the shift of territorial control can occur in the absence of overt fighting. This extension is important because territorial control is a key strategic objective during civil wars and has been less studied in the literature. In doing so, this paper contributes to the literature on conflict escalation and de-escalation. It also offers policy insights for conflict prevention, providing policymakers with a more nuanced understanding of how conflict dynamics can shift in the aftermath of natural disasters. The findings can help identify potential security hotspots and inform resource allocation for conflict prevention and resolution, ultimately contributing to more stable and peaceful post-disaster environments.

The second paper examines how the presence of third-party commercial actors moderates the impact of natural disasters on conflict dynamics. It argues that where capital-intensive, insured, and credit-backed extraction sites operate (e.g., large-scale mining and oil and gas extraction), companies have both the incentive and the means to buy stability in the aftermath of disaster shocks so that operations can resume quickly, mitigating violence. Empirically, this paper focuses on the extractive industries in Myanmar, a context in which civil war, disaster exposure, and resource extraction interact. Using subnational, geo-referenced data on disasters, the location of extractive companies' license, and conflict events from 2010 to 2018, and a series of difference-in-differences designs, this paper finds that disaster shocks increase violence in areas without capital-intensive extraction, but not in areas with large extractive companies present. In this way, this paper advances the emphasis of conditionality in the disaster-conflict literature by identifying the presence of commercial actors as a moderator of post-disaster violence escalation and restraint

during the ongoing civil war. Additionally, it also contributes to the literature on conflict escalation and de-escalation by including an ignored actor in the existing framework of strategic interaction - third-party commercial actors - in shaping post-shock strategic interaction between armed actors. Seminal conflict models emphasize how sudden and negative shocks change power relations between different armed actors, but largely ignore commercial actors whose economic interests may materially influence wartime strategies of armed actors, especially in resource-rich conflict settings. By integrating commercial actors into models of wartime strategic interaction under sudden and negative shocks, this paper shows how disasters can reshape conflict dynamics not only through their direct effects on armed actors, but also indirectly through the presence of those commercial actors with strong incentives to stabilize post-disaster environments.

The third paper examines how the anticipation of future climatic shocks shapes conflict dynamics. Theories of bargaining during civil war emphasize that anticipation about future shifts in power can generate incentives for preemptive violence before material conditions change. However, existing empirical research has struggled to isolate the effects of anticipation from other factors. This paper addresses this challenge by exploiting variation in the predictability of two otherwise comparable droughts, distinguishing between gradually developing and largely anticipated normal droughts and rapid-onset, difficult-to-anticipate flash droughts that generate comparable agricultural consequences. Using fine-grained data on these drought types and civil conflict across Sub-Saharan Africa, the analysis compares patterns of civil conflict before and after different forms of drought exposure, thereby isolating anticipation as the key mechanism driving pre-disaster conflict. The findings show that civil conflict increases prior to anticipated droughts but not before unanticipated ones, providing direct empirical evidence consistent with anticipation-based commitment problems. In doing so, this paper advances the disaster–conflict literature by demonstrating that expectations about future environmental shocks, rather than realized impacts alone, condition when and how disasters influence conflict dynamics. It also contributes to the literature on conflict escalation by providing credible empirical evidence for anticipation-based commitment problems. On the policy side, evidence that anticipation, not just its realized impacts, increases conflict suggests the need to move from reactive crisis response to forecast-based prevention. Advances in prediction technologies for

climatic shocks, such as satellite-informed early-warning platforms and AI-enhanced climate forecasting, make such timing increasingly feasible. Policymakers and international mediators can integrate these tools to identify where anticipated shocks can alter local bargaining power and engage in preventive interventions.

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Chapter 1

The Impact of Natural Disasters on Ongoing Civil War: The Role of Spatial Configuration of Territorial Control

Abstract

Understanding how natural disasters impact ongoing civil wars is increasingly important as more extreme weather events are expected due to climate change. Existing literature suggests that there is substantial unexplained variation in the consequences of natural disasters for conflict. I present a theoretical model, which provides insights into how the pre-disaster spatial configuration of territorial control can moderate the impact of natural disasters on conflict dynamics. Investigating these insights empirically, I introduce a new measure of natural disasters based on precipitation data, which improves geographical precision and mitigates endogeneity concerns in existing literature. Using the ongoing civil war between the New People's Army (NPA) and the Philippine government as a testing ground, results show that the impact of natural disasters on both shifts in territorial control and battle-related violence varies by the pre-disaster spatial configuration of territorial control.

Keywords: climate change, natural disasters, civil war, territorial control

Introduction

How do natural disasters affect ongoing civil wars? Given the rise in extreme weather events due to climate change, understanding the security implications of natural disasters is of increasing political and policy relevance (IPCC, 2018). Among policymakers, natural disasters are often viewed as a conflict "threat multiplier" (McDonald, 2018). The wars in Darfur and Syria are often labeled climate wars, as pre-war droughts were contributing

factors (Conca, 2019; Hendrix, 2017). However, within a country experiencing natural disasters, the response of armed actors can vary significantly, with some regions witnessing more violence while others remain peaceful. For instance, in the Philippines, following the devastating 2013 Super Typhoon Haiyan (locally known as Yolanda), a truce was declared between the military and the New People's Army (NPA), which temporarily reduced violence in disaster-stricken regions such as Leyte and Samar. However, violence from both sides still erupted sporadically in parts of these regions. The NPA was accused of "exploiting the disaster to their advantage" by attacking government relief efforts and manipulating civilian grievances (Impact News Service, 2019; Philippine Daily Inquirer, 2014). At the same time, the military strategically expanded its influence by weakening the NPA, including targeted killings of NPA leaders and the deployment of additional troops to affected regions (Walch, 2014).

These divergent empirical observations suggest that the impact of natural disasters varies across territories. This raises the question: Why do natural disasters escalate conflict dynamics in some territories, while in others they do not? This paper addresses this question by considering an understudied factor: the spatial configuration of territorial control in the location struck by a disaster. I introduce a typology of the spatial configuration of territorial control, which considers the control of both the given territory and its neighboring territories. I argue that the impact of natural disasters differs across three pre-disaster spatial configurations of territorial control: 1) the homogeneous setting, where the affected territory is encompassed by the territories controlled by the same actor; 2) the enclave setting, where the affected territory is encircled by the territories controlled by the rival; 3) the mixed setting, where the affected territory is surrounded by multiple territories, some of which are controlled by the same actor, others controlled by the rival.

I present a theoretical framework in which natural disasters are treated as exogenous shocks that disrupt the local power balance. Natural disasters create opportunities for belligerents to exploit the temporary weaknesses of affected opponents to expand territorial control. However, the extent of this opportunity is conditional upon the pre-disaster configuration of territorial control. More specifically, I suggest that in a homogeneous setting, logistical constraints prevent the rival from exploiting the disaster-induced weakness. In an enclave

setting, the weakened force is highly vulnerable, leading to shifts in territorial control with minimal battle-related violence. In a mixed setting, the defender may either reinforce the territory, resulting in increased battle-related violence without shifts in territorial control, or abandon it, producing conflict dynamics similar to those in an enclave setting. Additionally, since governments tend to have superior logistical capacity, they can reinforce affected territories more effectively than rebels. Consequently, natural disasters have a more pronounced effect on rebel-controlled territories.

Empirically, I employ a difference-in-differences design (DID) to explore the role of the pre-disaster spatial configuration of territorial control in moderating the relationship between natural disasters and two conflict outcomes - shifts in territorial control and battle-related violence. To study shifts in territorial control, I use fine-grained spatial data on natural disasters and territorial control at the barangay-year level in the Philippines (excluding the Autonomous Region in Muslim Mindanao (ARMM)[1]) between 2012 and 2014. The Philippines, which experienced frequent rainfall-related natural disasters such as storms or floods, and has an ongoing civil war between the NPA and the government, provides an ideal testing ground. For the analysis of battle-related violence, I use barangay-month level data from the Davao region between 2012 and 2014. Located in southeastern Mindanao, the Davao region was a hotbed for NPA activity during the study period.

Supporting the theoretical expectation, I find that the impact of natural disasters on shifts in territorial control and battle-related violence varies depending on the pre-disaster spatial configuration of territorial control. The empirical findings suggest that, after natural disasters, shifts in territorial control increase in mixed and enclave settings, but not in the homogeneous setting. Battle-related violence increases in the mixed setting only. Additionally, the impact of natural disasters on shifts in territorial control in mixed and enclave settings is more pronounced in rebel-controlled territories.

This paper offers several contributions. First, I introduce a new moderator that explains

[1]Due to the limitations in the data on territorial control, the dataset excludes the Autonomous Region of Muslim Mindanao (ARMM). This region was primarily affected by the Moro conflict between the Philippine government and Muslim separatist groups. During the study period, a peace process was ongoing, which ultimately led to a comprehensive peace deal between the government and the Moro Islamic Liberation Front (MILF) in 2014.

variation in the relationship between natural disasters and conflict dynamics. Existing literature that emphasizes the importance of disaster location focuses on whether natural disasters hit conflict zones (Rosvold, 2019), or on which combatant is victimized (Nemeth & Lai, 2022). Instead, I introduce a typology of spatial configurations of territorial control and analyze the heterogeneous effects of natural disasters across these pre-disaster spatial configurations. Second, I analyze how natural disasters differently affect both shifts in territorial control and battle-related violence. This extension is important because territorial control is a key strategic objective during civil wars. By focusing on the occurrence of battle-related violence alone, existing literature overlooks shifts in territorial control that may occur without explicit acts of violence, such as strategic withdrawals. Third, I address limitations in existing empirical research on natural disasters by introducing a new measure of rainfall-related natural disasters. Commonly used datasets, such as the Emergency Event Database (EM-DAT), suffer from the incomplete subnational coverage and endogeneity concerns due to the definition of natural disasters based on their human implications. To mitigate these issues, I construct a new measure using precipitation data from NASA's Integrated Multi-satellite Retrievals for GPM (IMERG).

Literature Review

Existing literature presents various theoretical mechanisms and empirical predictions on how natural disasters affect conflict dynamics. Some suggest an adverse effect of natural disasters on conflict dynamics, arguing that such disasters exacerbate violent conflicts (Kikuta, 2019; Nel & Righarts, 2008), fuel terrorist attacks (Adam & Tsavou, 2022; Berrebi & Ostwald, 2011), prolong conflict duration (Eastin, 2016), or escalate repression (Wood & Wright, 2016).

The literature identifies two primary theoretical mechanisms that underlie empirical findings. First, natural disasters may exacerbate grievances. The environmental security literature suggests that natural disasters contribute to escalating "simple scarcity conflicts" (Homer-Dixon, 1999). Additionally, natural disasters are argued to have psychological implications (Kelman, 2012; Slettebak, 2012). Scholars posit that disasters can increase individuals' discomfort and aggression, potentially leading to increased violence or trigger-

ing other forms of unrest, including protest (Anderson & Bushman, 2003; Flores & Smith, 2013).

Second, natural disasters may widen opportunities for conflict. Eastin (2016) indicates that natural disasters damage infrastructure and strain state finances, thereby weakening the government's capacity to combat rebel groups. This creates a window of opportunity for rebels to escalate conflict intensity (Berrebi & Ostwald, 2011), or prolong conflict duration (Nardulli et al., 2015). Others focus on the impact on rebels, arguing that these disasters disrupt their supply lines and paralyze their mobilization capacity. Consequently, this may shift conflict dynamics in favor of the government, potentially resulting in a reduction in conflict intensity or enabling increased government territorial reclamation (Kreutz, 2012; Tominaga & Lee, 2021; Walch, 2018).

In contrast, an alternative line of research emphasizes the potential pacifying effect of natural disasters. Scholars in the sociology of natural disasters propose the formation of a "community of sufferers" built upon inter-subjective understanding, which helps mitigate preexisting divides and deter violence (Fritz, 1996; Kang & Skidmore, 2018; Slettebak, 2012). Likewise, the disaster diplomacy literature argues that humanitarian activities and international involvement, together with domestic emergency responses in disaster-stricken regions, can contribute to short-term peace-building efforts (Kelman, 2012).

The divergent findings underscore the significance of moderators, with an emerging consensus suggesting that the impact of natural disasters on conflict dynamics varies across contexts (von Uexkull & Buhaug, 2021; Xu et al., 2016). Various moderators have been explored in existing literature, including ethnic heterogeneity (Schleussner et al., 2016), local institutions (Linke et al., 2018; Petrova, 2021), state capacity (Adam & Tsavou, 2022), post-disaster aid (Juan et al., 2020), or rebel–civilian relationships (Walch, 2014).

Following the emphasis on moderators, some scholars suggest the significance of disaster location in understanding how natural disasters affect conflict dynamics. Instead of solely evaluating whether a natural disaster occurs, it is important to disaggregate its effects based on where the disaster occurs. Existing research explores disaster location as a

moderator by investigating whether natural disasters occur within conflict zones (Rosvold, 2019) or which combatant is victimized (Nemeth & Lai, 2022). Despite their contributions, these studies focus on a single dimension - either the geographical location of the disaster or the identity of the affected actor. By focusing on the pre-disaster spatial configuration of territorial control, which considers the territorial control of both the given territory and its neighboring territories, this paper not only considers where disasters occur and who is impacted, but also the strategic interactions among belligerents.

Additionally, as reviewed above, the existing literature has examined various conflict outcomes. However, territorial control, which serves as a key goal for both belligerents in a civil war (Kalyvas, 2006; O'Sullivan, 1983) remains understudied in empirical research. Shifts in territorial control can still occur without explicit acts of violence. This is particularly evident when the defending force is incapacitated due to natural disasters. Thus, it is necessary to examine battle-related violence as a means and shifts in territorial control as an outcome.

Finally, the existing literature is notably restricted by the measure of natural disasters. Previous research often relies on event-level datasets such as EM-DAT. However, this approach faces criticism (Rosvold & Buhaug, 2021; Tin et al., 2024). First, these datasets often lack a complete subnational coverage of natural disasters, limiting the feasibility of within-country research designs. For example, EM-DAT includes many cases where location names cannot be matched to a specific geographical area or refer to multiple locations within the same country (Rosvold & Buhaug, 2021). Second, the EM-DAT definition of natural disasters relies on human and social vulnerability, which introduces endogeneity concerns, as socioeconomic attributes such as poverty or ineffective governance may contribute to both vulnerability to natural disasters and conflict dynamics (Felbermayr & Gröschl, 2014; Osberghaus, 2019). To address these limitations, I employ a new measure of natural disasters based on precipitation data from IMERG.

Theory

This paper focuses on the type of civil war in which rebels cannot move military resources freely, while governments retain greater mobility and logistical advantages. Also, it examines conflicts between two direct rivals, though the theory has the potential to extend to civil wars involving multiple armed actors. The case of the Philippines meets these scope conditions, as it centers on the direct contest between the Philippine government and the NPA[2].

Both territorial control and contention are important concepts in the study of civil wars. Territorial control serves as a key goal for belligerents involved in civil wars (O'Sullivan, 1983). Governments seek to maintain control over territory within their boundaries. Rebels often pursue the opposite - working toward goals such as overthrowing the government or achieving secession (Hammond, 2018). Contention, often manifested through battle-related violence, serves as a strategic means to contest or defend territorial control (Kalyvas, 2006).

While shifts in territorial control and contention are interrelated, they are distinct phenomena. Shifts in territorial control may occur without battle-related violence, especially in the aftermath of exogenous shocks such as natural disasters. In such scenarios, the defending force may become incapacitated due to the impact of natural disasters and subsequently choose to retreat or surrender, thus relinquishing the territory. Conversely, battle-related violence can occur without any shift in territorial control. For instance, failed offensives, where an attacking force is unable to break through strengthened defensive lines, often result in intense fighting and high casualties without altering territorial control (Kalyvas, 2006). I examine both shifts in territorial control and contention in tandem, which provides a more comprehensive understanding of how these two conflict outcomes evolve during civil wars.

Scholars and practitioners have long argued that the distribution of military power is

[2]The Philippine case used in this paper excludes regions such as Mindanao, where additional Muslim rebel groups operate and focuses specifically on the conflict between the government and the New People's Army (NPA)

pivotal in determining territorial control, with the belligerent holding a relative power advantage often winning a given territory (Kovenock & Roberson, 2012; Roberson, 2006). This power advantage broadly depends on military resources, including civilian collaboration, infrastructure, and military personnel and equipment.

First, civilian collaboration is important because it influences both counterinsurgency tactics and rebel violence (Kalyvas, 2006; Sonin & Wright, 2023). In counterinsurgency operations, information on rebel activities is considered 'a central resource within civil wars' (Lyall et al., 2013), and rebels similarly rely heavily on civilian collaboration (Sonin & Wright, 2023). Second, infrastructure, such as roads, bridges, and communication networks, plays a pivotal role by enhancing the military efficacy of counterinsurgency (Eastin, 2016). This argument also extends to rebels, since such infrastructure serves as key supply lines that influence their combat capabilities (Hammond, 2018; Walch, 2018). Third, personnel and equipment are indispensable for both belligerents and can be deployed across multiple territories, thus shaping local power dynamics (Eastin, 2016).

An exogenous shock that disrupts the prior power balance is the occurrence of natural disasters. Natural disasters can significantly alter civilian support, destroy infrastructure, and impact military resources. First, natural disasters threaten civilian survival and influence their collaboration decisions. Civilians may inform the rival when the opportunity cost of collaboration with the rival falls and grievances against the de facto controller rise (Hendrix, 2013; Walch, 2018). Second, natural disasters may damage infrastructure, personnel, and equipment in the affected territory, thereby weakening the military capacity of the de facto controller, including either government forces or rebels (Eastin, 2016; Tominaga & Lee, 2021).

Thus, natural disasters disrupt the balance of power between belligerents. This disruption creates opportunities for both shifts in territorial control and battle-related violence, depending not only on the extent to which one belligerent is weakened, but also on the pre-disaster spatial configuration of territorial control. Belligerents strategically choose their optimal targets based on the costs of attack and the rival's capacity to reallocate their personnel and equipment for defense. These strategic considerations are shaped by

the pre-disaster spatial configuration of territorial control.

I propose a typology of pre-disaster spatial configurations of territorial control. This typology accounts for the territorial control of both the affected territory and its neighboring territories. It includes three empirically exhaustive categories: the homogeneous setting, where the affected territory is encompassed by the territories controlled by the same actor, the enclave setting, where the affected territory is encircled by the territories controlled by the rival, and the mixed setting, where the affected territory is surrounded by multiple other territories, some of which are controlled by the same actor, others controlled by the rival, as shown in Figure 1.1.

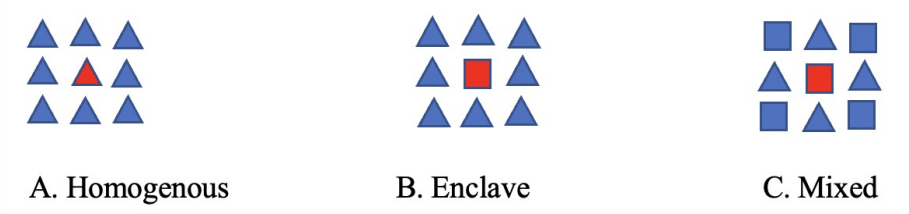

Figure 1.1: Territorial Settings

Note: the square and triangle represent territories controlled by two different belligerents. Light-colored squares and triangles indicate the central territories.

I consider both contiguous and non-contiguous movements. Under contiguous movement, both belligerents allocate military personnel or equipment to the affected territory only from neighboring territories, as reallocation from non-adjacent territories is prohibitively costly. Contiguous movement provides a simplified baseline for understanding how spatial configurations influence post-disaster shifts in territorial control and battle-related violence. I also extend the analysis to non-contiguous movement, in which one belligerent – often the government – possesses superior logistical capacity such as the use of air transport to reinforce its defense (Walch, 2018). While this capacity introduces additional strategic options, reliance on adjacent territories remains critical in determining how territorial control is contested[3].

[3]This distinction ensures that the theory applies across a wide range of civil war contexts, including the case of the Philippines, where the government exhibits logistical superiority but remains constrained by contiguous movement in routine military operations

Under contiguous movement, I theorize that the impact of natural disasters on shifts in territorial control and battle-related violence varies across different spatial configurations of territorial control, as they affect post-disaster military strategies, including the cost of moving military personnel and equipment for offensive operations, and the rival's capacity to reallocate forces to reinforce defense.

In the homogeneous setting, natural disasters weaken the military capacity of the controller in the affected territory. However, because the affected territory within this setting is surrounded by its own controlled territories, the configuration, combined with limited logistical capacity on both sides, makes a successful attack on the affected territories highly unlikely. Belligerents can only move personnel or equipment into directly adjacent territories, which makes it prohibitively costly for the rival to initiate an attack across multiple territories. Thus, I expect that:

H1.1: In the homogeneous setting, natural disasters have no effect on the likelihood of a shift in territorial control in the affected territory.

H1.2: In the homogeneous setting, natural disasters have no effect on the likelihood of battle-related violence in the affected territory.

In the enclave setting, natural disasters also weaken the actor controlling the affected territory, making it more vulnerable to become a target. At the same time, the unique spatial configuration of the enclave setting enables the rival to exploit the post-disaster power imbalance and seize control of the affected territory. The rival can reallocate military personnel and equipment from its adjacent controlled territories to launch an attack, while the affected actor cannot move forces across adjacent rival-controlled territories to reinforce the affected territory. In this power-imbalanced context, the defending force is at a severe disadvantage and is often overwhelmed or forced to retreat. Thus, I expect that

H2.1. In the enclave setting, natural disasters increase the likelihood of a shift in territorial control in the affected territory.

H2.2. In the enclave setting, natural disasters do not increase the likelihood of battle-related violence in the affected territory.

In the mixed setting, natural disasters weaken the controller in the affected territory. However, its unique spatial configuration introduces more complex strategic considerations for the affected actor. The mixed setting is characterized by the affected territory being encircled by territories, some of which are controlled by the affected actor, and others controlled by the rival. The affected actor faces a strategic decision: whether to defend the affected territory or abandon it. They may choose to reinforce the territory by reallocating military personnel or equipment from adjacent controlled territories, or opt for a strategic withdrawal. The decision depends on whether the value of the affected territory outweighs the costs of defending it, including the risk of losing the territories from which the defending forces are drawn. If the former outweighs the latter, the affected actor is more likely to defend the affected territory from being taken control by the rival. As a result, battle-related violence is more likely, as the rival attempts to exploit temporary weakness by attacking it, while the territory's strategic value draws in more defending forces. If the latter outweighs the former, the affected actor is more likely to abandon the territory, resulting in a scenario that mirrors the enclave setting. Thus, I expect that

H3.1. In the mixed setting, natural disasters increase the likelihood of a shift in territorial control in the affected territory, but this effect is smaller than in the enclave setting.

H3.2. In the mixed setting, natural disasters increase the likelihood of battle-related violence in the affected territory.

The impact of natural disasters on shifts in territorial control may spillover into adjacent unaffected territories, as the affected actor reallocates military personnel and equipment from those adjacent territories to defend the disaster-affected territory. In civil wars, belligerents often target the most vulnerable territories, including those affected by natural disasters. However, when the affected territory is considered highly valuable, the affected belligerent may choose to reallocate military personnel and equipment from adjacent territories to defend it (Eastin, 2016; Powell, 2007). This reallocation disrupts the power

balance in the territories from which military personnel and equipment are moved out, rendering them more vulnerable. As a result, the affected actor is more likely to strategically abandon those less valuable territories without engaging in battle-related violence. Thus, I expect that

H4.1. Natural disasters increase the likelihood of a shift in territorial control in adjacent unaffected territories.

H4.2. Natural disasters do not increase the likelihood of battle-related violence in adjacent unaffected territories.

Additionally, I examine an extended form of non-contiguous movement, in which the government can move personnel and equipment from non-adjacent territories (reflecting e.g. air transport capacity), while rebels can only mobilize personnel and military equipment from adjacent territories. Following natural disasters, rebels face logistical challenges in reinforcing their defense due to limited access to infrastructure and equipment, whereas governments can allocate resources nationwide (Walch, 2018). Consequently, the impact of natural disasters is more pronounced in rebel-controlled territories and varies depending on the pre-disaster spatial configuration of territorial control. In homogeneous settings, given the high cost of cross-territory attacks, I expect that the lack of the impact from natural disasters on two conflict outcomes remains unchanged.

In contrast, in enclave settings, the government can reinforce affected territories by reallocating military personnel and equipment from more distant territories, leveraging its superior logistical capacity. A similar military reallocation can also be observed in mixed settings. As a result, rebels face greater difficulty in seizing control of the affected territory due to the potential for defense reinforcements. As defending forces are reinforced in government-controlled territories, the likelihood of battle-related violence may increase, as rebels encounter stronger resistance during attempted attacks. Thus, I expect that

H5.1 (Under non-contiguous movement). In the enclave and mixed settings, natural disasters increase the likelihood of a shift in territorial control in the affected territory, with

the impact more pronounced in rebel-controlled territory.

H5.2 (Under non-contiguous movement). In the enclave and mixed settings, natural disasters increase the likelihood of battle-related violence in the affected territory, with the impact more pronounced in government-controlled territory.

Research Design

I empirically test my hypotheses using a DID design to analyze the impact of natural disasters on two conflict outcomes. For shifts in territorial control, I study the civil war between the NPA and the Philippine government between 2012 and 2014 at the barangay-year level. For battle-related violence, I focus on the Davao region only between 2012 and 2014 at the barangay-month level[4]. The barangay is selected as the unit to conceptualize the territory because it represents the smallest administrative and governance unit in the Philippines, making it an ideal level for examining localized conflict dynamics. As the primary site of state governance and community interaction, the barangay is also a focal point of NPA activity (Richard, 1989). This alignment is further reflected in counterinsurgency guidelines, which prioritize interventions and data collection at the barangay level[5].

The Philippines serves as an optimal testing ground due to its frequent exposure to rainfall-related disasters and the ongoing communist insurgency. Nemeth and Lai (2022) examine how the victimization of combatants by natural disasters influences negotiation outcomes, citing the Philippines as a key illustrative case. Additionally, qualitative research by Walch (2014) shows that government forces intensified the military operations in NPA-controlled territories following the Typhoon Bopha in 2012, which hit territories under NPA control. This empirical evidence aligns with my theoretical expectation.

[4]The choice of time period is restricted by the data on territorial control from AFP intelligence assessments. The period between 2012 and 2014 was during the presidency of Benigno Aquino III, and was marked by "fierce attacks on the NPA and the entire revolutionary movement"(PRWC, 2019).

[5]The barangay corresponds to the fourth and smallest subnational administrative (ADM4) level in the Philippines, and is grouped into 1647 municipalities (ADM3) in 87 districts (ADM2) located in 17 regions (ADM1).

Conflict Outcomes

I focus on two outcomes: shifts in territorial control and the occurrence of battle-related violence. Two datasets are constructed. For shifts in territorial control, the dataset encompasses 42,029 barangays across 1,647 municipalities in the Philippines (excluding the ARMM). For battle-related violence, due to data availability, the dataset is confined to the Davao region, where the NPA was most active during the specified three-year period. It comprises 1,161 barangays across 49 municipalities.

I measure shifts in territorial control using the Armed Forces of the Philippines (AFP) intelligence assessments. These assessments, while primarily intended for internal use, serve as a valuable tool for avoiding the implementation of peace-building or economic development initiatives in areas with security concerns. These year-end reports categorize each barangay into a four-category scale of NPA presence, defined as: 0) Unaffected, 1) Threatened, 2) Less Influenced, and 3) Influenced. Notably, the 2012 assessment does not distinguish between "less influenced" and "influenced" or between "threatened" and "unaffected" designations. Thus, I collapse the scale into a binary variable: 1 indicates NPA presence ("less influenced" or "influenced" barangays) and 0 denotes government control ("threatened" and "unaffected")[6]. This measure captures institutions of rebel control. Unlike short-term territorial fluctuations, these rebel-controlled territories reflect a more stable organizational structure capable of sustaining military operations. The shift in territorial control is calculated as the difference between the given year and the preceding year[7]. It takes a value of 1 if there is a shift in territorial control (from rebel to government or vice versa) and 0 otherwise. Figure 1.2 illustrates that 1,438 barangays (3.4%) experienced a shift in territorial control at least once between 2012 and 2014. Figure 1.3 shows the annual number of shifts in territorial control over the study period. The number of barangays that underwent shifts in territorial control decreased from 401 in 2012 to 186 in 2013, but then increased to 851 in 2014.

[6]In the AFP intelligence assessment, "Influenced" barangays contain established political and military structures capable of planned attacks, while "less influenced" barangays have weaker organizational presence but retain some offensive capability under favorable conditions. "Threatened" barangays have minimal rebel influence, primarily engaging in extortion or early-stage mass organization (Rubin, 2020). I collapse them into two categories based on the capacity of rebels to initiate military operations.

[7]Given that the AFP intelligence assessment is a year-end report, the difference between t-1 and t reflects shifts in territorial control during year t.

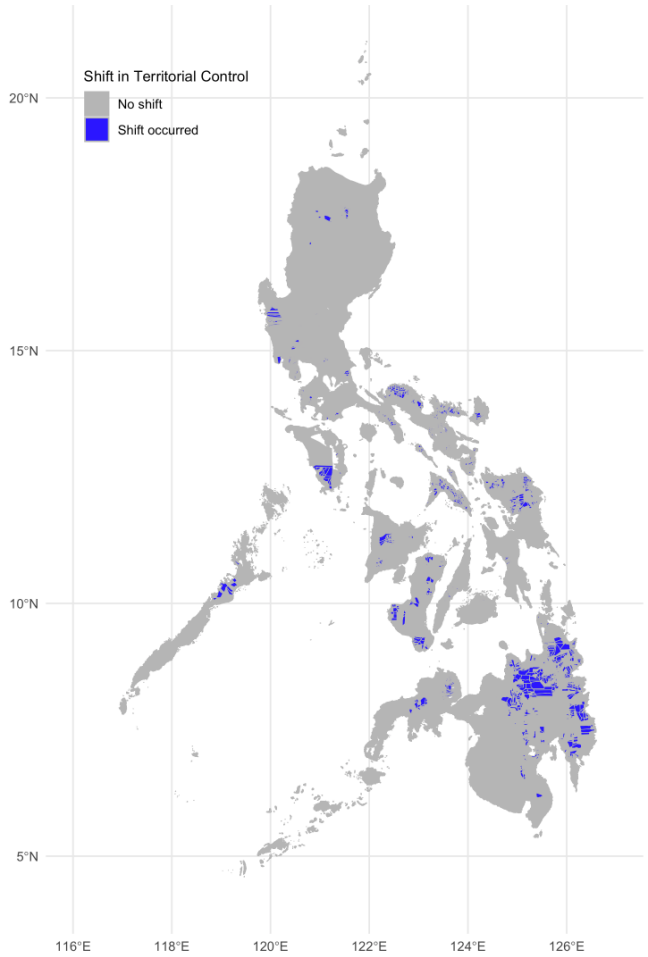

Figure 1.2: The Spatial Distribution of Shifts in Territorial Control

Note: The map illustrates the barangays that experienced at least one shift in territorial control between 2012 and 2014

The second outcome is battle-related violence. I measure it using data from the Bangsamoro Conflict Monitoring System (BCMS)[8]. The BCMS collects information from "regional and provincial police blotters", supplements it with "media reports", and subsequently validates the data through "meetings with various local stakeholders". This three-stage process enhances the reliability and granularity of the dataset. However, BCMS data exclusively covers the ARMM and the Davao region. Furthermore, the dataset includes various forms of violence, including clashes among civilians or clans, as well as those involving rebel groups and the government. For the analysis, I focus on the Davao region and the conflict between the NPA and the government. I aggregate the data at the barangay-month level and create a binary variable indicating whether battle-related violence occurred within one year following a given month. This variable is coded as 1

[8]More information on the BCMS data is available on the Conflict Alert (CA) website: https://conflictalert.info.

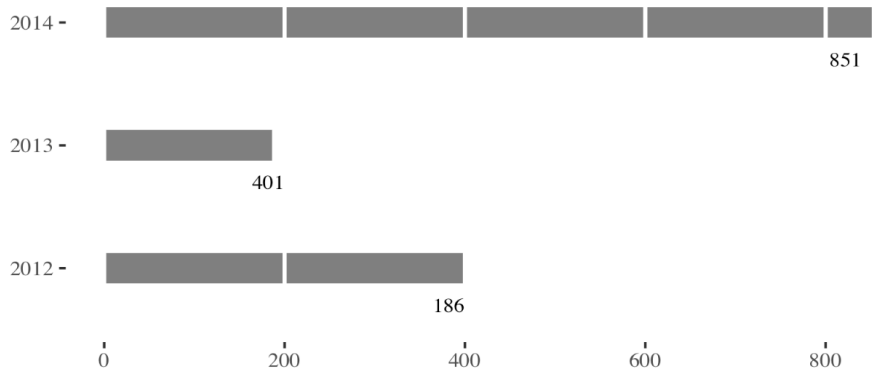

Figure 1.3: The Number of Barangays with a Shift in Territorial Control by Year, Between 2012 and 2014

if any battle-related violence occurred in the barangay during the 12-month period after that month, and 0 otherwise. Since battles often require preparation time, the one-year window is used to capture the delayed impact of natural disasters on battle-related violence. As a robustness check, I also use a count variable at the barangay-month level, which measures the number of battle-related events per barangay each month. Figure 1.4 shows the spatial distribution of battle-related violence in the Davao region, where 10.4% of barangays experienced battle-related violence at least once between 2012 and 2014. Figure 1.5 presents the annual trend in battle-related violence.

Natural Disasters as the Treatment

The main explanatory variable is the occurrence of natural disasters[9]. Given that the predominant natural disasters in the Philippines are extreme rainfall events, including landslides, floods, and storms, I classify a unit as having experienced a natural disaster when the precipitation deviation exceeds a barangay-specific threshold derived from the event-level dataset of EM-DAT[10]. Given the short study period (2012 - 2014) and the fact that disaster recovery often spans multiple years, a staggered adoption design is applied as the main specification for a better causal estimation. To avoid bias caused by lingering effects of previous disasters, I treat the period between the first and second hits of natural disasters in each barangay as a distinct treatment phase, and exclude the periods after

[9]See a more detailed explanation of the measurement of natural disasters in Appendix P.

[10]Based on EM-DAT, these rainfall-related disasters account for 87.76% of natural disasters that occurred between 2012 and 2014 in the Philippines.

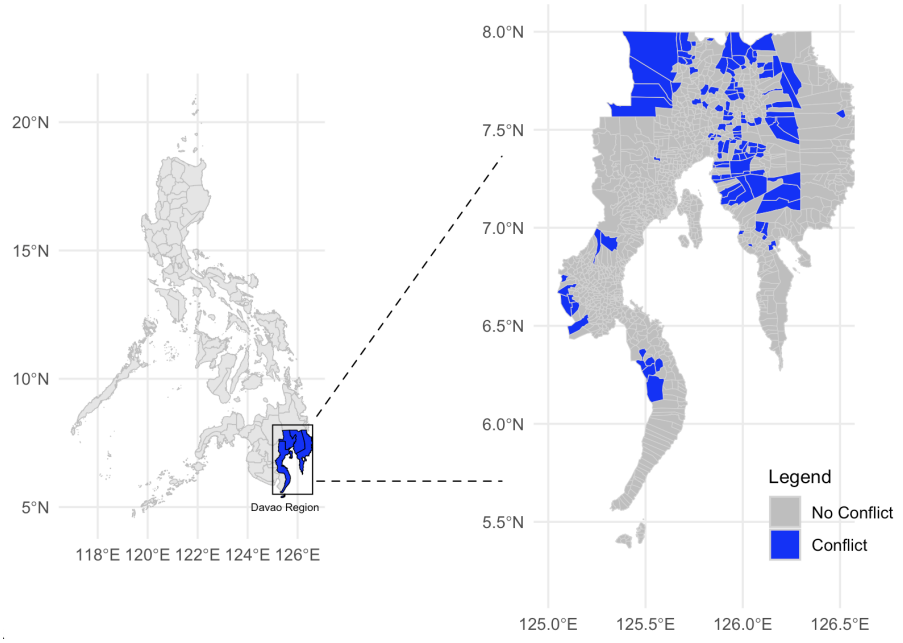

Figure 1.4: The Spatial Distribution of Battle-related Violence

Note: The map illustrates the barangays that experienced at least one conflict incident between 2012 and 2014

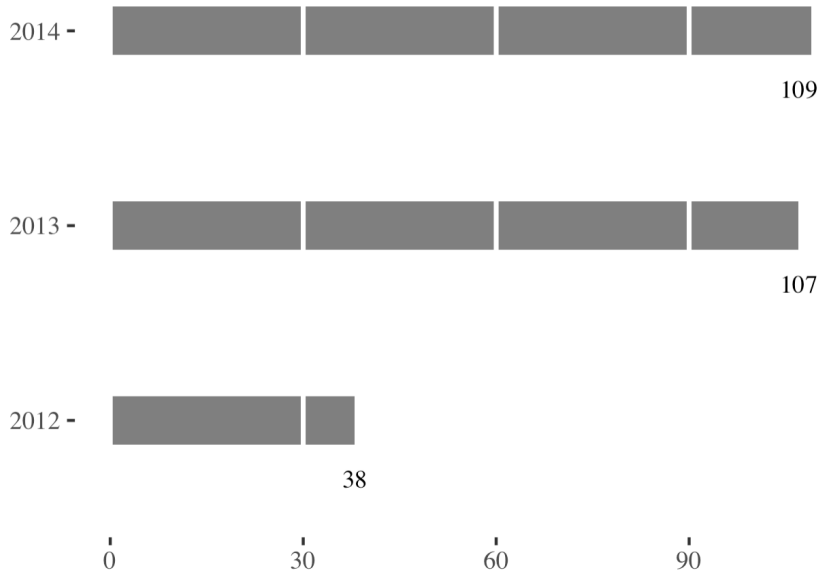

Figure 1.5: The Number of Barangays that Experienced Battle-related Violence by Year, Between 2012 and 2014

the second hit as well as affected barangays in 2010 and 2011 (Hassell & Holbein, 2024)[11]. As an alternative, I define natural disaster occurrence at the barangay-month level by assigning a value of 1 if a barangay's precipitation deviation exceeds the threshold in a given month, and 0 otherwise.

[11]The exclusion of only the years 2010 and 2011 is because incorporating additional earlier years leads to the removal of too many barangays, causing outcome variables to become constant.

This measure mitigates two concerns about EM-DAT raised above. First, the geographical precision of the precipitation data addresses EM-DAT's incomplete subnational coverage of natural disasters. Second, including precipitation - an exogenous phenomenon - mitigates the endogeneity concern in EM-DAT's disaster definition.

Precipitation deviation is based on the Integrated Multi-satellite Retrievals for GPM (IMERG) dataset, version 6.0, which provides monthly precipitation estimates aggregated at the barangay-month level[12]. The dataset offers a $0.1^\circ \times 0.1^\circ$ grid resolution (roughly 10×10 km) and spans from 2000 to the present, effectively covering the study period.

The precipitation deviation is calculated as the standardized difference between the monthly average precipitation and its 10-year moving average for each barangay-month. The formula is as follows:

$$(X_{bt} - \bar{X}_{bt})/S_{bt}$$

where X_{bt} is the precipitation for month t in barangay b, $\bar{X}_{bt}$ is the 10-year moving average precipitation for month t in barangay b, and S_{bt} is the 10-year moving standard deviation of the precipitation for month t in barangay b. The 10-year moving average is preferable because the precipitation exhibits a long-term trend over time.

To define a threshold for excessive precipitation indicative of a natural disaster, I calculate the precipitation deviation for barangays affected by rainfall-related natural disasters including storms, floods, and landslides. The classification of barangays as experiencing such disasters is based on geographical information from EM-DAT. I use a precipitation deviation value of 1.6 as the threshold for classifying a barangay as experiencing a rainfall-related natural disaster. This corresponds to the third quartile of the precipitation deviation distribution.

In Figure 1.6, I display the spatial distribution of natural disasters based on my measure. Additionally, Table 1.A16 in Appendix K presents a comparison of the annual number

[12]More information on the GPM (IMERG) dataset, version 6.0 is available: https://gpm.nasa.gov/resources/documents/imerg-v06-release-notes.

of barangays affected by natural disasters (at least once) between my measure and the EM-DAT-based measure.

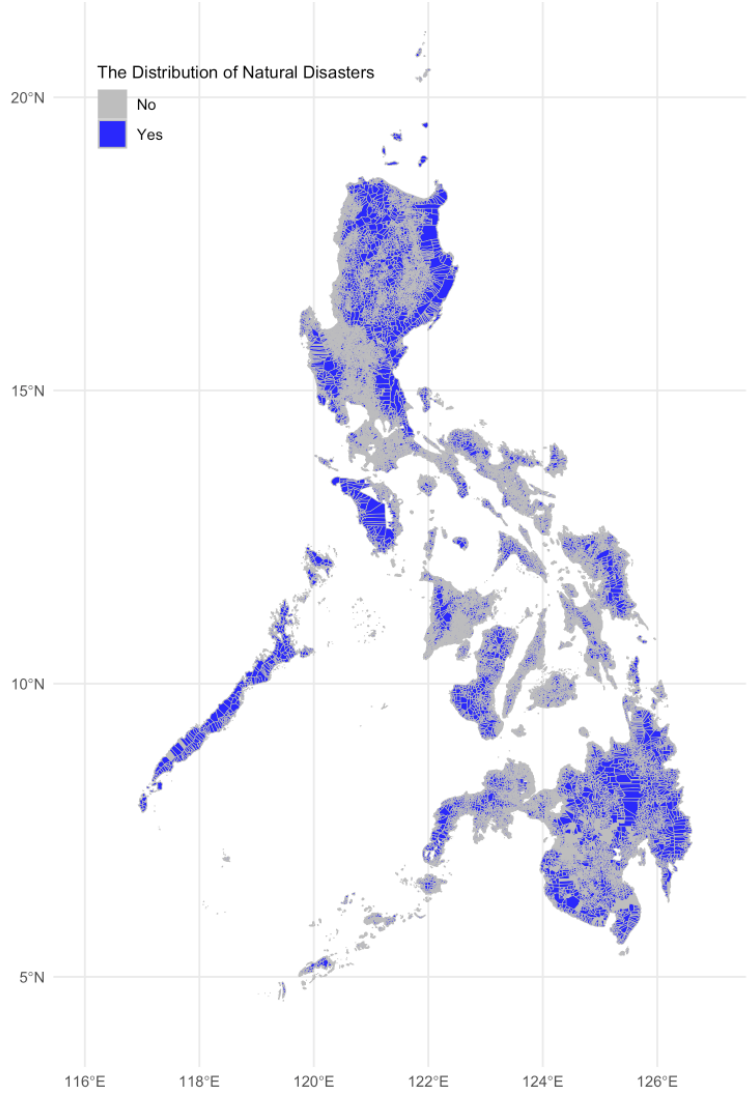

Figure 1.6: The Spatial Distribution of Natural Disasters

Note: The map illustrates the barangays that experienced at least one natural disaster between 2012 and 2014

Territorial Setting

Another explanatory variable is the territorial setting, where a setting is defined as a barangay and its immediately adjacent barangays (i.e., the site and its Moore Neighborhood). The spatial configuration of territorial control encompasses three distinct territorial settings - homogeneous, enclave, and mixed. These settings are measured using data derived from AFP intelligence assessments. The territorial setting of each barangay is determined by the status of territorial control in both the barangay itself and its neigh-

boring barangays. Table 1.1 presents the temporal distribution of these three settings. The homogeneous setting is the dominant category, increasing from 91.44% to 92.0% of barangays between 2012 and 2014. In contrast, the other two settings experienced a decrease. The mixed setting reduced to 7.69% in 2014 from 8.20% in 2012, while the enclave setting decreased from 0.35% in 2012 to 0.31% in 2014.

Table 1.1: The Distribution of Territorial Settings

	2012	2013	2014	Total
Enclave	0.35% (148)	0.29% (122)	0.31% (129)	0.32%(399)
Homogeneous	91.44% (38433)	91.83% (38595)	92% (38667)	91.76% (115695)
Mixed	8.2% (3448)	7.88% (3312)	7.69% (3233)	7.93% (9993)

Difference-in-Differences Design

To identify how territorial settings moderate the impact of natural disasters on two conflict outcomes, I exploit both the variation between disaster-affected and unaffected territories across different territorial settings, and within-unit changes in the risk of those conflict outcomes for affected territories before and after natural disasters.

I use a two-way fixed effects (TWFE) model to estimate the moderating relationship between natural disasters and conflict outcomes, accounting for common time shocks and time-invariant barangay characteristics. The main specification uses a TWFE model with a staggered adoption design, supplemented by a simple TWFE model that treats natural disasters as an occurrence at a specific time for each barangay. I first estimate the unconditional impact of a natural disaster on two conflict outcomes.

$$Y_{i,t} = \alpha_i + T_t + \beta_1 D_{i,t} + \epsilon_{i,t}$$

, where the outcome $Y_{i,t}$ represents either a shift in territorial control in barangay i during year t, or the occurrence of battle-related violence in barangay i between months $t+1$ to $t+12$. α_i denotes barangay-fixed effects, and T_t denotes year or month-fixed effects. $D_{i,t}$ is the key explanatory variable, indicating the occurrence of a natural disaster in barangay i at time t, where t is defined at the yearly or monthly level accordingly. The error term $\epsilon_{i,t}$ is clustered at the barangay and time level.

To test the hypotheses concerning the moderator of territorial settings, I include interaction terms between natural disasters and the territorial settings, as specified below.

$$Y_{i,t} = \alpha_i + \lambda_t + \beta_1 D_{i,t} + \beta_2 Mixed_{i,t} + \beta_3 Enclave_{i,t}$$
$$+ \beta_4 D_{i,t} \cdot Mixed_{i,t} + \beta_5 D_{i,t} \cdot Enclave_{i,t} + \epsilon_{i,t}$$

, where $Mixed_{i,t}$ is a binary variable equal to 1 if the barangay is located in a mixed setting, and 0 otherwise. $Enclave_{i,t}$ is equal to 1 if the barangay is located in an enclave setting, and 0 otherwise. The homogeneous setting is the omitted category.

For the TWFE model to be interpreted causally, it is important that, in the absence of natural disasters, each barangay would exhibit similar conflict dynamics - a parallel trends assumption (PTA). I conduct an event study model to examine the plausibility of parallel trends. This involves using binary treatment indicators for the occurrence of natural disasters, with leads and lags, to assess whether trends in conflict outcomes were comparable prior to the occurrence of disasters (Chiu et al., 2023; Hassell & Holbein, 2024). Results from Figure 1.A1 and Figure 1.A2 in Appendix D suggest no clear violation of the PTA[13].

Results

I begin by evaluating the unconditional effect of natural disasters on shifts in territorial control and the occurrence of battle-related violence. According to Model 2, which uses the staggered adoption design, in Table 1.2, the coefficient on natural disasters is positive and statistically significant at the 5% level. This suggests that the occurrence of natural disasters increases the risk of a shift in territorial control. The risk of a shift in territory control increases by 1.05% in barangay-months experiencing natural disasters, compared to barangay-months not experiencing natural disasters. Given the baseline risk of 0.52% - the probability of a shift in a barangay-month without a natural disaster, this represents a substantial relative increase. This result is consistent with the Model 1, which does not use the staggered adoption design.

[13]The interpretive power of the event study model for shifts in territorial control is limited by the availability of only one pre-treatment period, which constrains the ability to detect potential pre-trends.

Table 1.2: The Unconditional Impact of Natural Disasters on Two Conflict Outcomes (coefficients in percentage points)

Dependent Variables:	Control Shift (%)		Battle-related Violence (%)	
Model:	(1)	(2)	(3)	(4)
Variables				
Natural Disasters	1.10**	1.05**	-1.17*	1.55
	(0.56)	(0.45)	(0.62)	(1.16)
Staggered Adoption		✓		✓
Fixed-effects				
Barangay	✓	✓	✓	✓
Year	✓	✓		
Month			✓	✓
Fit statistics				
Observations	124,302	14,092	41,797	9,627
R^2	0.12094	0.37974	0.45066	0.45547
Within R^2	0.00259	0.00070	0.00017	0.00222

Clustered (Barangay & Year) standard-errors for (1) and (2) in parentheses
Clustered (Barangay & Month) standard-errors for (3) and (4) in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1 and 3 estimate the outcomes of shifts in territorial control and battle-related violence, using a DID model without the staggered adoption design. Model 2 and 4 estimate the same outcomes using the DID model with the staggered adoption design.

Table 1.2 also presents the results for the occurrence of battle-related violence. Model 4, which uses the staggered adoption design, indicates that the occurrence of natural disasters does not have a statistically significant effect on battle-related violence. This finding aligns with the expectation that shifts in territorial control should be treated independently and not every shift is accompanied by increased battle-related violence. However, it is worth noting that Model 3, which does not use the staggered adoption design, shows a negative and statistically significant effect on battle-related violence at the 10% level.

Main Model

Then, I evaluate the moderating role of territorial settings on the relationship between natural disasters and both shifts in territorial control and battle-related violence. Figure 1.7 (based on Model 2 with the staggered adoption design in Table 1.A1 in Appendix A) presents the results on the moderating effect of natural disasters on shifts in territorial control. As shown in Figure 1.7, the effect of natural disasters on shifts in territorial control in the enclave setting is positive and statistically significant at the 5% level. This

finding supports Hypothesis 2.1, which posits that in enclave settings, natural disasters are more likely to lead to shifts in territorial control. The risk of a shift in territorial control increases by 18.91% in disaster-affected territories within the enclave setting, compared to a 0.39% decrease in affected territories within the homogeneous setting. This result is consistent with Model 1 in Table 1.A1, which does not use the staggered adoption design. A similar pattern is also observed in the mixed setting, where natural disasters also increase the risk of a shift in territorial control. According to Figure 1.7, the effect of natural disasters in the mixed setting is positive and statistically significant at the 5% level, supporting Hypothesis 3.1, which proposes that natural disasters are more likely to lead to shifts in territorial control in the mixed setting. In affected territories within the mixed setting, the risk of a shift in territorial control increases by 3.90%. Although the estimated coefficients for the enclave and mixed settings are not statistically significantly different, this increase in the risk of a shift in territorial control is substantively larger in affected territories within the enclave setting than in the mixed setting. This supports Hypothesis 3.1, which compares the impact of natural disasters across these two settings. However, the result for the interaction term with the enclave setting should be interpreted with caution, as the statistically significant and positive effect does not hold in Model 1, which does not use the staggered adoption design, as shown in Table 1.A1.

Figure 1.7 (based on Model 2 in Table 1.A2) also presents results on the moderating effect of natural disasters on battle-related violence. As shown in the bottom panel of Figure 1.7, the effect of natural disasters in the mixed setting is positive and statistically significant at the 5% level. This finding suggests that battle-related violence is more likely to occur in disaster-affected territories within the mixed setting. The risk of battle-related violence increases by 3.93% in affected territories within the mixed setting, compared to a 0.95% increase in the homogeneous setting. This supports Hypothesis 3.2, which posits that in the mixed setting, natural disasters are more likely to lead to increased battle-related violence. However, it is worth noting that the positive and statistically significant result does not hold in Model 1, which does not use the staggered adoption design, as shown in Table 1.A2.

Consistent with Hypotheses 1.2 and 2.2, the results show no statistically significant effect

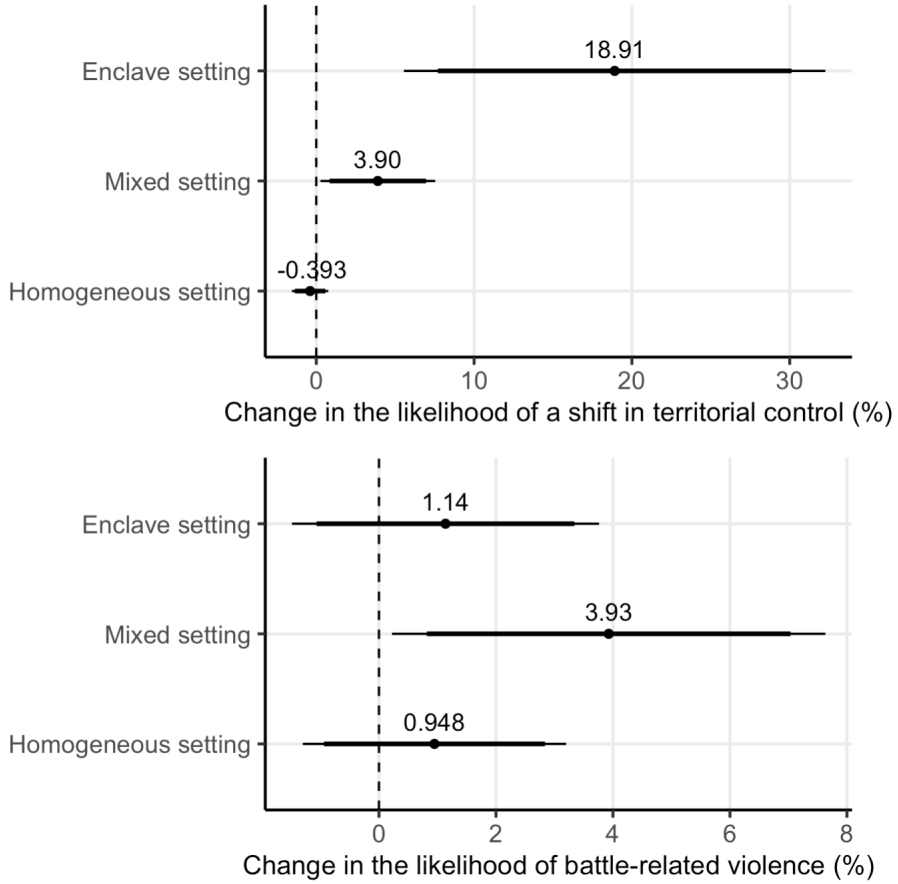

Figure 1.7: The Conditional Impact of Natural Disasters on Two Conflict Outcomes, by Territorial Setting: 90% and 95% Confidence Intervals

Note: The top panel shows the effect of natural disasters in enclave, mixed, and homogeneous settings on shifts in territorial control (based on Model 2 in Table 1.A1). The bottom panel shows the effect of natural disasters in the same settings on battle-related violence (based on Model 2 in Table 1.A2).

of natural disasters on battle-related violence in either enclave or homogeneous settings.

The Spillover Effect of Natural Disasters

Additionally, I examine the spillover effect of natural disasters. According to Model 2 in Table 1.A5 (Appendix C), the interaction term with the mixed setting is positive but not statistically significant. This finding does not support Hypothesis 4.1, which posits an increased risk of shifts in territorial control in adjacent unaffected territories within the mixed setting. Similarly, Model 2 in Table 1.A6 shows that the interaction term with the mixed setting is negative and not statistically significant, providing no support for Hypothesis 4.2, which concerns the occurrence of battle-related violence in adjacent unaffected territories.

Non-Contiguous Movement

I further examine shifts in territorial control and battle-related violence under non-contiguous movement. To assess how the moderating effect of rebel control varies across different spatial configurations, I include three-way interaction terms between natural disasters, mixed or enclave settings, and rebel control. Figure 1.8 (based on Model 2 in Table 1.A3 and Model 2 in Table 1.A4) presents these results for both shifts in territorial control (top panel) and battle-related violence (bottom panel).

As shown in the top panel of Figure 1.8, the effect of natural disasters on the risk of a shift in territorial control is more pronounced in rebel-controlled territories within the mixed and enclave settings compared to government-controlled ones. Specifically, in rebel-controlled territories affected by a disaster, the risk of a shift in territorial control increases by 46.71% in the mixed setting and by 44.9% in the enclave setting. In contrast, the corresponding increases in government-controlled territories are only 0.95% and 12.58%, respectively. These findings support Hypothesis 5.1, which posits a stronger effect of natural disasters on shifts in territorial control in rebel-controlled territories within enclave or mixed settings.

However, for battle-related violence, the bottom panel of Figure 1.8 shows a positive and statistically significant effect of natural disasters in government-controlled territories within the mixed setting, but not in the enclave setting. This provides only limited support for Hypothesis 5.2.

Robustness

The results remain robust across various robustness checks, including adjustments for the choice of the barangay as to conceptualize the territory, concerns about spatial dependence, and alternative measurements of key variables such as natural disasters, territorial settings, and conflict outcomes.

First, a critical consideration in this study is how territory is conceptualized across different levels of spatial aggregation. While the barangay, the smallest administrative unit in the

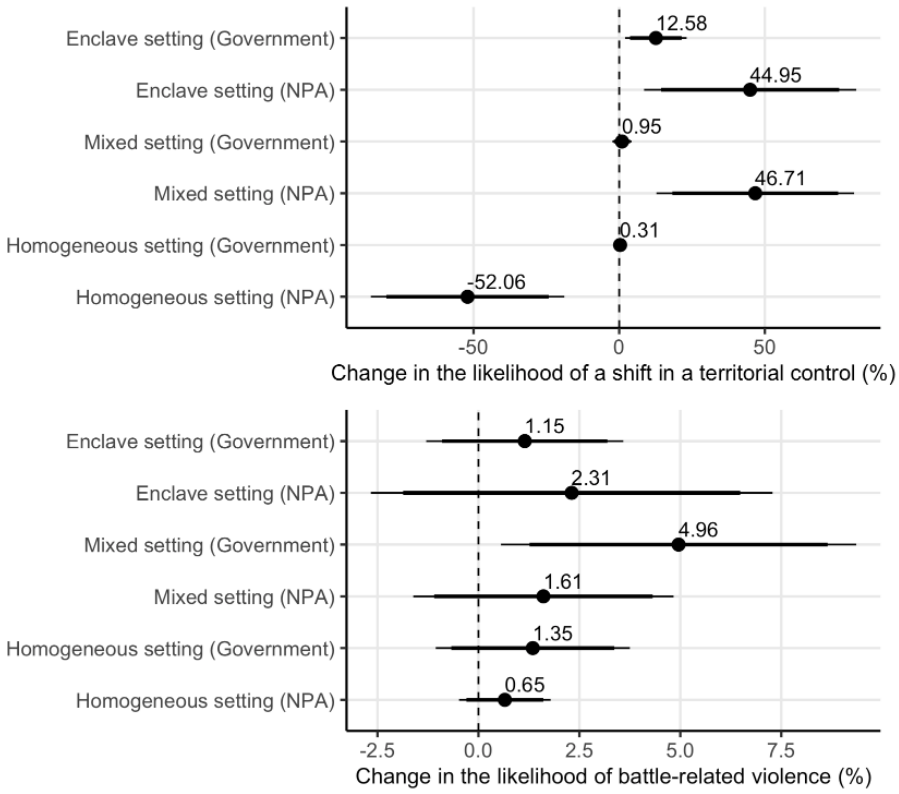

Figure 1.8: The Conditional Effect of Natural Disasters on Two Conflict Outcomes: A Three-Way Interaction Between Natural Disasters, Territorial Setting, and rebel Control

Note: The top panel shows results for shifts in territorial control (based on Model 2 in Table 1.A3), and the bottom panel presents results for battle-related violence (based on Model 2 in Table 1.A4. In both panels, from the top to bottom, the results show the effect of natural disasters in: 1) government-controlled territories within the enclave setting, 2) rebel-controlled territories within the enclave setting, 3) government-controlled territories within the mixed setting, 4) rebel-controlled territories within the mixed setting, 5) government-controlled territories within the homogeneous setting, 6) rebel-controlled territories within the homogeneous setting.

Philippines, serves as the most granular level for governance and conflict engagement, it raises questions about whether this level adequately captures broader territorial dynamics. For example, a homogeneous setting at the barangay level may appear as an enclave setting when analyzed at a higher level of aggregation. To address this concern, I conduct additional tests at the municipality level. I recalculate the municipality-level precipitation deviations and reclassify territorial control based on aggregated barangay data. The results from Appendix F confirm that the localized conflict patterns observed at the barangay level largely hold at the municipality level, particularly in the enclave setting.

Differences in the geographic size of barangays may also influence how territorial settings are classified, potentially shaping conflict dynamics. Larger barangays require a greater number of surrounding enemy-controlled territories to be classified as enclave or mixed

settings. In contrast, smaller barangays are more easily classified as enclave or mixed settings, as fewer surrounding enemy-controlled areas are required to meet the classification threshold. To address this concern, I examine the distribution of barangay sizes and find that 90% fall within a range, from -0.389 to 0.422 z-scores relative to the mean size of 7 km² (see Table 1.A10 in Appendix G). Furthermore, to account for the influence of outliers, I re-estimate the models using restricted samples that exclude the most extreme 5% and 10% of barangays by size. The results for shifts in territorial control remain robust (see Tables 1.A11 and Table 1.A12 in Appendix G).

Additionally, recognizing that regions without NPA presence may differ systematically from contested territories, I restrict the analysis to municipalities and provinces where the NPA is present. However, this sample restriction cannot be applied to the analysis of battle-related violence, as the Davao region is a hotspot for NPA activity, with NPA presence in most municipalities. The results for shifts in territorial control remain robust (see Table 1.A13 in Appendix H).

Second, another concern in this study is the spatial dependence. Variables such as rainfall-related natural disasters, territorial settings, and the two conflict outcomes may be spatially correlated. To address this, I re-estimate the main models with Conley standard errors (Conley, 1999), applying alternative distance cutoffs of 10 km, 20 km, 50 km, 100 km, and 200 km. The results, presented in Table 1.A14 and Table 1.A15 in Appendix I, confirm that the findings for shifts in territorial control remain robust. For battle-related violence, the results hold in models using Conley standard errors with cutoffs of 100 km and 200 km.

Additionally, I conduct a randomization inference (see details in Appendix J). I randomly reshuffle the values of $D_{i,t}$ across the study period. For example, $D_{i,t}$ in December 2011 may be replaced by its value from December 2012. I repeat this process 1,000 times and estimate placebo effects for each iteration. I then compare the actual estimates of the interaction terms between natural disasters and the mixed or enclave settings based on Model 2 in Table 1.A1 for shifts in territorial control and Model 2 in Table 1.A2 for battle-related violence (Appendix B) to the distribution of these placebo effects. The

estimated interaction effect between natural disasters and the mixed setting exceeds 95% of the placebo estimates ($p < 0.05$) for both shifts in territorial control and battle-related violence, while the interaction effect for the enclave setting exceeds 90% ($p < 0.1$) for shifts in territorial control and 95% ($p < 0.05$) for battle-related violence.

Third, a further concern relates to the measurement of key variables, including natural disasters, territorial settings, and conflict outcomes. To address this issue, I conducted a series of robustness checks using alternative measurements informed by both theoretical and empirical considerations.

Regarding the measurement of natural disasters, while my threshold-based approach - using monthly precipitation deviation - captures average monthly precipitation, it may overlook short-term rainfall intensity. A higher precipitation deviation, when coupled with rainfall concentrated over a short period, constitutes a more severe disaster than the same total precipitation distributed evenly throughout the month[14]. To test this, I incorporate monthly rainfall concentration into the model and assess its moderating effect on conflict outcomes. Monthly rainfall concentration is calculated using the IMERG dataset (version 6.0), which provides daily precipitation estimates. I use these daily values to calculate the monthly rainfall concentration. According to Model 2 in Table 1.A17 (Appendix L), the impact of natural disasters on shifts in territorial control in the mixed setting is more pronounced during months with higher rainfall concentration. However, this effect is not observed in the enclave setting. According to Model 2 in Table 1.A18 (Appendix L), the moderating impact of rainfall concentration is not observed for battle-related violence.

I also examine the heterogeneous effects of disaster severity by testing alternative thresholds for defining natural disasters, ranging from 1.5 to 2 precipitation deviation. Figure 1.A7 and Figure 1.A8 in Appendix N present results for both conflict outcomes. For shifts in territorial control, the effect of natural disasters in both mixed and enclave settings becomes notably stronger at the thresholds of 1.9 and 2, which suggests that more severe disasters are more likely to lead to shifts in territorial control. For battle-related violence, the impact becomes negative in both mixed and enclave settings, indicating that severe

[14]I am indebted to an anonymous reviewer for this point

disasters may reduce battle-related violence. One possible explanation is that the greater destruction caused by severe disasters forces defending forces to withdraw and thus enable shifts in territorial control without direct confrontation.

As an alternative to the threshold-based measure of natural disasters, I estimate a zero-stage model to assess the impact of natural disasters on both conflict outcomes. In the first stage, the occurrence of natural disasters based on EM-DAT geographical information is regressed on monthly precipitation deviation and rainfall concentration. In the second stage, the two conflict outcomes are regressed on the predicted values of natural disasters from the first stage (see Appendix M for more details). The results for both conflict outcomes remain robust.

For the measurement of territorial settings, while my main theory simplifies the spatial configuration of territorial control into three discrete territorial settings, the actual spatial configurations can be more complex. The proportion of neighboring territories controlled by enemy forces varies across affected territories. To account for this complexity, I further test the impact of pre-disaster spatial configurations of territorial control on shifts in territorial control by incorporating both the extensive and intensive margins of enemy-controlled neighboring territories. The extensive margin captures the presence of any enemy-controlled territory in the vicinity, while the intensive margin measures the proportion of neighboring territories under enemy control, ranging from 10% to 90% in 10% increments. As shown in Figure 1.A9 (Appendix Q), the marginal effect of enemy control on shifts in territorial control intensifies as the proportion increases, with the strongest effects observed at 60% and 90%. Beyond 60%, the effect slightly attenuates, suggesting possible saturation effects.

For the measurement of battle-related violence, instead of using a binary indicator, I also examine the total number of battle events per month to capture the intensity of contention. To address potential overdispersion in the outcome variable, I use count models - specifically, Poisson and negative binomial regressions. The results remain robust, as shown in Table 1.A25 (Appendix S).

Additionally, I test whether the moderating effect of natural disasters on battle-related violence holds under alternative temporal aggregations of battle-related violence (see Figures 1.A11 and 1.A12 in Appendix T). The results remain robust.

Conclusion

This paper presents a theoretical model and subnational quantitative analysis of the relationship between natural disasters and two conflict outcomes: shifts in territorial control and battle-related violence, introducing the spatial configuration of territorial control as a moderator. This paper delineates three distinct territorial settings: homogeneous, enclave, and mixed to capture the pre-disaster spatial configuration. This paper argues that natural disasters disrupt the power balance in affected territories. However, their impact varies across pre-disaster spatial configurations of territorial control, as these offer different opportunities for the post-disaster allocation of military personnel and equipment.

Taken together, the empirical findings lend support to several of the hypotheses. The analysis shows that after natural disasters, the risk of a shift in territorial control increases in the mixed and enclave settings, but not in the homogeneous setting. Battle-related violence increases only in the mixed setting. Additionally, the impact of natural disasters on the risk of a shift in territorial control in the mixed and enclave settings is more pronounced in rebel-controlled territories.

As with other subnational research, it is important to clarify the extent to which the findings from this paper, focused on the single case of the conflict between the NPA and the Philippine government, are applicable to broader civil war contexts (Blattman & Miguel, 2010). The dynamics observed in this paper – such as the moderating effect of spatial configurations of territorial control on post-disaster shifts in territorial control and battle-related violence – are likely relevant to other countries vulnerable to natural disasters due to climate change, where belligerents are engaged in territorial conflicts. For example, countries such as South Sudan, Myanmar, and Colombia have experienced territorial conflicts during civil wars and are projected to face increased vulnerability to climate change (IPCC, 2018). This suggests that the theory and empirical results

presented in this paper may have broader applicability.

However, the unique characteristics of the Philippines should be taken into account when generalizing these findings. Its archipelagic geography, which poses logistical challenges for both counterinsurgency and disaster response, and the decentralized structure of the NPA distinguish it from more contiguous conflict zones or those with centralized rebel groups. Future research should replicate this analysis across diverse conflict settings to assess the robustness of these findings and explore how context-specific factors may shape conflict dynamics.

Finally, these findings suggest that the growing frequency and severity of natural disasters due to climate change may further complicate conflict resolution by increasing uncertainty over territorial control. Additionally, this paper offers policy insights for conflict prevention, providing policymakers with a more nuanced understanding of how conflict dynamics can shift in the aftermath of natural disasters. The findings can help identify potential security hotspots and inform resource allocation for conflict prevention and resolution, ultimately contributing to more stable and peaceful post-disaster environments.

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Appendix

1. Main models
    - A. Main models for the moderating impact on conflict outcomes
    - B. Non-contiguous movement
    - C. Spillover effects of natural disasters
    - D. Parallel Trends Assumption - event study model
    - E. Models that include nighttime lights as a control variable
2. To address concerns about using barangays to conceptualize territorial units
    - F. Aggregation at the municipality level
    - G. Sensitivity to variations in barangay geographical size
    - H. Restricting the sample to regions with the NPA presence
3. To address concerns about spatial dependence
    - I. Using Conley standard error
    - J. Randomization inference
4. To address concerns about the measurement of natural disasters
    - K. Matching rate between my measure and EM-DAT
    - L. Accounting for rainfall concentration
    - M. Zero-stage models based on EM-DAT
    - N. Using alternative thresholds
    - O. A DID model accounting for the treatment phase
    - P. Details on the measurement of natural disasters
5. To address concerns about the measurement of the territorial setting
    - Q. Measuring spatial configuration as a continuum
6. To address concerns about the measurement of battle-related violence
    - R. Co-distribution of two conflict outcomes

S. Count models for battle-related violence

T. Exploring alternative temporal aggregations of battle-related violence

Appendix A: Models that examine the moderating impact of natural disasters

Table 1.A1: The Effect of Natural Disasters on Shifts in Territorial Control (Main Models)

Dependent Variable:	Control Shift (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	0.88	-0.39
	(0.63)	(0.59)
Mixed Setting	3.47	-9.29**
	(4.70)	(3.72)
Enclave Setting	54.62***	-6.53
	(17.23)	(10.51)
Natural Disasters × Mixed Setting	2.52**	4.29**
	(1.00)	(2.16)
Natural Disasters × Enclave Setting	-7.81	19.30***
	(11.98)	(7.12)
Staggered Adoption		✓
Fixed-effects		
Barangay	✓	✓
Year	✓	✓
Fit statistics		
Observations	124,302	14,095
R^2	0.42899	0.38662
Within R^2	0.02814	0.01179

Clustered (Barangay & Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Model 1 estimates the conditional effect of natural disasters on shifts in territorial control using a difference-in-differences (DID) model without the staggered adoption design. Model 2 uses a DID model with the staggered adoption design to estimate the same effect.

Table 1.A2: The Effect of Natural Disasters on Battle-related Violence (Main Models)

Dependent Variable:	Battle-related Violence (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	-0.80	0.95
	(0.72)	(1.15)
Mixed Setting	3.29*	1.49
	(1.83)	(2.80)
Enclave Setting	-0.90	1.99
	(2.33)	(2.21)
Natural Disasters × Mixed Setting	-1.27	2.98*
	(0.93)	(1.72)
Natural Disasters × Enclave Setting	-3.41	0.19
	(2.10)	(0.38)
Staggered Adoption		✓
Fixed-effects		
Barangay	✓	✓
Month	✓	✓
Fit statistics		
Observations	41,760	9,592
R^2	0.46673	0.48304
Within R^2	0.00137	0.01560

Clustered (Barangay & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1 estimates the conditional effect of natural disasters on battle-related violence using a difference-in-differences (DID) model without the staggered adoption design. Model 2 uses a DID model with the staggered adoption design to estimate the same effect.

Appendix B: Non-contiguous movement

Table 1.A3: The Effect of Natural Disasters on Shifts in Territorial Control, Interaction with Rebel Control and Territorial Setting

Dependent Variable:	Control Shift (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	0.52	0.31
	(0.49)	(0.19)
Rebel Control	-77.95***	34.78
	(8.68)	(29.10)
Natural Disasters × Rebel Control	-7.68**	-52.37***
	(3.74)	(17.07)
Mixed Setting		3.86
		(2.56)
Enclave Setting		17.24**
		(7.62)
Natural Disasters × Mixed Setting		0.65
		(1.68)
Mixed Setting × Rebel Control		-117.2***
		(29.11)
Natural Disasters × Enclave Setting		12.28**
		(5.58)
Rebel Control × Enclave Setting		-123.1***
		(41.89)
Natural Disasters × Mixed Setting × Rebel Control		45.75**
		(17.49)
Natural Disasters × Enclave Setting × Rebel Control		32.37
		(25.22)
Staggered Adoption	✓	✓
Fixed-effects		
Barangays	✓	✓
Year	✓	✓
Fit statistics		
Observations	14,092	14,092
R^2	0.74567	0.81355
Within R^2	0.59025	0.69961

Clustered (Barangays & Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1, using the staggered adoption design, estimates the unconditional effect of natural disasters on shifts in territorial control. Model 2, also using the staggered adoption design, estimates the conditional effect through a three-way interaction.

Table 1.A4: The Effect of Natural Disasters on Battle-related Violence, Interaction with Rebel Control and Territorial Setting

Dependent Variable:	Battle-related Violence (t+1) (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	1.97	1.35
	(1.20)	(1.23)
Rebel Control	10.12	12.69
	(9.17)	(10.64)
Natural Disasters × Rebel Control	-1.04***	-0.69
	(0.36)	(0.64)
Mixed Setting		0.31
		(1.89)
Enclave Setting		3.21
		(2.11)
Natural Disasters × Mixed Setting		3.61*
		(2.05)
Mixed Setting × Rebel Control		-1.45
		(2.50)
Natural Disasters × Enclave Setting		-0.20
		(0.16)
Rebel Control × Enclave Setting		-8.75
		(6.38)
Natural Disasters × Mixed Setting × Rebel Control		-3.35
		(2.08)
Natural Disasters × Enclave Setting × Rebel Control		1.16
		(1.71)
Staggered Adoption	✓	✓
Fixed-effects		
Barangays	✓	✓
Month	✓	✓
Fit statistics		
Observations	9,592	9,592
R^2	0.48433	0.49137
Within R^2	0.01805	0.03145

Clustered (Barangays & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1, using the staggered adoption design, estimates the unconditional effect of natural disasters on battle-related violence. Model 2, using the staggered adoption design, estimates the conditional effect through a three-way interaction.

Appendix C: Spillover effect of natural disasters

To test the potential spillover effects of natural disasters (Hypotheses 4.1 and 4.2), I estimate TWFE models that include spatial lags of the treatment, territorial settings, and their interactions. The model specification is as follows.

$$Y_{i,t} = \alpha_i + T_t + \beta_1 D_{i,t} + \beta_2 D_{i+1,t} + \epsilon_{i,t}$$

, where $Y_{i,t}$ denotes the outcome variable. Specifically, this is either shifts in territorial control in barangay i in the year t-1 or the occurrence of battle-related violence in barangay i during months t+1 to t+12. α_i represents barangay-fixed effects, and T_t denotes time-fixed effects. $D_{i,t}$ is the key explanatory variable, indicating the occurrence of natural disasters in barangay i at time t. $D_{i+1,t}$ is its spatial lag. The error term $\epsilon_{i,t}$ captures unobserved factors that may influence the outcomes.

$$\begin{aligned} Y_{i,t} = \alpha_i + \lambda_t &+ \beta_1 D_{i,t} + \beta_2 Mixed_{i,t} + \beta_3 Enclave_{i,t} + \beta_4 D_{i+1,t} \\ &+ \beta_5 Mixed_{i+1,t} + \beta_6 Enclave_{i+1,t} + \beta_7 D_{i,t} \cdot Mixed_{i,t} \\ &+ \beta_8 D_{i,t} \cdot Enclave_{i,t} + \beta_9 D_{i+1,t} \cdot Enclave_{i+1,t} \\ &+ \beta_{10} D_{i+1,t} \cdot Mixed_{i+1,t} + \epsilon_{i,t} \end{aligned}$$

, where $Mixed_{i,t}$ is a binary indicator equal to 1 if barangay is located in a mixed setting, and zero otherwise. Similarly, $Enclave_{i+1,t}$ equals to 1 if the barangay is located in an enclave setting, and zero otherwise. $Mixed_{i,t}$ and $Enclave_{i+1,t}$ equal 1 if any adjacent barangay is classified as a mixed or enclave setting.

Table 1.A5: The Spillover Effect of Natural Disasters on Shifts in Territorial Control

Dependent Variable:	Control Shift (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	1.79***	0.34
	(0.59)	(0.63)
Spatial lag(Natural Disasters)	-1.10	-1.06
	(0.84)	(0.60)
Mixed Setting (MS)		-9.18**
		(3.15)
Enclave Setting (ES)		-6.77
		(10.10)
Spatial lag(MS)		-0.64
		(0.96)
Spatial lag(ES)		4.67
		(7.41)
Natural Disasters × Mixed Setting		4.21
		(3.16)
Natural Disasters × Enclave Setting		19.07***
		(6.89)
Spatial lag(Natural Disasters) × Spatial lag(MS)		0.02
		(0.96)
Spatial lag(Natural Disasters) × Spatial lag(ES)		0.07
		(6.67)
Staggered Adoption	✓	✓
Fixed-effects		
Barangay	✓	✓
Year	✓	✓
Fit statistics		
Observations	14,095	14,095
R^2	0.37991	0.38762
Within R^2	0.00097	0.01340

Clustered (Barangay & Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1, which uses the staggered adoption design, estimates the unconditional effect of natural disasters on shifts in territorial control. Model 2, also using the staggered adoption design, estimates the conditional effect, depending on the territorial setting.

Table 1.A6: The Spillover Effect of Natural Disasters on Battle-related Violence

Dependent Variable:	Battle-related Violence (t+1) (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	2.27	1.26
	(1.46)	(1.17)
Spatial lag(Natural Disasters)	-1.38	-0.31
	(1.25)	(0.71)
Mixed Setting		2.70
		(3.31)
Enclave Setting		1.56
		(2.68)
Spatial lag(MS)		-1.63
		(1.42)
Spatial lag(ES)		-2.50
		(2.03)
Natural Disasters × Mixed Setting		4.33*
		(2.37)
Natural Disasters × Enclave Setting		0.12*
		(0.70)
Spatial lag(Natural Disasters) × Spatial lag(MS)		-0.80
		(0.54)
Spatial lag(Natural Disasters) × Spatial lag(ES)		-3.49
		(2.02)
Staggered Adoption	✓	✓
Fixed-effects		
Barangay	✓	✓
Month	✓	✓
Fit statistics		
Observations	9,627	9,592
R^2	0.47722	0.49207
Within R^2	0.00453	0.03279

Clustered (Barangay & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Model 1, which uses the staggered adoption design, estimates the unconditional effect of natural disasters on battle-related violence. Model 2, also using the staggered adoption design, estimates the conditional effect, depending on the territorial setting.

Appendix D: Event study model - parallel trends assumption

$$
\begin{aligned}
\mathrm{Y}_{i,t} = \alpha_i + \lambda_t + \beta_1 \mathrm{D}_{i,t} + \beta_2 \mathrm{Mixed}_{i,t} + \beta_3 \mathrm{Enclave}_{i,t} \\
+ \beta_4 \mathrm{D}_{i,t} \cdot \mathrm{Mixed}_{i,t} + \beta_5 \mathrm{D}_{i,t} \cdot \mathrm{Enclave}_{i,t} \\
+ \sum_{k=-L}^{-1} \gamma_k \mathrm{Lead}_k \cdot \mathrm{Mixed}_{i,t} + \sum_{k=-L}^{-1} \delta_k \mathrm{Lead}_k \cdot \mathrm{Enclave}_{i,t} \\
+ \sum_{k=1}^{L} \gamma_k \mathrm{Lag}_k \cdot \mathrm{Mixed}_{i,t} + \sum_{k=1}^{L} \delta_k \mathrm{Lag}_k \cdot \mathrm{Enclave}_{i,t} + \epsilon_{i,t}
\end{aligned}
$$

, where the outcome $Y_{i,t}$ represents shifts in territorial control in barangay i in year t and the occurrence of battle-related violence in barangay i from month $t+1$ to $t+12$. α_i denotes barangay-fixed effects, and λ_t represents year-fixed effects for shifts in territorial control and month-fixed effects for battle-related violence. $D_{i,t}$ is the treatment indicator under the staggered adoption, equal to 1 if a natural disaster occurred in barangay i at time t. The error term $\epsilon_{i,t}$ captures unobserved factors that may influence both outcomes.

For shifts in territorial control, where L=1, the specification includes one lead to capture short-term anticipation effects and one lag to examine how the impact of natural disasters may unfold over time.

For battle-related violence, where L = 6, the model includes six leads (from -6 to -1) to capture potential anticipation effects, and six lags (from +1 to +6) to explore how the impact of natural disasters may unfold over time.

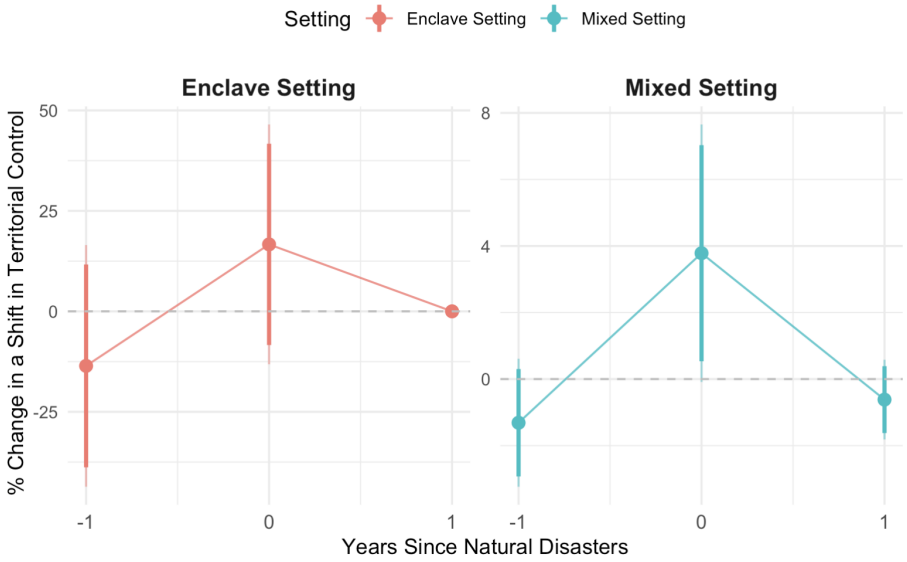

Figure 1.A1: The Visualization for Event Study Model for Shifts in Territorial Control: 90% and 95% Confidence Intervals

Note: The event study plot estimates one lead and one lag for the occurrence of natural disasters, using the year when natural disasters occur as the reference period.

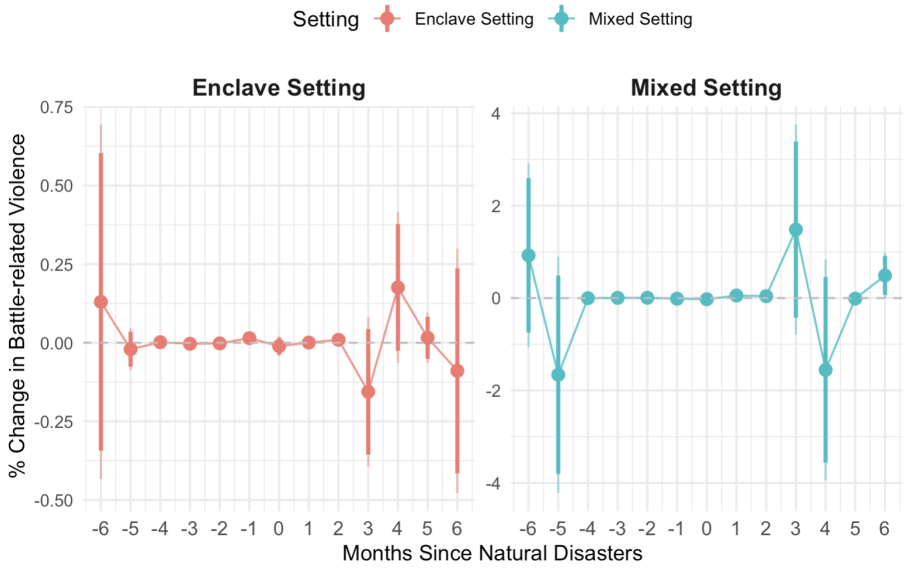

Figure 1.A2: The Visualization for Event Study Model for Battle-related Violence: 90% and 95% Confidence Intervals

Note: The event study plot estimates six leads and six lags for the occurrence of natural disasters, using the month when natural disasters occur as the reference period.

Appendix E: Models that include nighttime light as a control variable

To address concerns about time-varying confounders, such as local differences in economic development across barangays that may not be fully captured by two-way fixed effects, I incorporate nighttime light as a control variable and re-estimate the main model specifications using the staggered adoption design.

The nighttime lights data used in this analysis are derived from the VIIRS Day/Night Band (DNB) onboard the Joint Polar-orbiting Satellite System (JPSS) satellites. Specifically, I use the Annual VNL v2 dataset to construct a yearly measure for models analyzing shifts in territorial control, and the first available monthly cloud-free composite data to construct a month-level variable for models analyzing battle-related violence. These datasets are publicly available through the Earth Observation Group (https://eogdata.mines.edu/products/vnl/). The Annual VNL v2 dataset is derived from monthly cloud-free average radiance grids.

A key limitation of the dataset is that the monthly data begins only in April 2012, resulting in missing observations for January, February, and March of that year. Additionally, for annual-level analyses, the 2012 Annual VNL v2 composite is incomplete, as it is based solely on data from April to December. This may affect comparability with subsequent years.

The dataset is provided in GeoTIFF format with a spatial resolution of 15 arc seconds (approximately 500 meters at the Equator). The unit of measurement is nanowatts per square centimeter per steradian ($nW/cm^2/sr$), representing the intensity of emitted or reflected light detected at night.

The results presented in Table 1.A7 indicate that the main estimated coefficients remain consistent in both statistical significance and magnitude, further reinforcing the original hypotheses.

Table 1.A7: The Effect of Natural Disasters on Shifts in Territorial Control with Night Light as a Control Variable

Dependent Variables:	Control Shift (%)	Battle-related Violence (%)
Model:	(1)	(2)
Variables		
Natural Disasters	-0.35	0.99
	(0.56)	(1.15)
Mixed Setting	-9.25**	1.52
	(3.75)	(2.78)
Enclave Setting	-6.49	2.02
	(10.55)	(2.20)
Night Light	0.03	0.01
	(0.06)	0.02
Natural Disasters × Mixed Setting	4.24**	2.94*
	(2.21)	(1.68)
Natural Disasters × Enclave Setting	19.25***	0.15
	(7.09)	(0.40)
Staggered Adoption	✓	✓
Fixed-effects		
Barangay	✓	✓
Year	✓	
Month		✓
Fit statistics		
Observations	14,095	9,592
R^2	0.38662	0.48305
Within R^2	0.01180	0.01562

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1 estimates the conditional effect of natural disasters on shifts in territorial control, using a difference-in-differences (DID) model with the staggered adoption design. Model 2 uses the same specification to estimate the conditional effect on battle-related violence.

Appendix F: Municipality level

A critical aspect of my study is how territory is conceptualized across different levels of spatial aggregation. While the barangay represents the most granular unit of governance and conflict engagement, a question arises as to whether this level sufficiently captures broader territorial dynamics. To address this, I conduct robustness tests for shifts in territorial control at the municipality level.

In the robustness test, I define the treatment based on whether any barangay experiences a precipitation deviation above a specified threshold in a given month, assigning a value of 1 to indicate the presence of a natural disaster and zero otherwise, rather than using the staggered adoption design. This adjustment is necessary because the staggered adoption design excludes too many barangays, rendering the outcome constant. A comparable robustness test cannot be conducted for battle-related violence, as the focus on the Davao region produces constant outcomes at the municipality level.

For the aggregated sample, I recalculate municipality-level precipitation deviations and classify a municipality as affected by a natural disaster if the deviation exceeded 1.6. I also re-code territorial control and territorial settings based on aggregated barangay-level data. Specifically, a municipality is classified as NPA-controlled if more than 10% of its barangays are under NPA control. To test the sensitivity of this classification, I apply alternative thresholds of 20%, 30%, and 40%.

In an alternative specification, I use a different measure of natural disasters by aggregating barangay-level measures of natural disasters and calculating the proportion of barangays affected within each municipality. The results, presented in Table 2.A8, which uses the recalculated measure of natural disasters and Table 1.A9, which uses the aggregated measure, show consistent patterns in the enclave setting at 30% and 40% threshold. In the mixed setting, the results remain robust at the 40% threshold.

Table 1.A8: The Effect of Natural Disasters on Shifts in Territorial Control (%) - Municipality Level (Recoded)

Dependent Variable:	Control Shift (%)		
Model:	(1)	(2)	(3)
Variables			
Natural Disasters	2.14	0.53	0.34
	(1.58)	(0.59)	(0.38)
Mixed Setting	1.87	0.62	1.11
	(3.64)	(2.17)	(2.71)
Enclave Setting	12.02	10.10	23.33
	(28.56)	(15.56)	(15.19)
Natural Disasters × Mixed Setting	1.32	2.09	10.24***
	(1.68)	(2.80)	(3.31)
Natural Disasters × Enclave Setting	-1.48	36.29***	33.46***
	(9.51)	(11.16)	(10.64)
Staggered Adoption			
Fixed-effects			
Municipality	✓	✓	✓
Year	✓	✓	✓
Fit statistics			
Observations	4,872	4,872	4,872
R^2	0.40348	0.42665	0.47234
Within R^2	0.00816	0.06462	0.11432

Clustered (Municipality & Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: All three models use samples aggregated at the municipality level, and the measure of natural disasters is based on re-calculated precipitation deviation for each municipality. Model 1, which does not use the staggered adoption design, estimates the conditional effect of natural disasters on shifts in territorial control, using a 20% threshold to classify a municipality as NPA-controlled. Model 2 and 3 use alternative thresholds of 30% and 40%, respectively, to define NPA control.

Table 1.A9: The Effect of Natural Disasters on Shifts in Territorial Control - Municipality level (Aggregated)

Dependent Variable:	Control Shift		
Model:	(1)	(2)	(3)
Variables			
Natural Disasters	0.1160	0.0248	0.0219
	(0.0968)	(0.0359)	(0.0205)
Mixed Setting	0.0068	0.0060	0.0118
	(0.0360)	(0.0292)	(0.0292)
Enclave Setting	0.0964	0.1169	0.1832
	(0.3047)	(0.1698)	(0.1773)
Natural Disasters × Mixed Setting	0.1903	0.1463	0.6842***
	(0.1253)	(0.1623)	(0.1925)
Natural Disasters × Enclave Setting	0.1572	2.982***	2.792***
	(0.9634)	(1.084)	(0.8589)
Staggered Adoption			
Fixed-effects			
Municipality	✓	✓	✓
Year	✓	✓	✓
Fit statistics			
Observations	4,872	4,872	4,872
R^2	0.40407	0.42666	0.47322
Within R^2	0.00915	0.06463	0.11579

Clustered (Municipality & Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: All three models use samples aggregated at the municipality level, and the measure of natural disasters is defined as the proportion of barangays affected by natural disaster within each municipality, aggregated from barangay-level data. Model 1, which does not use the staggered adoption design, estimates the conditional effect of natural disasters on shifts in territorial control, using a 20% threshold to classify a municipality as NPA-controlled. Models 2 and 3 use alternative thresholds of 30% and 40%, respectively, to define NPA control at the municipal level.

Appendix G: Different geographical size of barangays

Table 1.A10: Distribution of the Geographical Size of Barangays in the Philippines

	Quantile	Z-Score
1	0%	-0.400
2	5%	-0.397
3	10%	-0.389
4	15%	-0.372
5	20%	-0.352
6	25%	-0.334
7	30%	-0.315
8	35%	-0.295
9	40%	-0.274
10	45%	-0.252
11	50%	-0.227
12	55%	-0.199
13	60%	-0.167
14	65%	-0.129
15	70%	-0.082
16	75%	-0.021
17	80%	0.061
18	85%	0.192
19	90%	0.422
20	95%	1.002
21	100%	47.396

Note: This table reports the z-scores corresponding to each quantile of the geographical size distribution of barangays in the Philippines. The z-score indicates the number of standard deviations from the mean size at each quantile.

Table 1.A11: The Conditional Effect of Natural Disasters on Shifts in Territorial Control, with excluding Barangays with Extreme Geographic Size

Dependent Variable:	Control Shift (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	-0.82	-0.77
	(0.56)	(0.58)
Mixed Setting	-2.71	-3.16
	(1.35)	(1.29)
Enclave Setting	1.64	-1.91
	(10.87)	(6.65)
Natural Disasters × Mixed Setting	5.54***	5.49**
	(1.54)	(2.39)
Natural Disasters × Enclave Setting	25.79**	24.16***
	(10.56)	(8.83)
Staggered Adoption	✓	✓
Fixed-effects		
Barangay	✓	✓
Year	✓	✓
Fit statistics		
Observations	13,049	13,671
R^2	0.38017	0.37787
Within R^2	0.01270	0.01092

Clustered (Barangay & Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1, using the staggered adoption design, estimates the conditional effect of natural disasters on shifts in territorial control based on a restricted sample that excludes the most extreme 5% of observations from both tails of the distribution (10% total). Model 2 applies the same approach but excludes the most extreme 2.5% from each tail (5% total).

Table 1.A12: The Conditional Effect of Natural Disasters on Battle-related Violence, with excluding Barangays with Extreme Geographic Size

Dependent Variable:	Battle-related Violence (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	1.39	1.35
	(1.45)	(1.33)
Mixed Setting	0.10	2.74
	(0.06)	(2.65)
Enclave Setting	0.13	1.81
	(0.91)	(2.09)
Natural Disasters × Mixed Setting	1.35	1.30
	(1.46)	(1.34)
Natural Disasters × Enclave Setting	0.04	-0.13
	(0.11)	(0.24)
Staggered Adoption	✓	✓
Fixed-effects		
Barangay	✓	✓
Month	✓	✓
Fit statistics		
Observations	8,699	9,093
R^2	0.33966	0.54395
Within R^2	0.00895	0.01594

Clustered (Barangay & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1, using the staggered adoption design, estimates the conditional effect of natural disasters on battle-related violence based on a restricted sample that excludes the most extreme 5% of observations from each tail of the distribution (10% total). Model 2 applies the same approach but excludes the most extreme 2.5% from each tail (5% total).

Appendix H: Restricting the sample to regions with NPA presence

The robustness test is conducted only for shifts in territorial control, not for battle-related violence, because most municipalities in the Davao region experienced some degree of battle-related violence during the study period, rendering the outcome nearly constant and unsuitable for estimation.

Table 1.A13: The Effect of Natural Disasters on Shifts in Territorial Control, with restricting to Regions with NPA Presence

Dependent Variable:	Control Shift (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	-2.95	-1.55
	(3.53)	(1.95)
Mixed Setting	-9.65*	-9.27***
	(3.29)	(3.54)
Enclave Setting	-6.65	-6.51
	(10.33)	(10.43)
Natural Disasters × Mixed Setting	5.22*	4.55*
	(2.96)	(2.41)
Natural Disasters × Enclave Setting	19.41***	19.28***
	(7.28)	(7.20)
Staggered Adoption	✓	✓
Fixed-effects		
Barangay	✓	✓
Year	✓	✓
Fit statistics		
Observations	2,789	4,605
R^2	0.38639	0.38629
Within R^2	0.01062	0.01095

Clustered (Barangay & Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

These two models estimate the conditional effect of natural disasters on shifts in territorial control, using two restricted samples. Model 1 includes only municipalities with the NPA presence, while Model 2 includes only provinces with the NPA presence.

Appendix I: Conley standard error

Table 1.A14: The Conditional Effect of Natural Disasters on Shifts in Territorial Control, Using Conley Standard Errors

Dependent Variable:	Control Shift (%)				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Natural Disasters	-0.39	-0.39	-0.39	-0.39	-0.39
	(0.43)	(0.52)	(0.56)	(0.62)	(0.64)
Mixed Setting	-9.29***	-9.29**	-9.29*	-9.29	-9.29
	(3.45)	(4.29)	(5.30)	(6.33)	(6.15)
Enclave Setting	-6.53	-6.53	-6.53	-6.53	-6.53
	(15.62)	(16.13)	(16.60)	(19.47)	(20.55)
Natural Disasters × Mixed Setting	4.29**	4.29*	4.29**	4.29***	4.29***
	(2.15)	(2.20)	(1.86)	(1.14)	(1.47)
Natural Disasters × Enclave Setting	19.30*	19.30**	19.30**	19.30**	19.30***
	(11.22)	(9.85)	(7.90)	(9.39)	(6.75)
Staggered Adoption	✓	✓	✓	✓	✓
Fixed-effects					
Barangay	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓
Fit statistics					
Observations	14,095	14,095	14,095	14,095	14,095
R^2	0.38662	0.38662	0.38662	0.38662	0.38662
Within R^2	0.01179	0.01179	0.01179	0.01179	0.01179

Clustered (Conley(10km)) standard-errors for (1) in parentheses
Clustered (Conley(20km)) standard-errors for (2) in parentheses
Clustered (Conley(50km)) standard-errors for (3) in parentheses
Clustered (Conley(100km)) standard-errors for (4) in parentheses
Clustered (Conley(200km)) standard-errors for (5) in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: All models use a difference-in-differences (DID) design with the staggered adoption design to estimate the conditional effect of natural disasters on shifts in territorial control. Conley standard errors are applied with distance cutoffs of 10, 20, 50, 100, and 200 km.

Table 1.A15: The Conditional Effect of Natural Disasters on Battle-related Violence, Using Conley Standard Errors

Dependent Variable:	Battle-related Violence (%)				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Natural Disasters	0.95	0.95*	0.95***	0.95	0.95***
	(0.75)	(0.53)	(0.21)	(0.69)	(0.02)
Mixed Setting	1.49	1.49	1.49	1.49***	1.49***
	(2.51)	(2.20)	(1.51)	(0.41)	(0.005)
Enclave Setting	1.99	1.99	1.99	1.99***	1.99***
	(2.22)	(1.75)	(2.18)	(0.38)	(0.08)
Natural Disasters × Mixed Setting	2.98	2.98	2.98	2.98***	2.98***
	(2.02)	(1.85)	(1.88)	(0.43)	(0.10)
Natural Disasters × Enclave Setting	0.19	0.19	0.19	0.19***	0.19***
	(0.27)	(0.17)	(0.24)	(0.07)	(0.01)
Staggered Adoption	✓	✓	✓	✓	✓
Fixed-effects					
Barangay	✓	✓	✓	✓	✓
Month	✓	✓	✓	✓	✓
Fit statistics					
Observations	9,592	9,592	9,592	9,592	9,592
R^2	0.48304	0.48304	0.48304	0.48304	0.48304
Within R^2	0.01560	0.01560	0.01560	0.01560	0.01560

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Conley(10km)) standard-errors for (1) in parentheses
Clustered (Conley(20km)) standard-errors for (2) in parentheses
Clustered (Conley(50km)) standard-errors for (3) in parentheses
Clustered (Conley(100km)) standard-errors for (4) in parentheses
Clustered (Conley(200km)) standard-errors for (5) in parentheses

Note: All models use a difference-in-differences (DID) design with the staggered adoption design to estimate the conditional effect of natural disasters on battle-related violence. Conley standard errors are applied using distance cutoffs of 10 km, 20 km, 50 km, 100 km, and 200 km.

Appendix J: Randomization inference

To account for potential spatial correlations in the data, I use randomization inference. Specifically, I compare the coefficients of interest, including two interaction terms in the main model for both conflict outcomes to placebo effects derived from randomized treatment assignments.

Treatment values are randomly reassigned across years to generate placebo datasets. For each coefficient of interest, 1,000 placebo estimates are generated and used to construct the reference distribution.

The original treatment effects for these interaction terms are estimated using Model 2 in Table 3.A8 (for shifts in territorial control) and Model 2 in Table 2.A14 (for battle-related violence).

Results for shifts in territorial control

The histograms below show the distributions of 1000 placebo treatment effects (β_{placebo}) for $\beta_{\text{treated:Mixed Setting}}$ and $\beta_{\text{treated:Enclave Setting}}$. The actual observed treatment effects are indicated by red dashed lines.

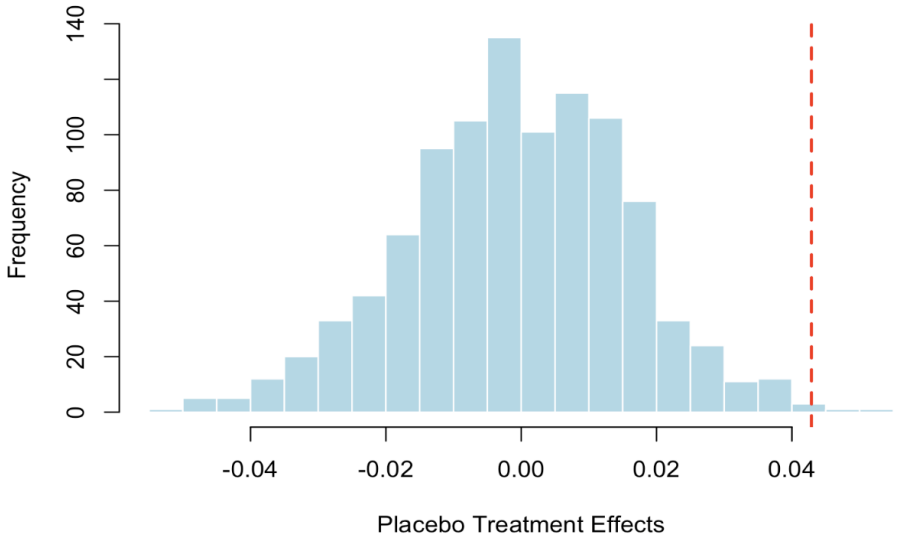

Figure 1.A3: Distribution of Placebo Effects for $\beta_{\text{treated:Mixed Setting}}$

- For $\beta_{treated:Mixed.Setting}$:

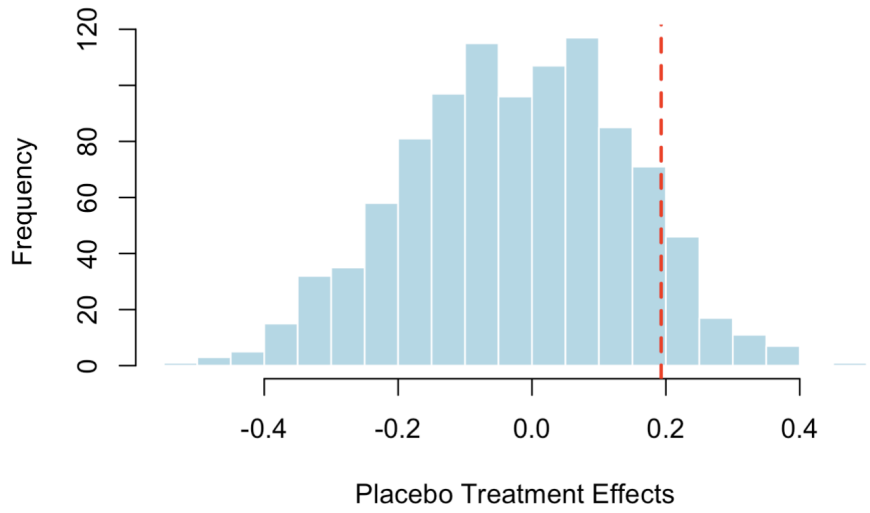

Figure 1.A4: Distribution of Placebo Effects for $\beta_{\text{treated:Enclave Setting}}$

- Original Effect: 0.043.
- P-value: 0.005.

- For $\beta_{treated:Enclave.Setting}$:
  - Original Effect: 0.193.
  - P-value: 0.095.

Results for battle-related violence

The histograms below show the distributions of 1000 placebo treatment effects (β_{placebo}) for $\beta_{\text{treated:Mixed Setting}}$ and $\beta_{\text{treated:Enclave Setting}}$. The actual observed treatment effects are indicated by red dashed lines.

- For $\beta_{treated:Mixed.Setting}$:
  - Original Effect: 0.030.
  - P-value: 0.00.

- For $\beta_{treated:Enclave.Setting}$:
  - Original Effect: 0.002.
  - P-value: 0.02.

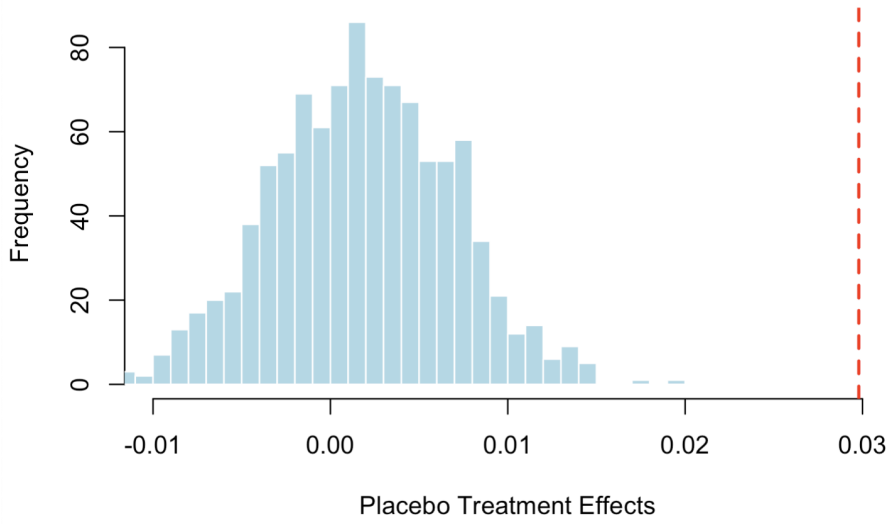

Figure 1.A5: Distribution of Placebo Effects for $\beta_{\text{treated:Mixed Setting}}$

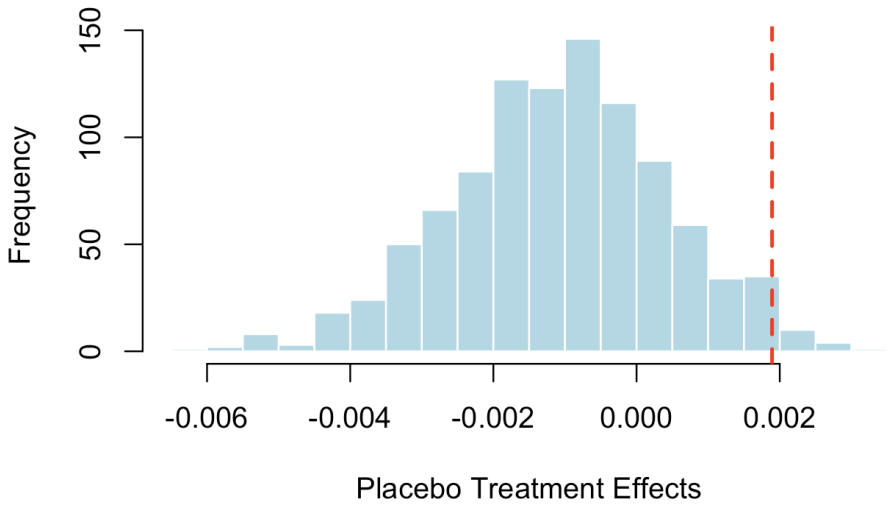

Figure 1.A6: Distribution of Placebo Effects for $\beta_{\text{treated:Enclave Setting}}$

Appendix K: Matching rate between my measure and EM-DAT

Table 1.A16: Matching Rate of Barangay-Month Units Classified as Natural Disasters using EM-DAT Measure and My Measure (z-score = 1.6) Across the Philippines

	2012	2013	2014	Total
Unmatched	26.48%	22.91%	17.88%	22.45%
Matched	73.52%	77.09%	82.12%	77.55%

Note: This table reports the matching rate of barangay-month units classified as experiencing natural disasters based on my measure and EM-DAT, for the years 2012, 2013, 2014, and the overall total. My measure defines a natural disaster as a month in which precipitation deviates from the mean by a z-score of 1.6 or higher.

Appendix L: Accounting for rainfall concentration

Since monthly rainfall concentration is measured at the month level, I disaggregate the dataset on shifts in territorial control to the monthly level. The outcome variable - shifts in territorial control - remains constant across each year (2012–2014), while the treatment variable varies monthly. To address potential bias introduced by the yearly constant outcome, I use barangay and year-fixed effects and cluster standard errors at the barangay and year levels.

To measure rainfall concentration, I use the Rainfall Concentration Index (RCI), which quantifies the distribution of rainfall within a given month. A high RCI value indicates that most rainfall occurred on only a few days, reflecting risks associated with short but intense rainfall events. The RCI is calculated as:

$$RCI = \frac{\sum_{i=1}^{n} P_i^2}{P_{\text{total}}^2}$$

where:

- P_i = Daily precipitation on day i.
- n = Number of days in the month, and.
- P_{total} = Total precipitation in the month:

$$P_{\text{total}} = \sum_{i=1}^{n} P_i$$

For each outcome, I first estimate a baseline model using the staggered adoption design to assess the conditional effect of natural disasters on conflict outcomes. I then estimate an additional model that includes three-way interaction terms between treatment, territorial settings, and rainfall concentration to evaluate how rainfall concentration moderates the effect of natural disasters.

Table 1.A17: The Effect of Natural Disasters on Shifts in Territorial Control, Interacting with Rainfall Concentration

Dependent Variable:	Control Shift (%)	
Model:	(1)	(2)
Variables		
Mixed Setting	0.07	-0.44
	(1.25)	(0.35)
Enclave Setting	18.88	13.31*
	(7.87)	(8.04)
Natural Disasters	-0.62	-0.91
	(0.64)	(0.72)
Natural Disasters × Mixed Setting	5.08**	3.58*
	(2.55)	(1.81)
Natural Disasters × Enclave Setting	24.58**	25.60*
	(12.20)	(14.23)
Rainfall Concentration Index (RCI)		-1.39
		(0.88)
Natural Disasters × RCI		0.97
		(0.66)
RCI × Mixed Setting		2.81
		(6.59)
RCI × Enclave Setting		39.29*
		(23.39)
Natural Disasters × Mixed Setting × RCI		13.99*
		(8.36)
Natural Disasters × Enclave Setting × RCI		1.54
		(24.27)
Staggered Adoption	✓	✓
Fixed-effects		
Barangay	✓	✓
Year	✓	✓
Fit statistics		
Observations	88,967	88,870
R^2	0.40337	0.40727
Within R^2	0.02115	0.02750

Clustered (Barangay & Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1 estimates the conditional effect of natural disasters on shifts in territorial control using a DID design with the staggered adoption design. Model 2 also uses the staggered adoption design but includes an interaction term to account for monthly rainfall concentration.

Table 1.A18: The Effect of Natural Disasters on Battle-related Violence, Interacting with Rainfall Concentration

Dependent Variable:	Battle-related Violence (%)	
Model:	(1)	(2)
Variables		
Natural Disasters	0.95	1.20
	(1.15)	(1.73)
Mixed Setting	1.49	1.05
	(2.80)	(2.84)
Enclave Setting	1.99	1.89
	(2.21)	(2.12)
Natural Disasters × Mixed Setting	2.98*	3.44*
	(1.72)	(1.81)
Natural Disasters × Enclave Setting	0.19	0.52
	(0.38)	(0.78)
RCI		2.04
		(3.07)
Natural Disasters × RCI		-2.14
		(4.54)
Mixed Setting × RCI		3.98
		(2.65)
Enclave Setting × RCI		1.00
		(1.88)
Natural Disasters × Mixed Setting × RCI		-4.32
		(6.52)
Natural Disasters × Enclave Setting × RCI		-3.43
		(3.81)
Staggered Adoption	✓	✓
Fixed-effects		
Barangay	✓	✓
Month	✓	✓
Fit statistics		
Observations	9,592	9,592
R^2	0.48304	0.48316
Within R^2	0.01560	0.01582

Clustered (Barangay & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1 estimates the conditional effect of natural disasters on battle-related violence using a DID design with the staggered adoption design. Model 2 also uses the staggered adoption design but includes an interaction term to account for monthly rainfall concentration.

Appendix M: A zero-stage model based on EM-DAT

To address potential bias introduced by the choice of the threshold for identifying natural disasters, I estimate a zero-stage model. The first stage regresses the variable of natural disasters based on EM-DAT on the rainfall deviation. The second stage regresses the conflict outcomes on the predicted values of natural disasters from the first stage. Because both the disaster occurrence (in the first stage) and the conflict outcomes (in the second stage) are binary variables, I also estimate both stages using logit models as a robustness check.

Below are the model specifications:

$$\begin{aligned}\text{EM DAT}_{i,t} = \alpha_i + \lambda_t + \beta_1 \text{Rainfall Deviation}_{i,t} + \beta_2 \text{RCI}_{i,t} \\ + \beta_4 I(\text{Rainfall Deviation}_{i,t} > 0.7) \\ + \beta_5 I(\text{Rainfall Deviation}_{i,t} > 1.6) + \epsilon_{i,t},\end{aligned}$$

, where $\text{EM DAT}_{i,t}$ is a binary variable indicating whether a natural disaster occurred in barangay i at time t, based on EM-DAT. $\text{Rainfall Deviation}_{i,t}$ is the z-score of monthly precipitation deviation. $\text{RCI}_{i,t}$ is the rainfall concentration index. $I(\text{Rainfall Deviation}_{i,t} > 0.7)$ is a binary indicator for rainfall deviation thresholds (0.7). $\text{Rainfall Deviation}_{i,t} > 1.6$ is a binary indicator for rainfall deviation thresholds (1.6). α_i denotes barangay fixed effects. λ_t represents month fixed effects (e.g. January, February,..., December).

The fitted values from this model are used to calculate $\text{Predicted.Disaster}_{i,t}$.

$$\begin{aligned}\text{Y}_{i,t} = \gamma_i + \eta_t + \beta_1 \text{Predicted Disaster}_{i,t} \\ + \beta_2 \text{Mixed Setting}_{i,t} + \beta_3 \text{Enclave Setting}_{i,t} \\ + \beta_4 \text{Predicted Disaster}_{i,t} \cdot \text{Mixed Setting}_{i,t} + \\ + \beta_5 \text{Predicted Disaster}_{i,t} \cdot \text{Enclave Setting}_{i,t} + \epsilon_{i,t},\end{aligned}$$

, where $\text{Y}_{i,t}$ represents either (a) the shift in territorial control in barangay i at time t, or (b) the occurrence of battle-related violence in barangay i from months $t+1$ to $t+12$. $\text{Predicted Disaster}_{i,t}$ is the fitted value from the first-stage model. $\text{Mixed Setting}_{i,t}$

is a binary variable equal to 1 if the barangay is in a mixed setting, and zero otherwise. Enclave Setting$_{i,t}$ is a binary variable equal to 1 if the barangay is in an enclave setting. γ_i represents the barangay fixed effects. η_t denotes year fixed effects for shifts in territorial control and month fixed effects for battle-related violence. $\epsilon_{i,t}$ represents the error term.

Table 1.A19: The Zero-Stage Model with Linear Probability Model for Shifts in Territorial Control

Dependent Variables:	EM-DAT (%)	Control Shift (%)
Model:	(1)	(2)
Variables		
Precipitation Deviation(PD)	6.43**	
	(2.73)	
Rainfall Concentration Index	11.20	
	(14.27)	
I(PD>0.7)	5.54	
	(4.98)	
I(PD>1.6)	13.46	
	(9.12)	
Predicted Disaster		0.25
		(0.24)
Mixed Setting		4.81
		(4.09)
Enclave Setting		46.24***
		(9.71)
Predicted Disaster × Mixed Setting		1.80***
		(0.66)
Predicted Disaster × Enclave Setting		14.68**
		(5.55)
Fixed-effects		
Barangay	✓	✓
Month	✓	
Year		✓
Fit statistics		
Observations	2,068,929	1,532,404
R^2	0.23212	0.43272
Within R^2	0.07743	0.02653

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Barangay & Month standard-errors for (1) in parentheses
Clustered (Barangay & Year standard-errors for (2) in parentheses

Note: Model 1 is the first-stage regression, estimating the occurrence of natural disasters (based on EM-DAT) using a linear model. Model 2 is the second-stage regression, estimating conflict outcomes using the predicted disaster values from Model 1, also with a linear specification.

Table 1.A20: The Zero-Stage Model with Logit Model for Shifts in Territorial Control

Dependent Variables:	EM-DAT	Control Shift (%)
Model:	(1)	(2)
	Logit	OLS
Variables		
Precipitation Deviation(PD)	0.4414**	
	(0.2270)	
RCI	0.8761	
	(1.248)	
I(PD>0.7)	0.3075	
	(0.3186)	
I(PD>1.6)	0.6341	
	(0.5370)	
Predicted Disaster Dummy		−0.00373
		(0.11)
Mixed Setting		5.02
		(4.03)
Enclave Setting		47.59***
		(8.87)
Predicted Disaster Dummy × Mixed Setting		1.57***
		(0.54)
Predicted Disaster Dummy × Enclave Setting		13.43***
		(2.22)
Fixed-effects		
Barangay	✓	✓
Month	✓	
Year		✓
Fit statistics		
Observations	2,068,929	1,532,404
Squared Correlation	0.23710	0.43310
Pseudo R^2	0.23287	-0.34816
BIC	2,368,708.4	-2,777,667.8

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Barangay & Month standard-errors for (1) in parentheses
Clustered (Barangay & Year standard-errors for (2) in parentheses

Note: Model 1 is the first-stage regression, estimating the occurrence of natural disasters (based on EM-DAT) using a logit model. Model 2 is the second-stage regression, estimating shifts in territorial control using the predicted disaster values from Model 1, also with a logit specification.

Table 1.A21: The Zero-Stage Model with Linear Probability Model for Battle-related Violence

Dependent Variables:	EM-DAT (%)	Battle-related Violence (%)
Model:	(1)	(2)
Variables		
Precipitation Deviation(PD)	8.48*	
	(5.17)	
RCI	124.7	
	(90.37)	
I(PD>0.7)	10.64	
	(7.94)	
I(PD>1.6)	-8.36	
	(12.22)	
Predicted Disaster		-0.19
		(1.99)
Mixed Setting		3.16*
		(1.84)
Enclave Setting		-1.07
		(2.43)
Predicted Disaster × Mixed Setting		0.32
		(2.06)
Predicted Disaster × Enclave Setting		0.15
		(4.55)
Fixed-effects		
Barangay	✓	✓
Single Month	✓	
Month		✓
Fit statistics		
Observations	55,728	41,760
R^2	0.33469	0.46660
Within R^2	0.11196	0.00113

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Barangay & Month standard-errors for (1) in parentheses
Clustered (Barangay & Month standard-errors for (2) in parentheses

Model 1 is the first-stage regression, estimating the occurrence of natural disasters (based on EM-DAT) using a linear model. Model 2 is the second-stage regression, estimating battle-related violence using the predicted disaster values from Model 1, also with a linear specification.

Table 1.A22: The Zero-Stage Model with Logit Model for Battle-related Violence

Dependent Variables:	EM-DAT	Battle-related Violence (%)
Model:	(1)	(2)
	Logit	OLS
Variables		
Precipitation Deviation(PD)	1.123**	
	(0.6331)	
RCI	12.82	
	(8.487)	
I(PD>0.7)	0.4205	
	(0.7859)	
I(PD>1.6)	-1.100	
	(0.7695)	
Predicted Disaster Dummy		-1.16*
		(0.56)
Mixed Setting		3.02
		(1.80)
Enclave Setting		-1.94
		(2.75)
Predicted Disaster Dummy × Mixed Setting		0.56
		(1.42)
Predicted Disaster Dummy × Enclave Setting		2.65
		(3.21)
Fixed-effects		
Barangay	✓	✓
Single Month	✓	
Month		✓
Fit statistics		
Observations	46,440	34,800
Squared Correlation	0.36605	0.46541
Pseudo R^2	0.33924	-1.7002
BIC	47,875.1	-22,126.0

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Barangay & Month standard-errors for (1) in parentheses
Clustered (Barangay & Month standard-errors for (2) in parentheses

Note: Model 1 is the first-stage regression, estimating the occurrence of natural disasters (based on EM-DAT) using a logit model. Model 2 is the second-stage regression, estimating battle-related violence using the predicted disaster values from Model 1, also with a logit specification.

Appendix N: Different thresholds for defining natural disasters

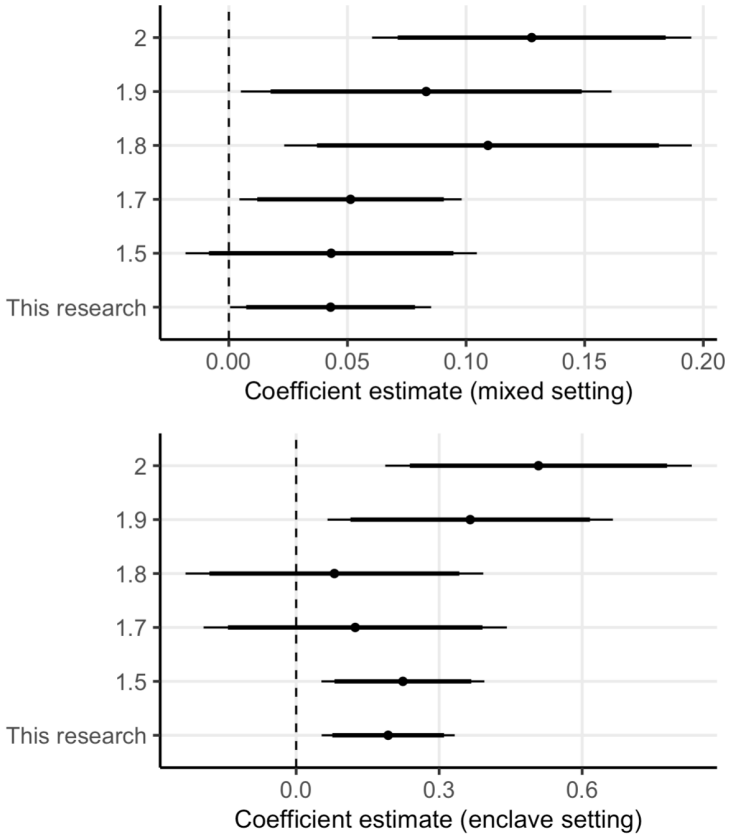

Figure 1.A7: The Conditional Impact of Natural Disasters on Shifts in Territorial Control: 90% and 95% Confidence Intervals (Different Thresholds for Natural Disasters)

Note: This top panel displays the estimated coefficients and confidence intervals for the interaction term between natural disasters and mixed settings, across models using different thresholds to define natural disasters. These thresholds include the one used in the main analysis (1.6 z-score - this research), as well as alternative z-scores: 1.5, 1.7, 1.8, 1.9, and 2.0. The bottom panel presents the corresponding results for the interaction term between natural disasters and enclave settings. Estimates are based on the main model specification using the staggered adoption design.

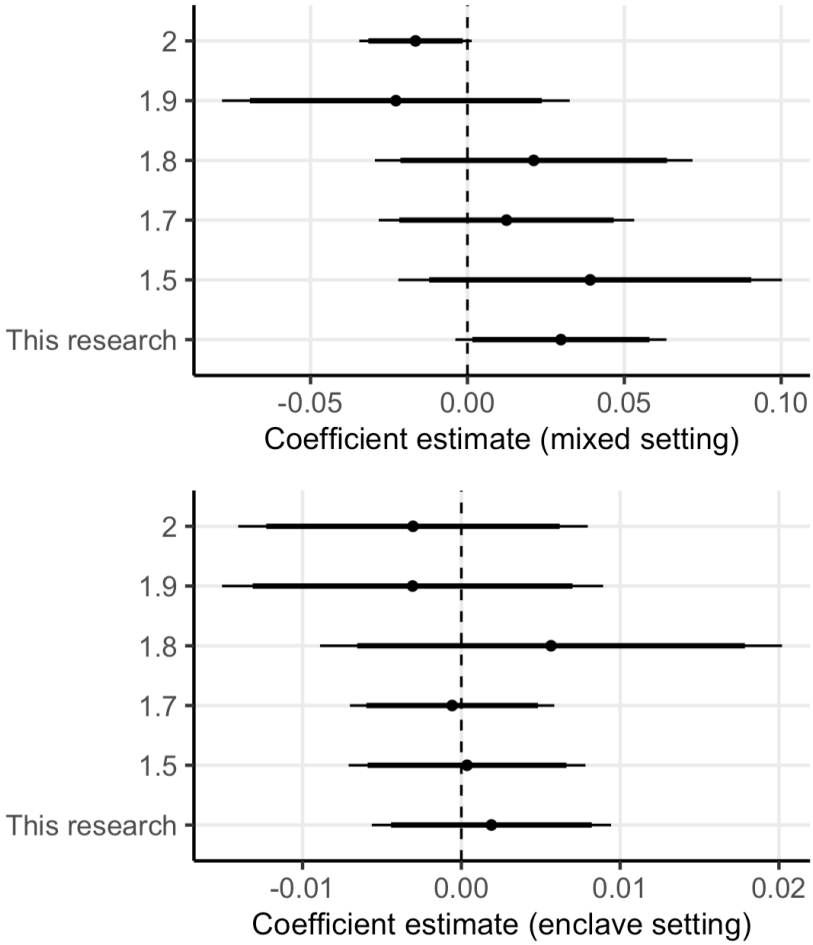

Figure 1.A8: The Conditional Impact of Natural Disasters on Battle-related Violence: 90% and 95% Confidence Intervals (Different Thresholds for Natural Disasters)

Note: The top panel displays the estimated coefficients and confidence intervals for the interaction term between natural disasters and mixed settings, across models using different thresholds to define natural disasters. These thresholds include the one used in this study (1.6 z-scores - this research), as well as alternative z-scores: 1.5, 1.7, 1.8, 1.9, and 2.0. The bottom panel presents the corresponding estimates for the interaction term between natural disasters and enclave settings. All results are based on the main model specification using the staggered adoption design.

Appendix O: The DID model that considers the treatment phases

I implement an alternative staggered adoption design in which the time after a second natural disaster is treated as a distinct phase of treatment.

The implementation follows three key steps:

(1) Once a unit is treated, it remains treated for the remainder of the observation period.

(3) The period following the second disaster is treated as a separate treatment phase.

(4) Barangays that experienced natural disasters in 2010 or 2011 are excluded from the analysis.

The model specification is as follows:

$$\begin{aligned}\text{Control Shift}_{it} = \beta_0 &+ \beta_1 \cdot \mathbb{1}(\text{Phase 1})_t + \beta_2 \cdot \mathbb{1}(\text{Phase 2})_t \\ &+ \gamma_1 \cdot \text{Mixed Setting}_i + \gamma_2 \cdot \text{Enclave Setting}_i \\ &+ \delta_1 \cdot \mathbb{1}(\text{Phase 1})_t \times \text{Mixed Setting}_i \\ &+ \delta_2 \cdot \mathbb{1}(\text{Phase 2})_t \times \text{Mixed Setting}_i \\ &+ \theta_1 \cdot \mathbb{1}(\text{Phase 1})_t \times \text{Enclave Setting}_i \\ &+ \theta_2 \cdot \mathbb{1}(\text{Phase 2})_t \times \text{Enclave Setting}_i \\ &+ \alpha_i + \lambda_t + \varepsilon_{it}\end{aligned}$$

, where $\text{Control Shift}_{i,t}$ denotes shifts in territorial control for barangay i at year t. $\mathbb{1}(\text{Phase 1})_t$ and $\mathbb{1}(\text{Phase 2})_t$ are binary indicators for treatment periods after the first and second disasters, respectively.

Mixed Setting_i and Enclave Setting_i are dummy variables equal to 1 if a barangay is located in the mixed or enclave settings, respectively, and zero otherwise. α_i captures barangay fixed effects, while λ_t denotes year fixed effects.

The results in Table 1.A22 indicate that the estimated effects of Phase 1 disasters on shifts in territorial control in both mixed and enclave settings are positive but not statistically significant. Due to the multicollinearity, interaction terms for Phase 2 treatment and both territorial settings were removed.

Table 1.A23: The Effect of Natural Disasters on Shifts in Territorial Control, Using the DID Model that Considers the Treatment Phases

Dependent Variable:	Control Shift (%)
Model:	(1)
Variables	
Treatment Phase = 1	-0.69
	(0.56)
Treatment Phase = 2	-1.92**
	(0.84)
Mixed Setting	-3.31
	(6.08)
Enclave Setting	15.63
	(31.93)
Mixed Setting × Treatment Phase = 0	-2.96
	(4.19)
Mixed Setting × Treatment Phase = 1	2.72
	(2.74)
Enclave Setting × Treatment Phase = 0	-14.66
	(28.10)
Enclave Setting × Treatment Phase = 1	7.80
	(27.90)
Fixed-effects	
Barangay	Yes
Year	Yes
Fit statistics	
Observations	14,787
R^2	0.42631
Within R^2	0.01688

Clustered (Barangays & year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1 estimates the conditional effect of natural disasters on shifts in territorial control using a difference-in-differences (DID) model that distinguishes between different phases of treatment.

Appendix P: Details on the measurement of natural disasters

Extreme rainfall events, such as landslides, floods, and storms, are the most common types of natural disasters in the Philippines. To capture these events, I construct a z-score that measures excessive rainfall. This value is calculated as the deviation of monthly precipitation in each barangay from a 10-year moving average, as shown in the equation below. To determine a threshold beyond which precipitation deviation is considered indicative of a rainfall-related natural disaster, I examine the distribution of z-scores for barangay-month units that experienced natural disasters, as recorded in EM-DAT. Based on this distribution, I select a z-score threshold for classifying an event as a disaster.

The following section first introduces the precipitation and disaster datasets used in this study. I then describe how these datasets are used to construct the z-score. Finally, I present and justify the threshold selected for identifying natural disasters.

Precipitation Data

Precipitation is measured using data from the Integrated Multi-satellite Retrievals for GPM (IMERG), the unified U.S. algorithm developed by the GPM team at NASA (Huffman et al., 2019). I use the "Final" satellite-gauge product, which is recommended for academic research due to its methodological rigor. It combines both forward and backward morphing techniques and incorporates monthly gauge analyses approximately 3.5 months after the observation period. The IMERG Final dataset provides high spatial resolution, estimating monthly mean precipitations at a grid of 0.1 X 0.1° (roughly 10x10 km). I use data covering the Philippines from 2006 to 2020, allowing for the construction of a 10-year moving average of monthly precipitation. Barangay-level precipitation is derived by overlaying administrative boundaries on the IMERG grid and calculating a weighted mean of precipitation values for each barangay. The data were accessed and downloaded via Giovanni, a web application developed by NASA's Goddard Earth Sciences Data and Information Services Center (GES DISC).

EM-DAT Dataset

To measure the occurrence of natural disasters, I use the event-level data from the Emergency Events Database (EM-DAT) collected by the Center for Research on the Epidemiology of Disasters (CRED). To be a disaster, an event meets at least one of the following criteria: 1) 10 or more causalities; 2) 100 or more people affected; 3) a declaration of a state of emergency; 4) or a call for international assistance. This dataset includes the start and end dates (down to the specific day), along with geographical information on the affected areas. For this study, I focus on rainfall-related natural disasters - specifically, storms, floods, and landslides, occurring in the Philippines between 2012 and 2014. A total of 43 events are identified. Using this data, I construct a spatial file indicating the geographic areas and periods during which natural disasters occurred.

The steps of calculating z-score

To construct the standardized precipitation deviation variable – the z-score, I calculate deviations from the 10-year moving average for each barangay and divide them by the corresponding 10-year moving standard deviation.

This is done using raster files of monthly precipitation values (2012–2014) for each grid cell in the Philippines, along with a shapefile containing the Philippine Standard Geographic Code (PSGC) administrative boundaries. Using the "exactextractr" package in R, I compute the mean precipitation within each barangay by taking the average of precipitation values from grid cells that intersect with that barangay. Second, I compute the 10-year moving average and standard deviation for each month using the "zoo" package in R. These moving statistics are calculated individually for each barangay and each calendar month (e.g., all Januaries from 2007–2019, all Februaries from 2007–2019, etc.). Finally, the z-score is calculated as:

$$(X_{bt} - \bar{X}_{bt})/S_{bt}$$

, where X_{bt} is the observed precipitation for that month and barangay, $\bar{X}_{bt}$ is the 10-year moving average for that month and barangay, and S_{tz} is the 10 years moving standard deviation for the same.

Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
-2.4	-0.1	-0.7	0.7	1.6	2.9

Table 1.A24: Summary Statistics for the Distribution of Z-Scores

The choice for z-score

The threshold for identifying extreme rainfall as a natural disaster is based on the distribution of z-scores in barangay-months affected by natural disasters, as reported in EM-DAT. Table 1.A24 shows the summary statistics for this distribution.

The z-score of 1.6, corresponding to the third quartile of this distribution, is selected as the primary threshold. I do not use the mean value because it is relatively low (0.7) and may not adequately reflect the extremity of rainfall associated with disaster events. Using the third quartile ensures that the classification of a natural disaster corresponds to exceptional levels of precipitation within the empirical distribution. To test the robustness of this classification, I also apply alternative thresholds of 1.5, 1.7, 1.8, and 2.0. This allows for evaluating the sensitivity of the disaster definition across a range of stricter or more inclusive cutoff points.

Appendix Q: Spatial configuration as a continuum

Instead of categorizing the spatial configuration of territorial control into three discrete territorial settings, I incorporate both the extensive and intensive margins of enemy-controlled neighboring territories. This approach differentiates between the mere presence of any enemy-controlled territory (extensive margin) and the degree of enemy control in surrounding areas (intensive margin). The following model is estimated:

$$\begin{aligned}\text{Conflict Outcomes} = \alpha + \beta_1 \cdot \text{Treated} + \beta_2 \cdot \text{Enemy\_around} \\ + \beta_3 \cdot \text{Enemy\_X} + \beta_4 \cdot \text{Treated} \cdot \text{Enemy\_around} \\ + \beta_5 \cdot \text{Treated} \cdot \text{Enemy\_X} + \epsilon\end{aligned}$$

, where Enemy_around is a binary indicator equal to 1 if the focal territory is adjacent to any enemy-controlled territory (extensive margin), while Enemy_X represents the proportion of neighboring territories under enemy control, ranging from 10% to 90% in 10% increments (intensive margin). β_5 captures how treatment effects vary with the intensity of enemy control in adjacent areas. Treated $\cdot$ Enemy_X is treated as a three-way interaction, reflecting the nested structure of the indicators. Territories with X% enemy control are, by definition, surrounded by at least one enemy-controlled territory.

This modeling strategy is not applied to battle-related violence due to sample restrictions in the Davao region, which result in insufficient comparison groups for estimating effects at each intensity level.

Results

The results, summarized in Figure 1.A9, show a relationship between the proportion of enemy-controlled surrounding territories and shifts in territorial control. The coefficient on Enemy_around captures the baseline effect of being adjacent to any enemy territory. The coefficients on Enemy_X and its interactions with treatment show that the marginal effect of enemy control increases as the proportion increases, with the strongest effects observed at 60% and 90% thresholds. Beyond 60%, the impact slightly attenuates, suggesting potential saturation effects.

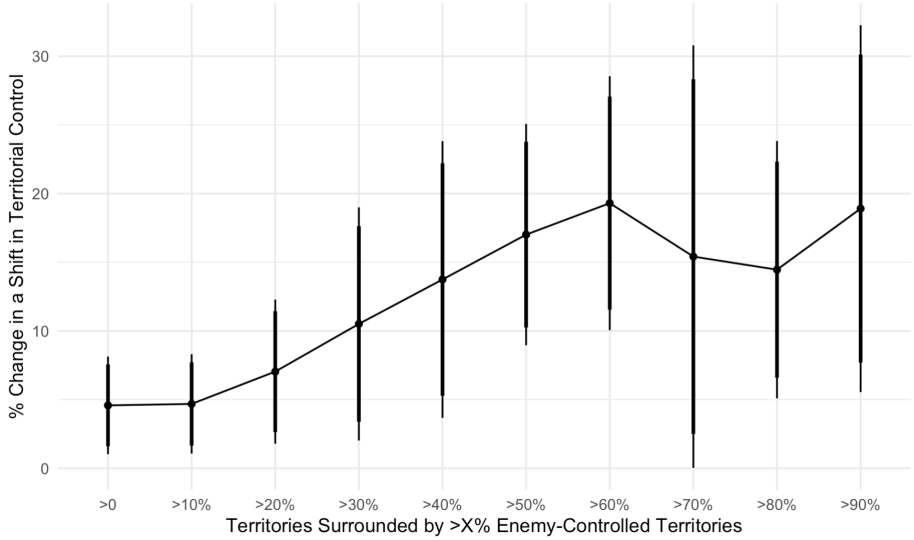

Figure 1.A9: Estimated Percentage Change in a Shift in Territorial Control by Intensity of Surrounding Enemy Control

Appendix R: The co-distribution of shifts in territorial control and battle-related violence in the Davao region

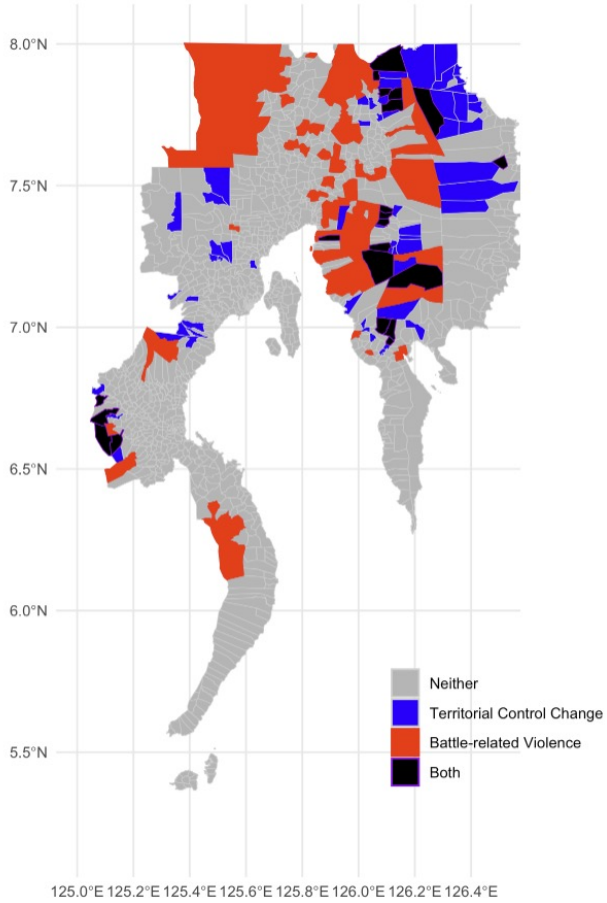

Figure 1.A10: The Co-Distribution of Shifts in Territorial Control and Battle-related Violence in the Davao Region

Note: Note: This map illustrates the distribution of barangays that experienced shifts in territorial control, battle-related violence, or both, from 2012 to 2014. Grey indicates barangays that experienced neither outcomes. Blue indicates barangays that experienced shifts in territorial control only. Red indicates barangays that experienced battle-related violence only. Black indicates barangays that experienced both.

Appendix S: Count models for battle-related violence

Table 1.A25: Count Models for Battle-related Violence

Dependent Variable:	Battle-related Violence (count)	
Model:	(1) Poisson	(2) Neg. Bin.
Variables		
Natural Disasters	-12.74***	-17.15***
	(0.1243)	(0.5565)
Mixed Setting	0.8916	0.8811
	(0.6089)	(0.5986)
Enclave Setting	-13.57***	-18.29***
	(0.2995)	(0.8784)
Natural Disasters × Mixed Setting	12.33***	16.72***
	(0.4169)	(0.8211)
Natural Disasters × Enclave Setting	1.058***	-0.1930
	(0.1602)	(1.113)
Staggered Adoption		
Fixed-effects		
Barangay	✓	✓
Month	✓	✓
Fit statistics		
Observations	4,080	4,080
Squared Correlation	0.09792	0.09006
Pseudo R^2	0.13477	0.10924
BIC	3,005.5	2,986.5
Over-dispersion		0.71294

Clustered (Barangay & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Model 1 estimates the conditional effect of natural disasters on battle-related violence using a Poisson regression model. Model 2 estimates the same effect using a negative binomial regression model to account for overdispersion.

Appendix T: Exploring alternative temporal aggregations of battle-related violence

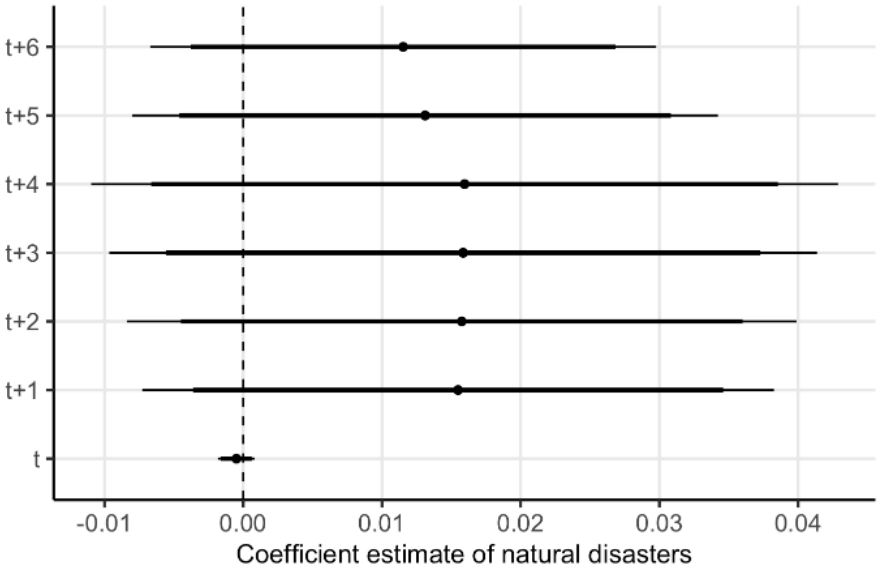

Figure 1.A11: The Impact of Natural Disasters on Battle-related Violence: 90% and 95% Confidence Intervals (Different Temporal Aggregations for Outcome)

Note: This figure presents the estimated coefficients for natural disasters and their confident interval (90% and 95%) across different model specifications using the staggered adoption deign. The models vary the time lag for the dependent variable, which is a binary indicator for the occurrence of battle-related violence. Specifically, the dependent variables represent: the occurrence of battle-related violence in the same month as the natural disaster (t), the occurrence of battle-related violence from t+1 to t+12 (t+1), and separately for t+2, t+3, t+4, t+5, and t+6.

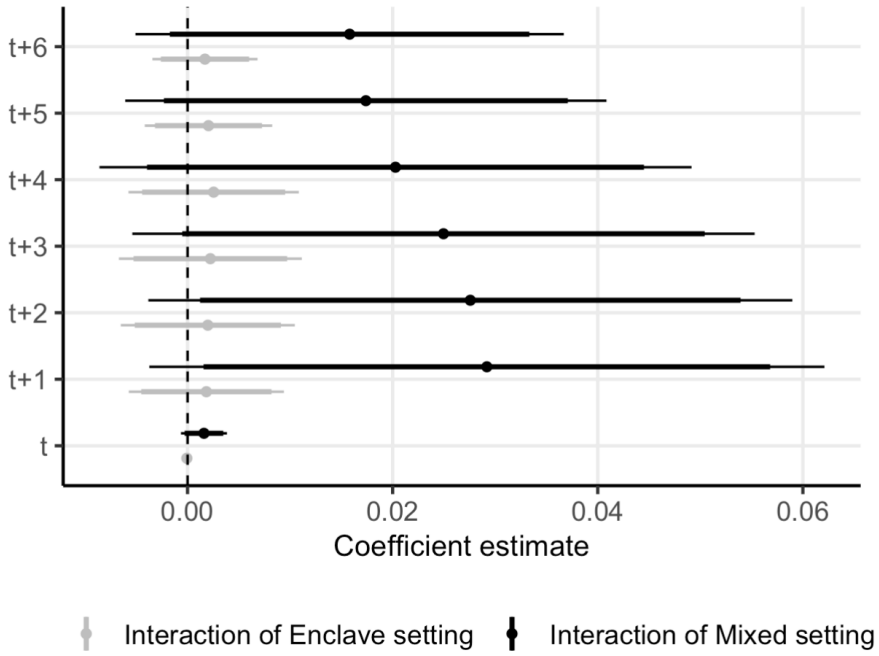

Figure 1.A12: The Conditional Impact of Natural Disasters on Battle-related Violence, Depending on the Territorial Setting: 90% and 95% Confidence Intervals (Different Temporal Aggregations of Battle-related Violence)

Note: This figure presents the estimated coefficients and 90% and 95% confidence intervals for two interaction terms: between natural disasters and mixed setting, and between natural disasters and enclave setting across different model specifications using the staggered adoption design. The dependent variable is a binary indicator for the occurrence of battle-related violence, measured at different time lags: the occurrence of battle-related violence in the same month as natural disasters (t), the occurrence of battle-related violence from t+1 to t+12 (t+1), and separately for t+2, t+3, t+4, t+5, and t+6. The black lines represent estimates and confidence intervals for the interaction with the mixed setting, while the grey lines represent those for the enclave setting.

Chapter 2

Buying Stability After Sudden and Negative Shocks: How Extractive Companies Dampen Post-Disaster Conflict

Abstract

Why do sudden and negative shocks during civil wars sometimes fuel, but at other times dampen conflict? The existing literature emphasizes how such shocks shift the balance of power among armed actors and sometimes civilians, but largely overlooks the role of third-party commercial actors. This paper examines disaster shocks and extractive companies. I argue that the impact of disaster shocks depends on the presence of companies operating capital-intensive extraction sites. In territories with such sites, disasters disrupt production and armed actors' revenues. Motivated to restore operations and revenue quickly, insured and credit-backed companies use their financial leverage to buy post-disaster stability. In contrast, where such companies are absent, this financial buffer is missing, and civil conflict risk increases. Using subnational, geo-referenced data from Myanmar (2010–2018), I show that disasters increase civil conflict in territories with labor-intensive or no extraction sites, but have no effect where capital-intensive extraction occurs. This mitigating effect fades as extraction licenses approach expiration.

Keywords: disaster shocks, resource extraction sites, civil war

Introduction

In June 2024, days of monsoon rain sent whole hillsides sliding over the hand-dug rare earth pits of the Pang War in northern Myanmar. Ore trucks stopped, revenues for armed actors evaporated, and the Kachin Independence Army (KIA) and the military junta traded

fire to seize control of stranded convoys (Kachin News Group, 2024; RFA Burmese, 2024; Zan, 2024). Barely nine months later, a magnitude 7.7 earthquake struck the Sagaing Region, destroying bridges and cutting power to the Chinese-backed Letpadaung copper complex. Within forty-eight hours, both the military junta and the National Unity Government opposition declared overlapping 20-day ceasefires to allow foreign engineers and relief convoys to reach the stricken mines (Han et al., 2025; Reuters, 2025).

These two episodes pose a puzzle: why do some disaster shocks ignite fresh violence, while others temporarily dampen it? Existing research on the disaster-conflict nexus presents mixed findings. Some scholars argue that disasters increase violence (Eastin, 2016; Kikuta, 2019), while others find no effect or even a pacifying one (Bergholt & Lujala, 2012; Slettebak, 2012). Resolving this debate becomes increasingly urgent. As climate change intensifies the frequency and severity of extreme weather events, such disaster shocks will increasingly intersect with civil conflicts in regions such as Southeast Asia, the Middle East, and Africa (IPCC, 2018).

These empirical controversies may stem from broader incomplete theoretical considerations in the conflict literature. Seminal theoretical work focuses mostly on how sudden and negative shocks impact the strategic interaction between rebels and governments (Fearon, 1995; Powell, 2006) or among multiple armed actors (Cunningham, 2013; Cunningham et al., 2009), with later work incorporating civilians (Condra & Shapiro, 2012; Kalyvas, 2006). Building on these frameworks, the disaster–conflict literature typically frames disasters as exogenous shocks that affect the balance of power between armed actors, thus inviting opportunistic attacks by challengers. When civilians are included, they are often treated as potential recruits motivated by post-shock grievances or economic desperation (Eastin, 2016; Ide et al., 2020; Nel & Righarts, 2008; Zhao, 2025).

What these frameworks share is their limited attention to third-party commercial actors. In contrast, the literature on the political economy of resources and foreign investment highlights the role of commercial companies operating capital-intensive sites. These studies show how ownership structures and foreign involvement can shape violence, but they generally associate resource extraction with greater instability or no change in armed

conflict (Blair et al., 2021; Brazys et al., 2023; Christensen, 2019; Wegenast & Schneider, 2017).

My research reconsiders the role of third-party commercial actors in shaping conflict dynamics after sudden and negative shocks that shift the balance of power among armed actors. I argue that in areas where companies hold long-term investments and a financial interest, they have strong incentives to restore economic activity following a sudden and negative shock. With access to credit and insurance, such companies can use their financial means to buy stability during post-shock recovery and operations. In this way, commercial actors can shape the post-shock war bargaining and act as stabilizing forces in conflict-affected environments.

Specifically, I focus on natural disasters as sudden and negative shocks that affect the balance of power between armed actors and resource extraction companies as third-party commercial actors. I distinguish between capital- and labor-intensive extraction sites. Capital-intensive extraction requires substantial fixed investments, heavy mechanization, and immobile infrastructure, and is thus typically operated by formal extractive companies with access to financial resources such as credit or insurance, for example, the Myanmar's Letpadaung copper mine or the Magway onshore oil fields. These companies can be international, domestic, or joint ventures. What matters is their financial capacity and long-term commitment to maintain capital-intensive operations. In contrast, labor-intensive sites rely primarily on manual extraction with minimal equipment and are operated by small operators or artisanal miners, such as the hand-dug ruby pits in Mogok located in the Mandalay Region.

In territories with capital-intensive extraction sites, armed actors typically fund themselves through strategic extortion, demanding "security fees" that depend on daily output. When production stops, so does their cash flow. Disaster shocks temporarily halt production and disrupt extortion activities, creating a shock to armed actors' capacity to sustain military operations. This gives their opponents an incentive to attack and take advantage of the disruption. Such violence is not in the interest of extractive companies, whose priority is to restore revenue flows quickly. Unlike cash-strapped armed actors hit by a natural disaster,

companies can draw on insurance payouts and credit lines, putting them in a favorable position to buy stability by paying off both sides. In contrast, in territories with no or labor-intensive resource extraction sites, where such companies are generally absent, no such buffer exists, and disasters lead to opportunistic behavior by belligerents, resulting in an increased conflict risk, similar to disaster-hit regions without extraction sites. Finally, in territories with extraction sites whose licenses are nearing expiration, companies have weaker incentives to invest in stability, and the buffer effect diminishes.

I test the theory using a staggered adoption Difference-in-Differences (DiD) design in the case of Myanmar between 2010 and 2018. Myanmar provides an ideal setting for studying the impact of sudden and negative shocks that affect the balance of power between armed actors, as frequent disaster shocks intersect with an ongoing civil war. The country is also richly endowed with natural resources extracted under different modes of production: highly labor-intensive sites such as artisanal mines, and capital-intensive sites such as large-scale mining and oil and gas operations. Revenues from both types of extraction have historically financed armed conflicts (Global Witness, 2021; Patrick, 2024). This DiD framework compares changes in violence before and after a disaster between townships exposed to disasters with different types of extraction sites, including capital-intensive, labor-intensive, or none, to the contemporaneous change in violence in unaffected townships, thus identifying whether capital-intensive companies moderate the impact of sudden and negative shocks on violence.

I find that disaster shocks increase civil conflict in territories with no resource extraction sites or with labor-intensive extraction sites, but have no discernible effect - and may even slightly reduce civil conflict in territories with capital-intensive extraction sites. This mitigating effect weakens as licenses for capital-intensive extraction sites approach expiration. These results support the argument that extractive companies help dampen post-disaster civil conflict. To rule out the possibility that the decline reflects armed actors reallocating forces to defend high-value territories, I test for spillovers in adjacent territories and find none. Another alternative is that the foreign ownership of capital-intensive sites might deter attacks, as armed actors may fear retaliation from the companies' home states rather than companies directly buying peace. However, interacting disaster exposure with

an indicator for international ownership involvement yields no evidence to support this mechanism.

This paper advances debates in both conflict studies and the international political economy (IPE) literature on extractive industries. First, this paper extends conflict literature by bringing commercial actors into the study of wartime bargaining under sudden and negative shocks. Existing seminal theoretical work focuses on how such shocks shift the balance of power between armed actors, and sometimes civilians (Condra & Shapiro, 2012; Cunningham, 2013; Cunningham et al., 2009; Fearon, 1995; Kalyvas, 2006; Powell, 2006), but overlooks the role of third-party commercial actors. By introducing third-party commercial actors into this framework and using disasters as sudden and negative shocks that affect the balance of power between armed actors, this paper offers a more complete account of how such shocks influence post-shock war bargaining. It also helps explain why disaster shocks may have heterogeneous effects on conflict.

Second, this paper contributes to the literature on resources and civil war. Most previous empirical research on resources and violence finds that resource wealth fuels conflicts (Berman et al., 2017; Blair et al., 2021; Collier & Hoeffler, 2004; Rigterink, 2020), while a few studies identify a null effect (Christensen, 2019). In contrast, I show that the presence of capital-intensive extractive companies can mitigate rather than exacerbate violence after sudden and negative shocks, a finding that echoes theoretical arguments emphasizing production methods or ownership structures as moderators of the resource curse (Andersen & Ross, 2014; Christensen, 2019; Luong & Weinthal, 2006).

Finally, this paper advances the IPE literature on extractive industries. Prior studies emphasize how foreign investment can heighten contestation by producing information asymmetries, generating grievances among affected populations, and prompting governments to securitize investment sites through coercive protection (Brazys et al., 2023; Christensen, 2019; Kishi et al., 2017; Wegenast & Schneider, 2017). Despite these insights, existing work largely conceptualizes violence as emerging from either company–civilian or company–state dynamics. In contrast, this paper extends the analysis to how companies affect war-time bargaining between armed actors. In doing so, it bridges the IPE liter-

ature's focus on extractive industries with the conflict literature's emphasis on wartime bargaining.

Literature Review

The existing literature on the disaster-conflict nexus presents mixed empirical findings, with some studies indicating that natural disasters exacerbate conflict (Kikuta, 2019; Nel & Righarts, 2008), while others show a mitigating or no effect (Bergholt & Lujala, 2012; Omelicheva, 2011; Petrova, 2022; Slettebak, 2012). Those inconsistencies suggest the need for further exploration of moderators that influence this relationship (Ide et al., 2020; von Uexkull & Buhaug, 2021). These divergent findings may also be due to incomplete theoretical considerations in conflict literature, which emphasizes the interaction among armed actors, while leaving the role of third-party commercial actors underexplored.

Seminal conflict theories conceptualize war as a bargaining process between armed actors whose incentives depend on information, commitment, and shifting power (Fearon, 1995; Powell, 2006). Extensions to multi-armed actors and civilian-inclusive models emphasize how alliances, fragmentation, and civilian collaboration shape bargaining and violence patterns (Condra & Shapiro, 2012; Cunningham, 2013; Cunningham et al., 2009; Kalyvas, 2006). While these frameworks highlight strategic adaptation after sudden and negative shocks that shift the balance of power between armed actors, they largely neglect commercial actors whose presence may reshape post-shock wartime bargaining.

Building on these foundations, theoretical frameworks on the disaster-conflict nexus often focus on strategic interactions between governments and rebel groups. The "opportunity" mechanism, for example, argues that disaster shocks may shift power balance in a way that may favor either the state or insurgents. Eastin (2016) suggests that natural disasters weaken government capacity, which creates an opportunity for rebel groups to intensify violence. Conversely, natural disasters can also disrupt rebel logistics, and give governments the upper hand in conflict situations (Kreutz, 2012; Tominaga & Lee, 2021; Walch, 2018). Similarly, the "disaster diplomacy" literature proposes that disasters often catalyze diplomatic engagement or cooperation between warring parties (Kelman, 2012).

Another strand of theory examines civilian roles in post-disaster conflicts through frameworks such as “grievances", “opportunity cost", and the sociological theory of disaster response. While these frameworks account for the role of civilians, they often still view them as potential recruits or future rebels in the civil war. The opportunity cost mechanism suggests that disasters may increase the likelihood of civilians being recruited by rebel groups due to economic disruption, agricultural decline, or livelihood losses (Brancati, 2007; Miguel et al., 2004; Nel & Righarts, 2008). The grievance mechanism, on the other hand, posits that dissatisfaction with government disaster responses can fuel rebellion (Hendrix, 2013; Homer-Dixon, 1999). Additionally, some scholars frame civilians as potential adversaries either against the government or against each other in the aftermath of disasters. While the grievance theory emphasizes the social strain caused by post-disaster resource shortages and displacement (Homer-Dixon, 1999), sociological literature suggests that disasters may foster unity and reduce preexisting tensions through the formation of a community of sufferers, potentially deterring violence (Fritz, 1996; Kang & Skidmore, 2018; Slettebak, 2012).

Those existing theoretical frameworks analyze post-disaster conflict dynamics within the context of an interaction between government and rebel groups or civilians, with little attention to how the third-party commercial actors affect the post-shock bargaining.

The role of resource extraction has been extensively studied in both the civil war and international political economy literature. Early "resource curse" studies argue that resources fuel conflict by offering rents to fight over (Berman et al., 2017; Collier & Hoeffler, 2004; Lujala et al., 2005; Rigterink, 2020). However, more recent literature suggests that conflict dynamics in resource-rich territories are more complex. They depend not only on the type of resource but also on the strategic interactions among armed actors, as well as the interests of other stakeholders present in these territories.

Rather than treating resources as a single category as in early research (Collier & Hoeffler, 2004), scholars now argue that different types of resources have different impacts on civil war (Vesco et al., 2020). Various distinctions have been proposed, such as point versus diffuse resources (Lujala et al., 2005), renewable versus non-renewable (Koubi et al., 2014),

or primary versus secondary diamonds (Rigterink, 2020). However, all these hinge on differences in extraction type: whether the resource is capital- or labor-intensive, which in turn shapes how armed actors generate revenue. For example, while armed groups engaged in smuggling often exploit diffuse, and labor-intensive resources such as opium or timber, capital-intensive resources are more conducive to extortion (Tominaga & Lee, 2021).

Except for linking conflict outcomes to resource types, other scholars emphasize the strategic interactions between armed actors and other stakeholders. While resources are indeed lucrative prizes that attract competition, armed groups controlling them also weigh the risks of rival attacks and may allocate greater military resources to protect valuable assets (Blair et al., 2024; Sánchez De La Sierra, 2020). For example, Sánchez De La Sierra (2020) shows that armed groups in the Democratic Republic of Congo provided protective services at coltan mines, assuming "state-like" roles to better extract rents. Similarly, Blair et al. (2024) argue that armed actors may deliberately target less valuable assets, such as oil pipelines, in order to avoid confrontation over high-value production sites that are heavily defended. These findings indicate that armed groups may engage in selective targeting and protection strategies based on the resource's relative value and defensive status, and then reshape strategic calculations around resource-rich territories.

Additionally, the IPE literature brings the ownership structures of resource-extracting companies into analyses, rather than resources themselves. Early literature suggests that the presence of extractive industries tends to exacerbate social conflicts, as governments and companies are prompted to securitize investment sites through coercive protection, while local communities experience land dispossession, environmental degradation, and uneven benefit distribution (Bebbington, 2012; Brazys et al., 2023; Christensen, 2019; Wegenast & Schneider, 2017).

A few exceptions explicitly examine the relationship between extractive industries and organized armed violence. Mihalache-O'Keef (2018) and Brazys et al. (2023) find that the presence of extractive industries heightens the risk of conflict, while Christensen (2019) and de Soysa (2020) report null or mitigating effects on organized armed conflict. Import-

antly, both Christensen (2019) and Brazys et al. (2023) use bargaining frameworks with information asymmetries, in which uncertainty about project outcomes and benefit distribution generates contestation between companies, governments, and local communities. In these accounts, violence functions as a mechanism for information revelation, a way for actors to update beliefs about each other's resolve or private valuations when formal bargaining breaks down. Although these studies treat organized armed violence as one outcome, they theorize bargaining primarily among companies, communities, and states, without modeling direct companies–armed-actor bargaining in wartime settings.

Building on existing literature on both natural disaster-conflict nexus and resource-conflict nexus, this paper advances the discussion by emphasizing the moderating role of third-party commercial companies and examining how they shape wartime bargaining with armed groups in the aftermath of sudden and negative shocks.

Theory

I develop a theory that explains how extractive companies moderate the impact of disaster shocks on civil conflict. I begin by contrasting the military strategies that armed actors use in territories with capital-intensive resource extraction, typically operated by companies with access to financial resources such as credit or insurance, with those used in other territories that have labor-intensive extraction sites or no resources. I then frame natural disasters as exogenous shocks that disrupt the balance of power between armed actors and create opportunities for opportunistic attacks. I argue that extractive companies, leveraging their post-disaster financial advantages, often "buy" stability to restore operations, thus dampening conflict. Where such companies are absent, particularly in labor-intensive extraction sites, sites with licenses nearing expiration, or non-resource territories, this buffer is missing, and civil conflict is more likely to intensify.

Pre-disaster conflict dynamics over resource

Natural resources often provide critical funding for armed groups and fuel violence, as armed actors seek either to control the resources directly or to disrupt extraction activities (Berman et al., 2017; Collier & Hoeffler, 2004; Elliott & Kreutz, 2019; Lujala et al., 2005;

Rigterink, 2020). However, the way armed groups profit from resources depends heavily on the type of extraction - capital-intensive (e.g., large-scale mining or oil and gas extraction) or labor-intensive (e.g., small-scale artisanal mining). This distinction shapes whether armed groups rely primarily on disrupting extraction to extort revenue or directly seizing and controlling resource-rich territories.

In territories with capital-intensive resource extraction sites, armed groups often target extraction operations through disruption in order to extort revenue from the extractive companies involved. Capital-intensive resources such as oil and gas extraction or large-scale mining require substantial financial and technological investments and are typically operated by extractive companies with access to financial resources (Boyes, 2020). These companies also frequently invest heavily in security to protect their infrastructure, making direct control by armed groups challenging (Blair et al., 2024).

Given these constraints, armed groups often use "weapons of the weak," such as sabotage, to extract revenue (Blair et al., 2024). By attacking infrastructure such as pipelines or transportation networks, they can signal threats and extort "security fees" from extractive companies. For example, between 1985 and 2014, left-wing guerrillas such as the National Liberation Army (ELN) and the Revolutionary Armed Forces of Colombia (FARC) in Colombia engaged in kidnappings, extortion, and bombing of oil pipelines to extract payments from oil companies. Similar patterns occurred in southeastern Turkey between 2009 and 2016, where the Kurdistan Workers' Party (PKK) engaged heavily in oil theft, and in Mexico, where the Ejército Popular Revolucionario (EPR) attacked natural gas pipelines and stole oil (Blair et al., 2024). Similarly, in Hpakant of Myanmar, one source noted that "they (extractive companies and miners) pay fees and taxes to both sides, which helps prevent Tatmadaw (Myanmar's military) and KIA from fighting for control of lucrative mining areas that would disrupt the operation of resource extractions" (Global Witness, 2021).

In contrast, in territories without the presence of extractive companies, armed actors often prioritize territorial control, a long-recognized objective in civil wars (O'sullivan, 1983; Zhao, 2025). This trend is especially pronounced in territories with labor-intensive re-

source extraction sites. Such sites require minimal financial and technological investment, making them accessible to armed actors and offering no protection from companies. In this context, the resource itself becomes the prize, as the resource–war literature predicts (Berman et al., 2017; Collier & Hoeffler, 2004). Armed groups act as "stationary bandits" (Olson, 1993), expelling rivals, monopolizing violence, and taxing land, production, or even miners' wages through local levy systems (Sánchez De La Sierra, 2020). For example, in eastern regions of the Democratic Republic of the Congo (DRC), various armed groups including some self-defense groups called Mai-Mai, and the Congolese National Armed Forces (FARDC) were involved in violence over the control of small-scale artisanal mining mainly for tin, tungsten, tantalum, and gold (referred to as 3TG) (Hanai, 2021).

Post-disaster conflict dynamics over resource

Scholars generally agree that disaster shocks disrupt the local power balance and create opportunities for armed actors to exploit temporary advantages (Eastin, 2016; Walch, 2018). When roads wash out, bases flood, or depots collapse, supply lines break down, and communications falter, defending forces are weakened. Attackers who suffer fewer losses thus face a window in which the cost of attacking is lower and the potential payoffs higher.

Thus, as a baseline, I hypothesize:

H1. After a disaster shock, the likelihood of civil conflict increases in territories with no resource extraction sites.

I theorize that whether these opportunities materialize after disaster shocks depends on the presence of extractive companies. These companies, typically operating capital-intensive extraction sites, invest heavily in infrastructure and equipment, and thus require long-term stability to safeguard profitability. Because violence and disorder directly disrupt operations, they have strong incentives to prevent conflict. In the aftermath of disaster shocks, such companies often prioritize resuming production and use their financial and logistical resources to promote stability.

Next, I explain the moderating role of extractive companies by comparing conflict dy-

namics across four settings: 1) territories with capital-intensive resource extraction sites, representing the presence of extractive companies, 2) those with labor-intensive resource extraction sites, 3) extraction sites where extraction licenses approach expiration, and 4) extraction sites directly hit by the disaster.

In territories with capital-intensive extraction sites

In territories with capital-intensive resource extraction sites, disaster shocks may create a temporary opportunity for armed groups to seize control of land. However, their limited financial and technological capacity to operate capital-intensive extraction independently deters them from doing so. Instead, armed actors there typically fund themselves through strategic extortion rather than direct control, extracting "security fees" that fluctuate with daily output. When a flood or earthquake halts production, washing out haul roads, flooding pits, shutting in wellheads, and prompting the evacuation of expatriate staff, the revenue stream collapses. With no output to sabotage, armed actors lose their day-to-day cash flow, and repair work can take weeks, leaving them short of payroll and munition funds.

Although the halt in production and revenue also affects extractive companies, they are far more financially resilient than armed actors because they are often protected by business-interruption insurance and their company credit. Furthermore, their priority is to resume production as soon as possible. Capital-intensive extractions involve large sunk costs, and every day of inactivity represents substantial financial losses. This combination of financial resources and urgency to restart operations places companies in a stronger bargaining position at a time when armed actors are cash-strapped. This widened gap allows companies to "buy" stability by offering cash, repair contracts, or relief supplies. Such payments may be directed to the armed actor extorting these companies or to rival actor contesting that site. In practice, companies often face pressure from both sides, but the drying up of revenues during disasters makes each actor more inclined to accept such arrangements. As a result, all parties share an immediate interest in resuming production and the associated flows of resources and payments.

Although direct evidence of post-disaster "buy-offs" is rare, there are well-documented

cases in which extractive actors collaborated with armed groups. These examples suggest the plausibility of companies stepping in to stabilize conflict when their commercial interests are at stake. For example, in Kachin State, Yup Zau Hkawng, the owner of Jadeland Company and one of the region's most powerful businessmen was reported to play an instrumental role in brokering the 1994 ceasefire between the Kachin Independence Organization (KIO) and the military regime (Frontier Myanmar, 2022). Similarly, in Nigeria, oil companies have been revealed to regularly make "stay-at-home" payments to armed groups to avoid attacks on their infrastructure (Amunwa, 2012). According to US embassy cables in 2003, Shell and Chevron made direct payments to individual armed militants in the Warri areas (WikiLeaks, 2003). Although these cases are not related to bargaining after disaster shocks, they suggest that these extractive actors have used their economic leverage to broker stability.

Thus, when a disaster hits territories with capital-intensive resource extraction sites, production is stopped, and the extortion income of armed actors collapses. Extractive companies, however, retain access to cash through insurance and company credit. Since sabotage pays only when output flows and armed actors lack the capacity to operate these resource extractions independently, they have few alternatives and are more likely to accept company side-payments to restore stability.

Thus, I hypothesize that

H2: After a disaster shock, the likelihood of civil conflicts increases less in territories with capital-intensive resource extraction sites compared to territories with no extraction sites.

In territories with labor-intensive resource extraction sites

As a direct comparison, in territories with labor-intensive resource extraction sites, the resources themselves function as valuable prizes to be fought over. Because labor-intensive extraction requires little capital or technology, companies are not needed and armed actors see greater returns in directly seizing land and labor. Once entrenched, they can establish taxation systems on both output and labor, reducing dependence on outside actors. This direct management of resources enables armed groups to assume state-like functions

(Tilly, 1985), as seen in artisanal mining in the Democratic Republic of Congo, secondary diamond mining in Sierra Leone, and some jade extraction sites in Myanmar (Global Witness, 2021; Hanai, 2021; Rigterink, 2020).

Disaster shocks disrupt the local balance of power and create a temporary advantage for rivals to seize control of these territories. Floods and earthquakes can wash out roads, topple checkpoints, and weaken local garrisons, lowering the logistical and military costs of a takeover. In the absence of extractive companies, there is no "buy-off" mechanism, leaving armed actors with few counter-incentives to launch opportunistic attacks. This combination of high returns from controlling resources, low entry barriers to extraction, and the absence of company mediation makes civil conflict more likely in territories with labor-intensive extraction sites.

Thus, I hypothesize that

H3: After a disaster shock, the likelihood of civil conflicts is more likely in territories with labor-intensive extraction sites compared to territories with no extraction sites.

Limits on companies' willingness to buy stability

Additionally, the company-led "buy-off" mechanism is not automatic. It operates only when companies anticipate future profits from the extraction sites. Two conditions can undermine this incentive and restore the opportunistic attack dynamics.

The first condition concerns the time horizon of extraction licenses, which affects the commitment of extractive companies to the resource extraction sites and their willingness to make stability-buying investments. When licenses are expired or approach expiration, companies tend to prioritize rapid extraction and are less willing to bear the extra costs of post-disaster instability[1]. In contrast, companies with longer-time licenses have stronger incentives to safeguard their investment and promote post-disaster stability. Thus, I hypothesize:

H4: The mitigating impact of capital-intensive resource extraction sites decreases in

[1]Operational note: in the empirical tests, "near expiry" is coded as fewer than two years remaining on the license.

territories with resource extraction sites whose license is about to expire, compared to territories where extraction sites hold active licenses.

The second condition concerns the ability of the extraction sites to generate stable revenue. Resource extraction involves multiple steps, including extraction, transportation, and processing. While extraction must occur at the site, other steps rely on auxiliary infrastructure such as roads or processing facilities. When disaster shocks disrupt this supporting infrastructure, but leave extraction sites intact, revenues can resume quickly once repairs are completed. In such cases, both extractive companies and armed groups have incentives to prioritize reconstruction and restrain violence, reinforcing the conflict-mitigating mechanism outlined earlier.

However, when disaster shocks directly damage extraction sites and halt production, revenue flows can cease entirely. For example, hydrological disasters such as heavy rain or floods pose serious risks to open-pit mines, underground mines, and quarries: equipment may be submerged, site infrastructure damaged, and prolonged inundation may degrade ore quality, reducing productivity and increasing reconstruction costs (Minetek, 2023).

Thus, when disaster shocks directly affect resource extraction sites, extractive companies and armed groups lose the opportunity to restore revenue flows and the incentives to maintain a conflict-avoidant environment. I hypothesize:

H5: The mitigating impact of capital-intensive resource extraction sites decreases in territories where extraction sites are directly hit by disaster shocks.

Research Design

To test how extractive companies shape the disaster-conflict nexus, I estimate a staggered-adoption DiD model with a rolling window on a township-month panel data. This data covers Myanmar's 330 townships from 2010 to 2018[2]. I focus on floods and earthquakes, the country's two most frequent disaster types during this time period.

[2]The time of study is restricted by the data availability of the flood events in Myanmar.

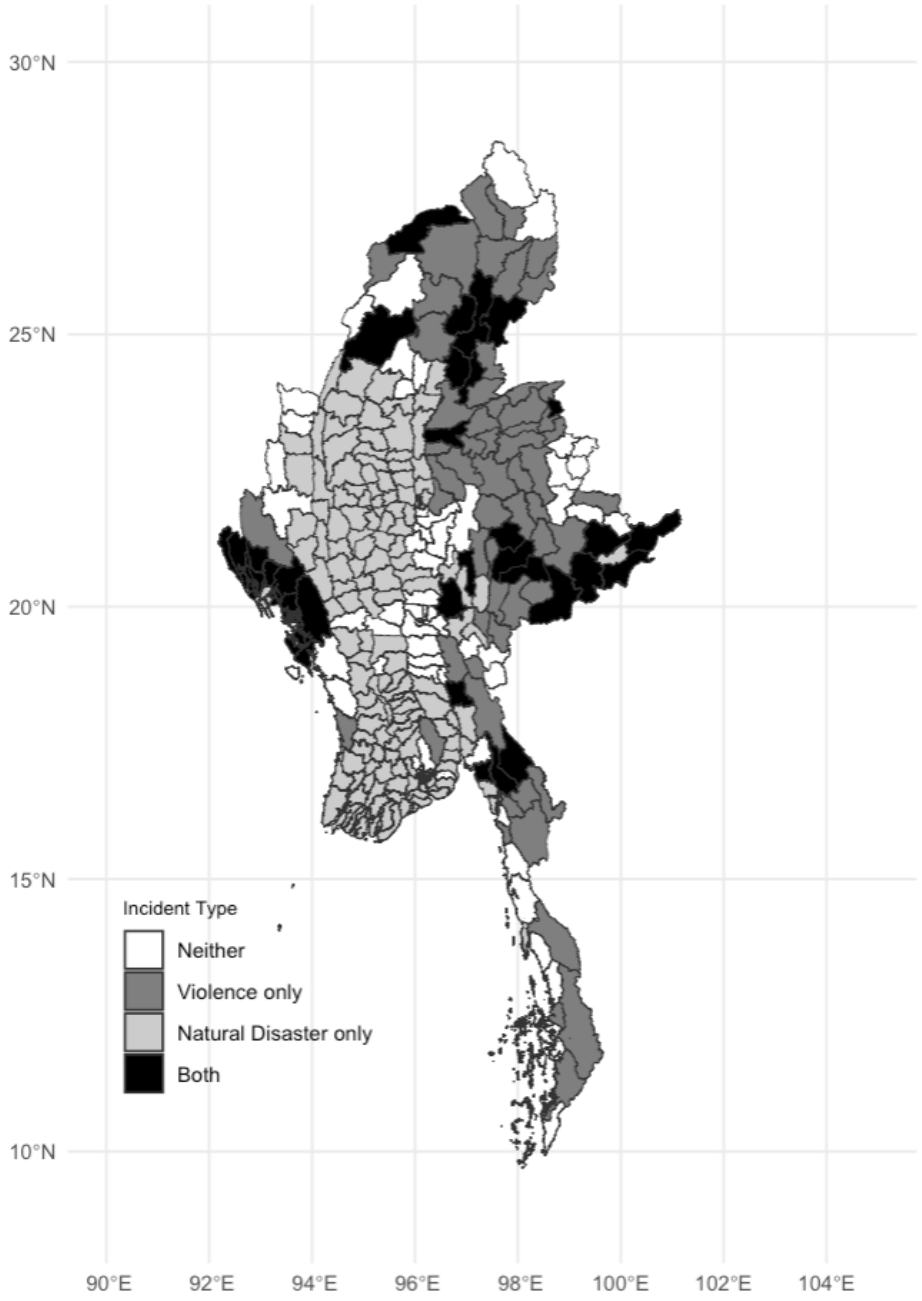

Figure 2.1: The Spatial Distribution of Civil Conflicts and Natural Disasters

Note: The map illustrates the townships experiencing civil conflicts or natural disasters or both at least once during at least one year from 2010 to 2018

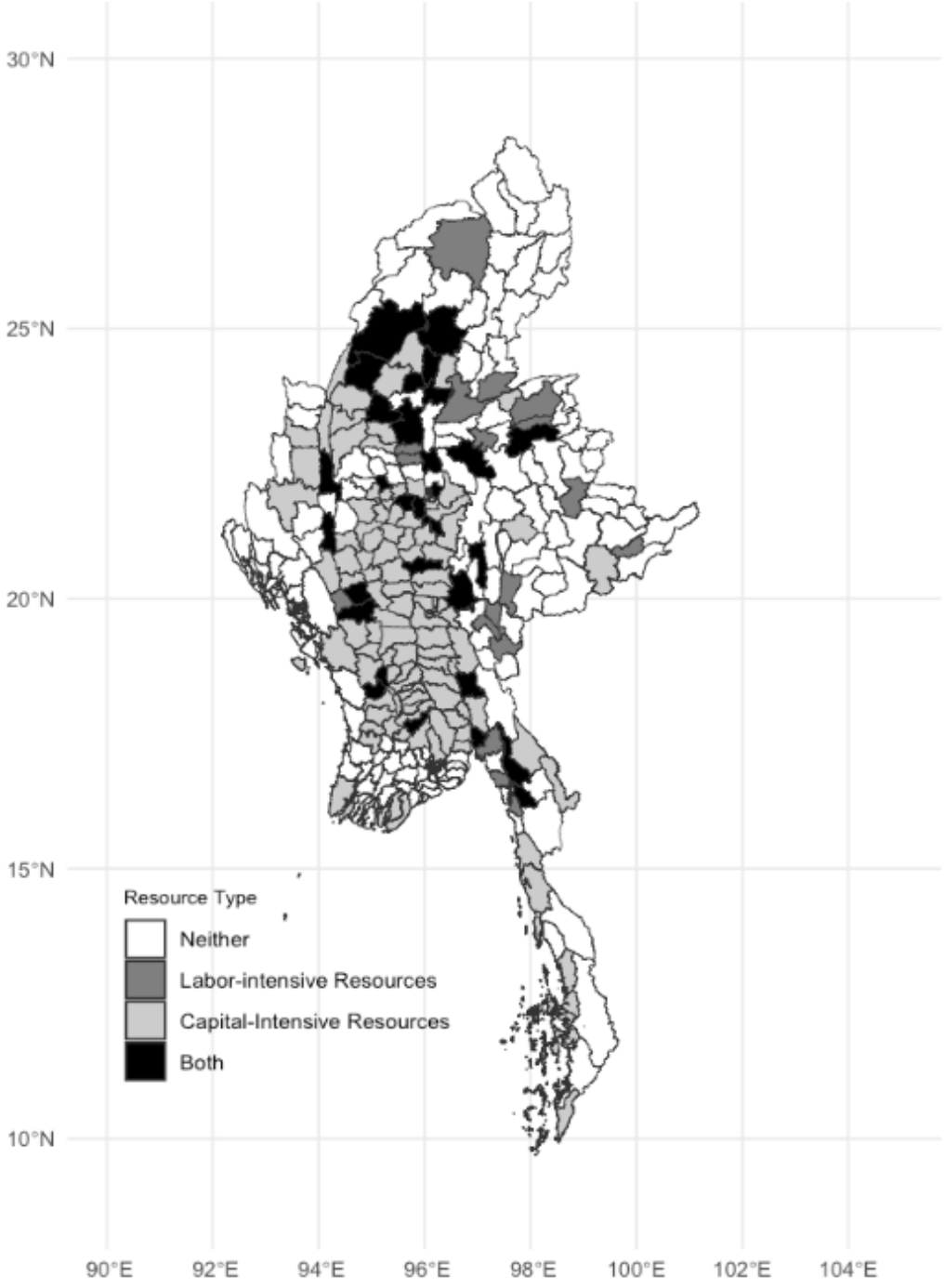

Figure 2.2: The Spatial Distribution of Resource Types in Myanmar

Note: The map illustrates the townships with at least one capital or labor-intensive resource extraction sites or both.

Case Selection

Myanmar offers an ideal testing ground. First, it is one of the world's most disaster-prone countries regularly hit by floods and earthquakes whose exact time and location are hard to anticipate (MIMU, 2022; World Bank, 2024). Those disaster types arrive largely

exogenously to local conflict dynamics, which provides a quasi-random temporal variation. Second, Myanmar's civil war is long-running and spatially fragmented. Since 1948, a mosaic of ethnic armed organizations has fought the Tatmadaw despite intermittent ceasefire rounds (Callahan, 2012; Chow, 2024). This brings cross-sectional variation that can be paired with the temporal shocks. Third, Myanmar is also richly endowed with natural resources extracted under very different modes of production: these include highly labor-intensive extraction sites such as artisanal jade and ruby mines and capital-intensive sites such as large-scale mining and offshore oil and gas extractions. Finally, resource revenue matters to all armed actors there. Both Tatmadaw and the rebel groups have historically taxed or controlled mines and pipelines to fund combat operations (Woods, 2014).

Empirical Strategy

While disaster shocks provide plausibly exogenous shocks, they also present a challenge for causal identification. Disaster exposure is recurrent and non-absorbing: the same township can be hit, recover, and be hit again. This pattern breaks the "once-treated-always-treated" assumption behind the standard and staggered TWFE estimator for DiD. When the treatment can be switched on and off, the TWFE design risks producing "forbidden comparisons", in which already-treated units can be mistakenly used as controls for later-treated units, thus creating the invalid counterfactual (de Chaisemartin et al., 2025; De Chaisemartin & d'Haultfoeuille, 2024; Sun & Abraham, 2021). In the disaster setting, two mechanisms create these flawed comparisons. First, disaster shocks can have the carryover effect, which allows their impacts to persist for years. Thus, the periods coded as "untreated" may still reflect the impact of the earlier disaster shocks. Second, overlapping treatments occur when a new disaster strikes before the previous one's effect fades, making it impossible to disentangle the influence of each event.

To address these challenges, I use a staggered-adoption design with a rolling treatment window. First, I anchor treatment on each township's first disaster and drop all observations after any second disaster to avoid overlap. Second, I exclude townships whose first disaster falls within two years of the 2008-2009 disaster sequence, ensuring a clean pre-period. Third, I impose a 12-month limited-carry-over assumption - a township is coded as treated if a disaster occurred in the previous year, while the outcome records whether

civil conflict arises in the subsequent twelve months. This 12 + 12 structure captures lingering effects, yet restores the "untreated' status after one year. Additionally, I test the plausibility of the parallel trends assumption (PTA) using an event-study model (Chiu et al., 2023) (see more in Appendix G), and no obvious violation of the PTA is found. Additionally, robustness checks that relax the above restrictions are introduced to ensure that the results are not an artifact of design choices.

The baseline model is a two-way fixed-effects regression with standard errors clustered by township and month:

$$Y_{i,t} = \alpha_i + \lambda_t + \beta_1 D_{i,t} + \epsilon_{i,t}$$

where $Y_{i,t}$ represents civil conflicts in each township i during the twelve months following month t. α_i denotes the township-fixed effect, while λ_t denotes the month-fixed effect. $D_{i,t}$ indicates disaster exposure for township i within the twelve months preceding month t. The error term denotes other unobserved factors that affect civil conflicts.

To test the moderating effect of extractive companies, I interact disaster exposure with indicators for labor-intensive and capital-intensive resource extraction sites:

$$Y_{i,t} = \alpha_i + \lambda_t + \beta_1 D_{i,t} + \beta_2 D_{i,t} \cdot \text{Labor Intensive}_i + \beta_3 D_{i,t} \cdot \text{Capital Intensive}_i + \epsilon_{i,t}$$

where $Labor.Intensive_i$ denotes an indicator variable, equaling 1 if the township has labor-intensive resource extraction sites, and zero otherwise. $Capital.Intensive_i$ is an indicator variable, equaling 1 if the township has capital-intensive resource extraction sites, and zero otherwise.

Data

Civil conflict, the dependent variable in my analysis, is measured using the Armed Conflict Location & Event Data Project (ACLED), which relies on "local, regional and national resources and the information", supplemented by Myanmar Peace Monitor for local insights (Raleigh et al., 2010).

The analysis focuses on the civil conflicts between the state and the rebel group, specifically including two types: battles and explosion/remote violence between government and rebel group. In ACLED, the battle is defined as "a violent interaction between two politically organized armed groups at a particular time and location", and explosion/remote violence is defined as "one-sided violent events in which the tool for engaging in conflict creates asymmetry by taking away the ability of the target to respond" (Raleigh et al., 2010, p. 656). Incidents are aggregated over the twelve months following a disaster shock and coded as 1 if any civil conflict occurs within that period, and 0 otherwise. Overall, 94 out of 330 townships in Myanmar between 2010 and 2018 experienced civil conflicts. In Figure 2.1, the spatial distribution of civil conflicts is displayed.

The independent variable, disaster exposure, includes two most frequent disaster types in Myanmar: floods and earthquakes. Flood data comes from the Global Flood Database (GFD), which builds on Dartmouth Flood Observatory records and provides high-resolution (250 m) inundation data for every major flood between 2000 and 2018 (Vestby et al., 2024). 23 flood events were recorded from 2010 to 2018 in Myanmar. I remove permanent water and overlay each flood footprint on Myanmar township polygons. Earthquake exposure comes from USGS ShakeMap, which provides contour lines of equal ground-shaking intensity. Because shaking damage extends beyond epicenters, I use the area enclosed by the Modified Mercalli Intensity ≥ 5 contour.

A township is coded as disaster-affected when either (i) at least 1 percent of its territory is inundated or shaken, or (ii) at least 10 percent of its resource extraction sites are directly hit. The 1 percent threshold is large enough to disrupt roads, markets, and security patrols, yet strict enough to avoid flagging trivial water coverage or minor shaking. The additional threshold for resource extraction sites reflects their strategic importance: damage to extraction sites directly reduces the revenues that armed actors rely on to fund operations. A significant loss, defined as at least 10 percent of an extraction site, can thus have a pronounced effect on armed actors' capacity and company incentives. Using the combination of these two thresholds captures both community-wide disruption, which may weaken local state or rebel control, and targeted funding shocks that can change the balance of incentives to maintain or resist post-disaster stability, in line with the paper's

theoretical mechanism. Over the study window, 134 townships are flooded at least once and 74 are shaken at least once. Figure 2.2 maps the distribution of disaster shocks. To ensure that the results are not an artifact of how disaster exposure is measured, I re-estimate the models under alternative thresholds for classifying a township as disaster-affected (see more in Appendix J).

I construct township-level indicators of resource extractions using spatial license data compiled by Patrick (2024) from the 6th Myanmar Extractive Industries Transparency Initiative (MEITI) report. This report covers two main types of natural resources – mining[3], and oil and gas. The resource license area is defined as "a legally defined area where the license holder has certain exclusive rights and obligations related to the exploration or extraction of natural resources" (Patrick, 2024). Although license polygons are sometimes imprecise, aggregation to the township level minimizes mapping error, and MEITI remains the most comprehensive source for Myanmar.

In this paper, I capture the distinction between capital-intensive and labor-intensive extraction based on the information about resource types from MEITI. Capital-intensive sites include large-scale mining and oil and gas blocks, which typically involve substantial investments and are operated by extractive companies. Labor-intensive sites include small-scale or artisanal mining, which require minimal equipment and are often operated informally. A township is coded as 1 if it contains at least one large-scale mining or an oil- or gas extraction site, and zero otherwise. Additionally, another indicator is constructed for the presence of any resource extraction sites, coding as 1 when a township contains any types of resource extraction sites, and zero otherwise. To test the impact of license expiration that relates to companies' willingness to buy the post-disaster stability, I create another indicator for the capital-intensive extraction sites whose license approaches expiration, coded as 1 if its extraction license is scheduled to expire within the next two years[4]. Finally, to test the mechanism related to the direct hit, I record whether a resource extraction site is directly hit by a disaster shock by overlaying the license polygons with

[3]MEITI lists jade and gemstones separately. I merge them into the mining category.

[4]license renewal negotiations for large-scale extraction projects typically begin years before expiry because regulatory review, environmental compliance, and capital budgeting take time. Thus, companies facing <2 years remaining often delay their effort on major reinvestment and security unless renewal is assured.

the affected areas of disaster shocks.

For robustness, I compare results with data from earlier 4th and 5th MEITI reports (2015 and 2016), finding that while the number of licenses has slightly increased, the presence of extraction sites remains stable at the township level (see Appendix O).

Results

Overall, the results support my hypotheses. It has been hypothesized that disaster shocks increase civil conflicts in territories with no resource extraction sites (H1) and with labor-intensive sites (H3), yet have no discernible effect - and may even slightly reduce civil conflicts in territories with capital-intensive sites (H2). The mitigating effect weakens as licenses for corresponding capital-intensive extraction sites approach expiration (H4).

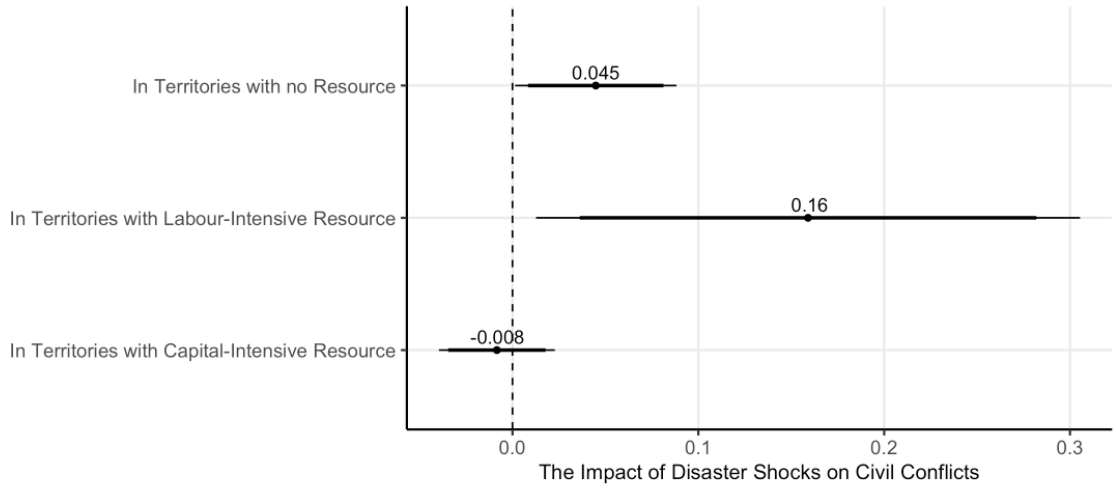

Figure 2.3: The Impact of Disaster Shocks on Civil Conflicts: 90% and 95% Confidence Intervals

Note: The above figure (from above to below) shows the impact of disaster shocks on civil conflicts in territories with no resource extraction sites, with the labor-intensive resource extraction sites, and with the capital-intensive resource extraction sites. This figure is based on Table 2.A1 in Appendix A.

According to Figure 2.3, townships with no resource extraction sites experience a statistically significant rise in civil conflict after a disaster shock ($p < 0.05$), reflecting the baseline post-disaster opportunistic attack mechanism (H1). Moreover, the increase is substantial, with the 4.5 percentage point increase in the likelihood of future civil conflicts compared

to a baseline of 12.7% in territories not experiencing any disaster shocks (roughly 36% rise).

In territories with labor-intensive resource extraction sites, the post-disaster conflict escalation is sharper, consistent with H3. According to Figure 2.3, the likelihood of civil conflict rises by 16 percentage point after a disaster - an effect that is statistically significant ($p < 0.05$) and more than triples the conflict-intensifying impact observed in territories without resource extraction sites. This pattern is consistent with H3.

In contrast, in territories with capital-intensive resource extraction sites, the conflict-intensifying impact observed in the territories with no resource extraction sites and labor-intensive extraction sites is muted. Adding a negative interaction (estimated coefficient = –0.167) (as shown in Table 2.A1 in Appendix A) almost offsets the baseline effect and leaves a net estimate near zero, as shown in Figure 2.3. These results support H2, suggesting that extractive companies would have a stronger post-disaster financial leverage over their armed actor counterparts, and then would buy stability for their long-term operations and revenue flow.

I further examine how license expiration affects their ability to moderate post-disaster civil conflicts (H4). When a disaster hits territories with capital-intensive extraction sites whose license is far from expiry, the interaction term is negative and sizeable (as shown in Table 2.A2 in Appendix B), implying a 14 percentage point decrease in the likelihood of post-disaster civil conflict ($p < 0.1$), as shown in Figure 2.4. This suggests that extractive companies that hold long-duration licenses and thus have strong incentives to protect future revenue can almost fully offset the conflict-intensifying impact of a disaster shock.

However, this mitigating effect is noticeably weaker once a license for capital-intensive sites is near or already at expiry. Figure 2.4 shows that the townships in these territories experienced a 2.2 percentage point decrease only ($p > 0.1$) compared to 14% in territories with capital-intensive resource extraction sites whose license is far from expiry ($p < 0.1$). This implies that when future rents are no longer guaranteed, a company's willingness to pay for stability wanes, and thus erodes its buffer role, offering support for H4.

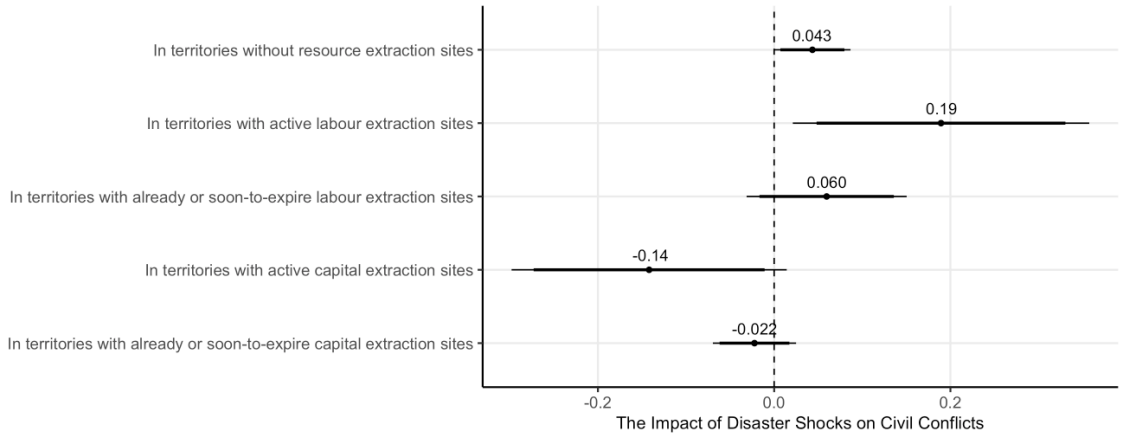

Figure 2.4: The Impact of Disaster Shocks on the Civil Conflicts: 90% and 95% Confidence Intervals (Expired Licenses)

Note: The above figure (from above to below) shows the impact of disaster shocks on civil conflicts in territories with no resource extraction sites, with the active labor-intensive resource extraction sites, with the soon-to-expire labor-intensive resource extraction sites, with the active capital-intensive resource extraction sites, and with the soon-to-expire capital-intensive resource extraction sites. This figure is based on Table 2.A2 in Appendix B.

Additionally, I examine whether the impact of disaster shocks changes when they directly hit resource extraction sites. According to Table 2.A3 in Appendix C, in territories with capital-intensive sites, the interaction term remains negative (–0.13) and is statistically insignificant ($p > 0.1$). This suggests that direct damage does not increase civil conflict and provides no statistical support for H5.

Robustness

The results remain robust across various checks, including a test of competing theoretical mechanisms, alternative DiD specifications, and different ways of measuring both disaster exposure and civil conflict.

First, a plausible alternative account of the lower post-disaster conflict observed in territories with capital-intensive resource extraction sites focuses on post-disaster troop redeployment. Because capital-intensive extraction sites are strategically important to the military and many rebel groups, both sides may prioritize sending troops to disaster-affected territories, deterring attacks and reducing civil conflict there. This logic has another important and testable implication: if troops are pulled in to guard a copper mine or oil terminal,

the nearby territories that they leave behind should be weaker on defense and, by Powell (2007)'s logic, more attractive targets for the rivals. The redeployment story thus predicts higher post-disaster conflict in unaffected townships adjacent to affected territories with capital-intensive extraction sites.

To test this, I include spatial lags for both the presence of labour- and capital-intensive extraction sites and interact them with the disaster exposure. As shown in Table 2.A5 (Appendix E), the interaction for neighboring townships is small and statistically indistinguishable from zero ($p > 0.1$). Thus, there is no statistical evidence that post-disaster troop redeployment is driving the main result.

Another alternative mechanism is that the mitigating effect of capital-intensive sites may not reflect companies "buying peace" but rather the deterrent effect of home states of these foreign investors (Brazys et al., 2025; Christensen, 2019; Wegenast & Schneider, 2017). Because large-scale, capital-intensive extraction projects in the developing countries involve international investment, armed actors may refrain from attacking them out of fear that violence could trigger diplomatic pressure or retaliation from home states of these foreign investors.

To rule out this explanation, I interact disaster exposure with the capital-intensive site indicator and allow the effect to vary with the proportion of joint-venture and foreign-exclusive licenses. If the pacifying effect is driven primarily by foreign deterrence, one would expect that the mitigating effect to appear only in townships dominated by joint ventures or foreign-exclusive licenses. As a complementary test, I interacted disaster exposure directly with the proportions of joint venture and foreign-exclusive projects without conditioning on the capital-intensive site indicator. Those results show no evidence of this pattern (see Appendix F).

Second, because my staggered DiD design with a rolling window relies on specific design choices, it is important to check that the results are not an artifact of these decisions, especially how I handle multiple disasters and carry-over effects. In the robustness analysis, I relax two key restrictions. First, instead of dropping all post-second-disaster observations,

I retain townships that experience at least a two-year gap after any earlier disaster. This allows me to verify that the estimated effects are not limited to the first disaster alone, and that subsequent disasters (when isolated) do not distort the results (see Appendix H). Second, I vary the rolling treatment window from six to twelve months to test whether the results depend on the specific duration assumed for carry-over effects (see Appendix I). Across these alternative windows, the results remain robust.

Third, to ensure that the results are not an artifact of how disaster exposure is measured, I re-estimate the models under alternative spatial cutoffs: i) using only the $> 1\%$ township-area overlap rule; ii) varying the township-area threshold at 2%, 3%, 4%, 5%, 10%, and 20%. The results remain robust across these specifications (see Appendix J). Additionally, to further test the heterogeneous impact across separate types of disaster shocks, I disaggregate the main treatment indicator into separate variables for floods and earthquakes. While the estimated effects for each disaster type remain substantively similar to the pooled estimate, the statistical significance weakens (see Appendix K). This drop in precision is expected: splitting the treatment reduces the number of treated observations for each disaster type, which widens standard errors.

Fourth, I also test whether results depend on how future civil conflict and conflict type are defined. I first re-estimate the models using alternative post-disaster windows (from 6 to 12 months) to capture subsequent civil conflicts (see Appendix L). I then restrict the dependent variable to state–rebel battle events only, excluding other ACLED categories of remote attacks (see Appendix M). Finally, I distinguish conflict onset from continuation by i) estimating a dynamic specification with a lagged conflict indicator and ii) running separate models for township-months with (continuation) and without prior conflict (onset) (see Appendix N). The results are robust across all these alternative model specifications.

Finally, I test whether the license-expiry mechanism is sensitive to the window used to capture the resource extraction sites with already or soon-to-expire license. I re-estimate all models with alternative cut-offs: licenses due to expire within 12 months and within 36 months. As shown in Appendix P, the 36-month specification is robust, while the 12-month specification loses statistical significance. This pattern is consistent with industry practice:

negotiations to renew large-scale resource extraction licenses typically begin about two years before formal expiry, because regulatory review, environmental compliance, and capital-budget planning are time-consuming. By the final year of a license, most companies already know whether renewal is forthcoming and thus face much less revenue uncertainty.

Conclusion

In this paper, I analyze how third-party commercial actors moderate the impact of sudden and negative shocks on civil conflicts, using disaster shocks and extractive companies as cases. The findings show that disaster shocks increase civil conflicts in territories with no resource extraction sites and with labor-intensive sites, yet have no discernible effect - and may even slightly reduce civil conflicts in territories with capital-intensive sites. The mitigating effect weakens slightly as licenses for corresponding capital-intensive extraction sites approach expiration.

Whereas most conflict research focuses on strategic interactions solely between government forces and rebel groups in the aftermath of shocks, this study incorporates the role of third-party commercial actors, who often have the capacities and incentives to stabilize local areas for uninterrupted operations and profitability. Furthermore, while existing resource-conflict literature has often emphasized that resource wealth increases violence, this paper shows that extractive companies, often present in territories with capital-intensive resource extraction sites, can mitigate post-disaster violence. Finally, while existing literature on extractive industries largely conceptualizes violence as emerging from either company–civilian or company–state dynamics, this paper extends the analysis to how companies affect wartime bargaining between armed actors. In doing so, it bridges the IPE literature's focus on extractive industries with the conflict literature's emphasis on wartime bargaining.

As with other sub-national level research, it is important to explain how my results from the single case of Myanmar can generalize to other civil war contexts (Blattman & Miguel, 2010). While Myanmar has its particularities, this country also displays several similarities with other civil war countries. Civil war in Myanmar is rooted in ethnic divisions,

with various ethnic armed groups seeking autonomy or independence. Similar ethnic or religious tensions are present in other civil war contexts such as Syria, Afghanistan, and South Sudan. Moreover, like Myanmar, many conflict-affected developing countries such as Colombia, the Democratic Republic of Congo, and Nigeria are resource-rich and host capital-intensive extraction projects with strong involvement from extractive companies. In these contexts, weak or fragmented central governments often grant extractive companies - whether private, joint ventures, or even state-owned companies - great autonomy to manage security and negotiate with armed groups. Finally, these countries also face significant threats from disaster shocks, exacerbated by ongoing climate change. Thus, the insights from this paper are potentially applicable to other conflict-affected regions with natural resource extraction and frequent disaster exposure.

Finally, while this paper recognizes the short-term conflict-mitigating effects of extractive companies, it is crucial to recognize accompanying negative externalities. For instance, in Myanmar, taxation and extortion from resource revenue are key funding sources for both the military and rebel groups. Although these extractive companies may reduce immediate violence post-disaster, their presence without any proper regulations may contribute to the persistence of conflict over time by fueling revenue sources for all parties involved. Furthermore, extractive actors may circumvent government regulations and ignore local appeals, leading to environmental degradation and socio-economic harm, as seen in Myanmar's jade and gold mining regions in Kachin or oil industry in Nigeria's Niger Delta region. Future research should continue exploring the impacts of resource extraction sites in post-disaster civil war contexts, including potential negative externalities on local communities and long-term conflict dynamics.

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Appendix

1. Main models
    A. Main models for moderating impact on civil conflict
    B. Considering the license expiration
    C. Considering the extraction sites directly hit by disasters
    D. The unconditional impact of resource extraction sites on civil conflict
2. Alternative theoretical explanations
    E. The spillover impact of disaster shocks
    F. Considering the distinction between domestic and international owned companies
3. Alternative DiD design
    G. Parallel Trending Assumption - Event study model
    H. Retaining the township-month two years after the previous disaster occurrence
    I. Varying the rolling window for disaster exposures - from 6 months to 12 months
4. To address the concern about the result sensitivity to different measures of key variables
    J. Using different threshold for coding the disaster exposure
    K. Looking at different types of natural disasters
    L. Using different time windows to capture the future civil conflict - from 6 to 12 months
    M. Using the battle between government and rebel only as the outcome
    N. Using the dynamic model to differentiate between the escalation and occurrence of civil conflict
    O. The comparison between my use of 6th MEITI and its 5th version
    P. Using different time windows to capture the nearly expired licenses

Appendix A: Main models for moderating impact on civil conflict

Table 2.A1: The Conditional Effect of Disaster Shocks on Civil Conflicts (Main Models)

Dependent Variable:	Gov V.S. Rebels
Model:	(1)
Variables	
Disaster exposure	0.0448**
	(0.0222)
Disaster exposure × Labor-intensive sites	0.1142
	(0.0753)
Disaster exposure × Capital-intensive sites	-0.1674**
	(0.0740)
Fixed-effects	
Township	Yes
Month	Yes
Fit statistics	
Observations	27,206
R^2	0.64167
Within R^2	0.00468

Clustered (Township & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix B: Considering the licensee expiration

Table 2.A2: The Conditional Effect of Disaster Shocks on Civil Conflicts (the Licensee Expiration)

Dependent Variable:	Gov V.S. Rebels
Model:	(1)
Variables	
Disaster exposure	0.0434**
	(0.0221)
Capital-intensive sites (expiring)	0.0214
	(0.0347)
Labor-intensive sites (expiring)	-0.0189
	(0.0423)
Disaster exposure × Labor-intensive sites	0.1459*
	(0.0866)
Disaster exposure × Capital-intensive sites	-0.2014**
	(0.0857)
Disaster exposure × Capital-intensive sites(expiring)	0.1195
	(0.0739)
Disaster exposure × Labor-intensive sites(expiring)	-0.1298*
	(0.0783)
Fixed-effects	
Township	Yes
Month	Yes
Fit statistics	
Observations	27,206
R^2	0.64198
Within R^2	0.00554

Clustered (Township & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix C: Considering the extraction sites directly hit by disasters

Table 2.A3: The Conditional Effect of Disaster Shocks on Civil Conflicts (Extraction Sites Directly Hit by Disasters)

Dependent Variable:	Gov V.S. Rebels
Model:	(1)
Variables	
Disaster exposure	0.0425**
	(0.0207)
Labor-intensive sites affected by disasters	0.0824
	(0.1050)
Capital-intensive sites affected by disasters	-0.0534
	(0.1007)
Disaster exposure × Labor-intensive sites	0.0617
	(0.0758)
Disaster exposure × Capital-intensive sites	-0.1333*
	(0.0718)
Fixed-effects	
Township	Yes
Month	Yes
Fit statistics	
Observations	27,442
R^2	0.65440
Within R^2	0.00495

Clustered (Township & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix D: The unconditional impact of resource extraction sites on civil conflict

Table 2.A4: The Conditional Effect of Resource Extraction Sites on Civil Conflict

Dependent Variable:	Gov V.S. Rebels	
Model:	(1)	(2)
Variables		
Labor-intensive sites	0.1941***	0.1948***
	(0.0071)	(0.0068)
Capital-intensive sites	-0.2472***	
	(0.0059)	
Oil sites		-0.2715***
		(0.0057)
Large mining sites		-0.0150***
		(0.0057)
Fixed-effects		
Month	Yes	Yes
Fit statistics		
Observations	43,560	43,560
R^2	0.08770	0.11181
Within R^2	0.08312	0.10735

Clustered (Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

I examine the impact of different types of resource extraction sites on civil conflict, independent of disaster shocks. As shown in Model 1 in Table A4, the presence of labor-intensive extraction sites is associated with a statistically significant increase in civil conflict, about 19.4% higher than in territories without such sites. This finding is consistent with the established resource curse theory, which suggests that territories with small-scale or artisanal extraction tend to face greater baseline conflict risk due to weaker security and greater opportunities for local armed groups to compete over easily extractable resources.

However, this conflict-intensifying effect varies by resource type. The presence of capital-intensive sites reduces the likelihood of conflict by about 24.7%. More specifically, according to Model 2 in Table A4, the mitigating effect is stronger for oil sites, which are associated with a 27.2% reduction in civil conflict. Similarly, large mining sites are linked to a modest mitigating effect, with a 1.5% decrease in conflict likelihood.

Appendix E: The spillover impact of disaster shocks

Table 2.A5: The Spillover Conditional Effect of Disaster Shocks on Civil Conflicts

Dependent Variable:	Gov V.S. Rebels
Model:	(1)
Variables	
Disaster exposure	0.0078
	(0.0179)
Disaster exposure(Spatial lag)	-0.0007
	(0.0194)
Labor-intensive sites(Spatial lag)	-0.0620
	(0.0474)
Capital-intensive sites(Spatial lag)	0.0376
	(0.0423)
Disaster exposure × Labor-intensive sites	0.1628
	(0.1118)
Disaster exposure(Spatial lag) × Labor-intensive sites(Spatial lag)	0.1133
	(0.0702)
Disaster exposure × Capital-intensive sites	-0.2127*
	(0.1210)
Disaster exposure(Spatial lag) × Capital-intensive sites(Spatial lag)	-0.0842
	(0.0690)
Fixed-effects	
Township	Yes
Month	Yes
Fit statistics	
Observations	27,206
R^2	0.64197
Within R^2	0.00550

Clustered (Township & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix F: Considering the distinction between domestic and international owned companies

A possible alternative explanation for the mitigating effect of capital-intensive extraction sites is foreign deterrence: armed groups may choose not to target foreign-involved sites to avoid diplomatic or economic costs. To address this concern, I classify all licenses into three mutually exclusive categories at the license level: (1) domestic projects (licenses not operated as joint ventures), (2) joint ventures, and (3) foreign-exclusive projects, using the information provided by MEITI report, and then aggregate them to the township–month level to construct proportions of domestic, joint venture, and foreign-exclusive sites in each township.

I re-estimate the main specification by interacting disaster exposure with capital-intensive sites indicator and allowing interactive effect to vary with these township-level proportions of Joint-venture and foreign exclusive licenses. This model evaluates whether the mitigating effect of capital-intensive sites is conditional on foreign involvement.

As a complementary test, I interacted disaster exposure directly with the proportions of joint venture and foreign-exclusive projects without conditioning on the capital-intensive site indicator.

Table 2.A6: The Conditional Effect of Disaster Shocks on Civil Conflicts, Interacting with Foreign or Joint-Venture Licenses

Dependent Variable:	Gov V.S. Rebels	
Model:	(1)	(2)
Variables		
Disaster exposure	0.0738	0.0811
	(0.0516)	(0.0494)
Disaster exposure× Joint Venture(%)	-0.0292	
	(0.0289)	
Disaster exposure × Foreign(%)	-0.0436	
	(0.0491)	
Disaster exposure × Capital-intensive sites × Joint Venture(%)		-0.0432
		(0.0267)
Disaster exposure × Capital-intensive sites × Foreign(%)		-0.0447
		(0.0453)
Fixed-effects		
Township	Yes	Yes
Time	Yes	Yes
Fit statistics		
Observations	15,493	15,493
R^2	0.66079	0.66099
Within R^2	0.00213	0.00274

Clustered (Township & Year-month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix G: Parallel Trending Assumption

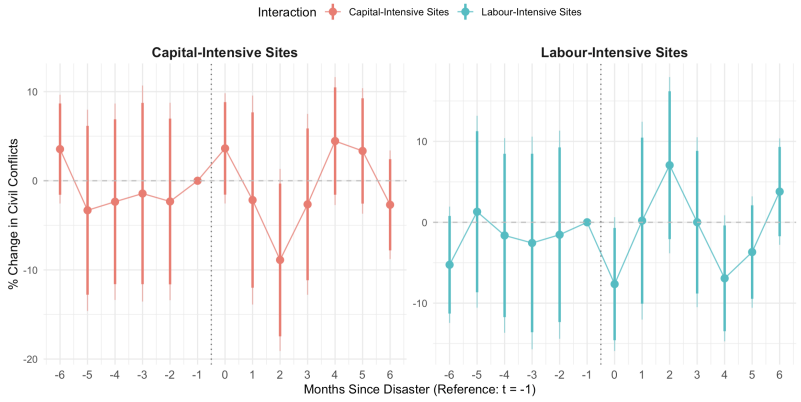

Figure 2.A1: A Dynamics Model with Leads and Lags

To test the parallel trends assumption (PTA), I estimate an event-study model using the same sample and fixed-effects structure as the main analysis. The specification interacts monthly leads and lags of disaster exposure with the capital-intensive and labour-intensive site indicators, controlling for township and month fixed effects and two-way clustered standard errors. The reference period is $t = -1$ (one month before the disaster): all pre- and post-disaster coefficients are normalised against this period.

Figure A1 plots the interaction coefficients - the differential conflict trend in capital-intensive and labour-intensive townships relative to no-resource townships across pre-disaster months ($t = -6$ to $t = -2$) and post-disaster months ($t = 0$ to $t = +6$). The pre-disaster coefficients constitute the PTA test: non-zero values would indicate that resource townships were already trending differently before the disaster, threatening the causal interpretation of the interaction effects. Both sets of pre-disaster coefficients are close to zero, and joint F-tests fail to reject the null of jointly zero pre-disaster interactions for capital-intensive ($p = 0.363$) and labour-intensive sites ($p = 0.4767$).

Appendix H: Retaining the township-month two years after the previous disaster occurrence

As a robustness, I relax the restriction that drops all observations after a township's second disaster. Instead, I retain townships that experience at least a two-year gap after any earlier disaster, ensuring that any overlapping treatment effects are likely to have faded. This test verified that my main findings are not driven by the exclusion of later-period observations.

Table 2.A7: The Conditional Effect of Disaster Shocks on Civil Conflict, Retaining Units Two Years After the Previous Disaster Occurrence

Dependent Variable:	Gov V.S. Rebels
Model:	(1)
Variables	
Disaster exposure	0.0392*
	(0.0212)
Disaster exposure × Labor-intensive sites	0.1127
	(0.0731)
Disaster exposure × Capital-intensive sites	-0.1562**
	(0.0722)
Fixed-effects	
Township	Yes
Month	Yes
Fit statistics	
Observations	27,495
R^2	0.63887
Within R^2	0.00424

Clustered (Township & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix I: Varying the rolling window for disaster exposures

In this section, I examine the sensitivity of the results to the specification of the disaster exposure rolling window. Specifically, I compare the main estimates using a one-month rolling-treatment window to alternative windows ranging from one to twelve months, and to a version that uses no rolling window at all and instead codes disaster exposure only for the month in which the disaster occurs. This allows me to assess whether the estimated impacts of disaster shocks on civil conflict are robust to different assumptions about how long disaster effects persist.

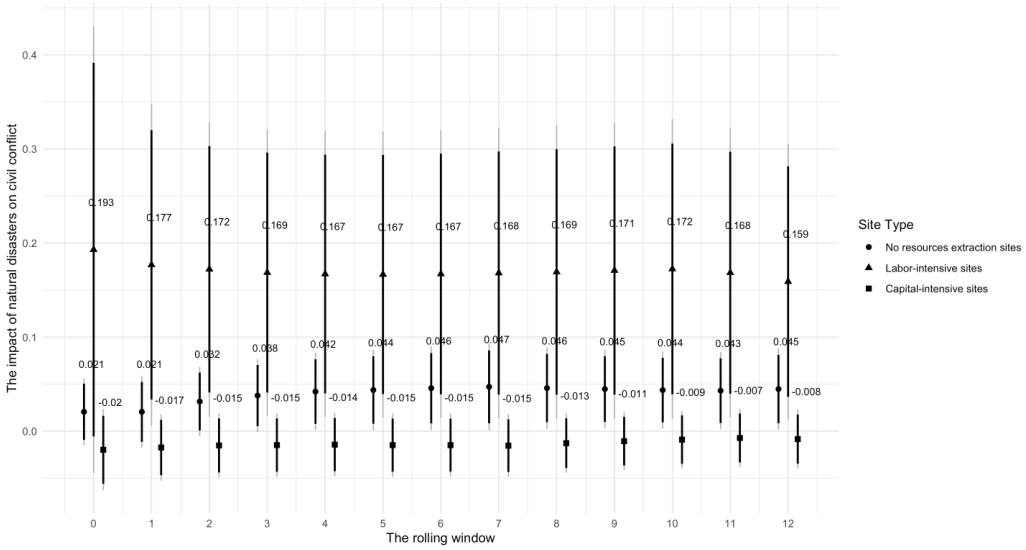

Figure 2.A2: The Impact of Disaster Shocks on Civil Conflict Across Different Rolling-Treatment Windows: 90% and 95% Confidence Intervals (Varying the Rolling Window for Disaster Exposures)

This figure shows the estimated impact of disaster shocks on civil conflict for territories with no resource extraction sites, labor-intensive extraction sites, and capital-intensive extraction sites. The x-axis indicates the length of the rolling-treatment window in months. The point at zero represents the specification without a rolling window, using only the month of the disaster. Vertical lines show 90% (black) and 95% (grey) confidence intervals.

Appendix J: Using different threshold for coding the disaster exposure

Table 2.A8: The Conditional Effect of Disaster Shocks on Civil Conflicts, Using Different Threshold for Coding the Disaster Exposure

Dependent Variable:	Gov V.S. Rebels						
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables							
Disaster exposure	0.0446**	0.0208	0.0518*	0.0556	0.0145	-0.0086	0.0038
	(0.0221)	(0.0158)	(0.0281)	(0.0339)	(0.0221)	(0.0139)	(0.0080)
Disaster exposure × Labor	0.0902	0.1075	0.0774	0.0673	0.1110	0.1375**	0.1253*
	(0.0760)	(0.0729)	(0.0730)	(0.0719)	(0.0672)	(0.0692)	(0.0686)
Disaster exposure × Capital	-0.1460*	-0.1247*	-0.1259*	-0.1218*	-0.1273*	-0.1316*	-0.1317*
	(0.0753)	(0.0727)	(0.0688)	(0.0655)	(0.0654)	(0.0693)	(0.0694)
Fixed-effects							
Township	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Observations	27,270	28,379	28,891	29,244	29,495	30,202	30,456
R^2	0.64133	0.63605	0.63487	0.63296	0.63698	0.63643	0.63666
Within R^2	0.00340	0.00256	0.00326	0.00303	0.00255	0.00250	0.00249

Clustered (Township & Year-month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Each model applies a different threshold of disaster exposure. Model 1 uses more than 1% township-area overlap only, and the rest of models use alternative thresholds corresponding to 2%, 3%, 4%, 5%, 10%, and 20%.

Appendix K: Looking at different types of natural disasters

Table 2.A9: The Conditional Effect of Earthquake and Flood on Civil Conflicts

Dependent Variable:	Gov V.S. Rebels
Model:	(1)
Variables	
Earthquake exposure	0.0713*
	(0.0422)
Flood exposure	0.0356
	(0.0266)
Earthquake exposure × Labor-intensive sites	0.0593
	(0.0967)
Earthquake exposure × Capital-intensive sites	-0.1227
	(0.0900)
Flood exposure × Labor-intensive sites	0.1134
	(0.1243)
Flood exposure × Capital-intensive sites	-0.1819
	(0.1247)
Fixed-effects	
Township	Yes
Month	Yes
Fit statistics	
Observations	27,206
R^2	0.64131
Within R^2	0.00366

Clustered (Township & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix L: Using different time windows to capture the future civil conflict - from 6 to 12 months

In this section, I examine the sensitivity of the results to how future civil conflict is defined. Specifically, I estimate the impact of disaster exposure on the likelihood that civil conflicts occurs within varying time horizons following a disaster, ranging from twelve months down to six months. The outcome variables are defined to capture whether any violent incident takes place within one to X months after a disaster. This approach allows me to assess whether the estimated effects of disasters persist over different lengths of time and ensures that the findings are not driven by an arbitrary choice of the outcome window.

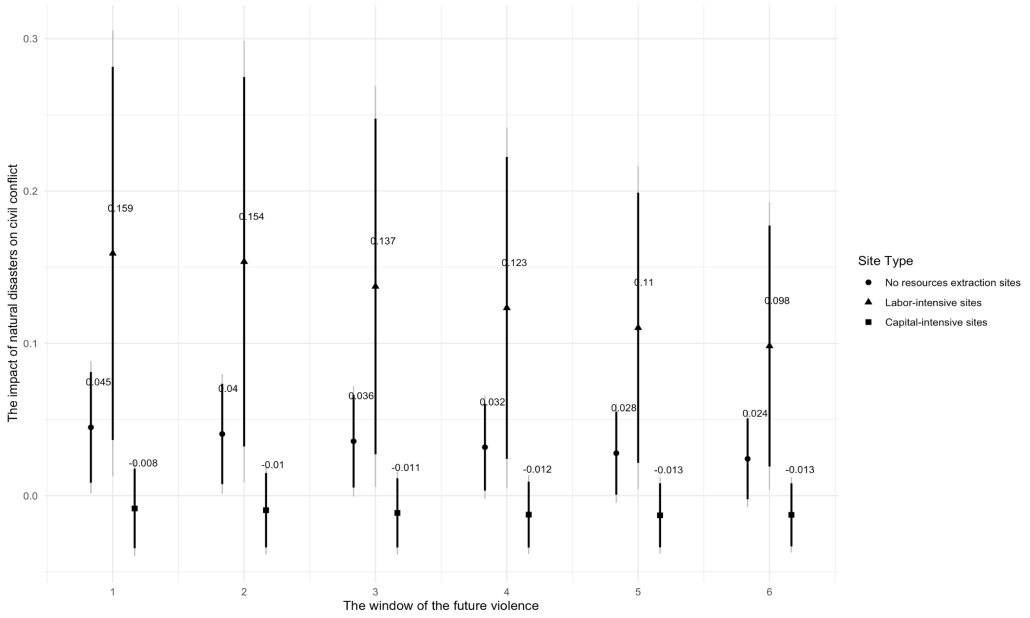

Figure 2.A3: The Impact of Disaster Shocks on Civil Conflict Across Different Windows to Define Conflict Outcome: 90% and 95% Confidence Intervals

Note: This figure shows the estimated impact of disaster shocks on civil conflict for territories with no resource extraction sites, labor-intensive extraction sites, and capital-intensive extraction sites. The x-axis indicates the length of the future conflict window in months. Each estimate reflects the effect on the probability that civil conflict occurs within the specified period following a disaster. For example, the 1 captures civil conflict occurring within one to twelve months post-disaster, while 2 covers one to eleven months. Vertical lines represent 90% (black) and 95% (grey) confidence intervals.

Appendix M: Using the battle between Government and Rebel only as the Outcome

Table 2.A10: The Conditional Effect of Disaster Shocks on Battle between the Government and the Rebel Only

Dependent Variable: Model:	Battle only (1)
Variables	
Disaster exposure	0.0453**
	(0.0221)
Disaster exposure × Labor-intensive site	0.1131
	(0.0752)
Disaster exposure × Capital-intensive site	-0.1735**
	(0.0743)
Fixed-effects	
Township	Yes
Month	Yes
Fit statistics	
Observations	27,206
R^2	0.63844
Within R^2	0.00478

Clustered (Township & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix N: Using the dynamic model to differentiate between the escalation and occurrence of civil conflict

In this appendix, I test whether the estimated effect of disaster exposure differs for the onset of new conflict versus the continuation of existing violence. To do so, I first estimate a dynamics model and then split the sample based on whether a township experienced government-rebel conflict in the prior twelve months.

Model (1) estimates a dynamics model with including the prior civil conflict as a control. Model (2) estimates the effect of disaster exposure for township-months without prior conflict, capturing the probability of new conflict onset. Model (3) estimates the effect for township-months with prior conflict, capturing the probability that conflict persists or reoccurs (continuation/escalation).

The dependent variable in all models is a binary indicator equal to one if government-rebel conflict occurs in the following twelve months. Both specifications include township and year-month fixed effects, and standard errors are clustered by township and month.

Table 2.A11: The Conditional Effect of Disaster Shocks on Civil Conflict Onset and Continuation

Dependent Variable:	Gov V.S. Rebels(t+1)		
Model:	A dynamic model	Conflict Onset	Continuation
Variables			
Disaster exposure	0.0493**	0.0227**	0.2263***
	(0.0219)	(0.0112)	(0.0462)
Prior civil conflict	-0.0220		
	(0.0383)		
Disaster exposure × Labor-intensive sites	0.1334*	0.0798	0.0661
	(0.0789)	(0.0669)	(0.1322)
Disaster exposure × Capital-intensive sites	-0.1867**	-0.1053	-0.3854*
	(0.0776)	(0.0683)	(0.2080)
Fixed-effects			
Township	Yes	Yes	Yes
Month	Yes	Yes	Yes
Fit statistics			
Observations	23,498	20,515	2,983
R^2	0.67579	0.51943	0.48576
Within R^2	0.00750	0.00289	0.01556

Clustered (Township & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix O: The comparison between my use of 6th MEITI and its 4th and 5th version

To assess whether my findings are sensitive to the choice of MEITI version, I compare township and licenses-level coverage across the 4th, 5th, and 6th reports. The first table reports the number of townships in which any mining or oil licenses are present, while the second table reports the total number of licenses across these sectors. As shown, the total number of licenses has grown somewhat between the 4th and 6th MEITI reports, but the geographic distribution of licensed activity at the township level remains stable. This suggests that the choice of version does not materially affect the results. 4th MEITI reports are suggested to be very rough and imprecise.

Table 2.A12: Number of Towns Reporting Mining, Oil, and Overall Licenses by MEITI Version

Version	4th MEITI	5th MEITI	6th MEITI
Mining	118	120	120
Oil and Gas	123	114	114
Overall	200	192	192

Table 2.A13: Number of Licenses Reporting Mining, Oil, and Overall Licenses by MEITI Version

Version	4th MEITI	5th MEITI	6th MEITI
Mining	197208	160116	160116
Oil and Gas	25872	22308	22308
Overall	26400	25344	25344

Appendix P: Using different time windows to capture the nearly expired license

Table 2.A14: The Conditional Effect of Disaster Shocks on Civil Conflict, Using Different Window to Capture the Nearly Expired License

Dependent Variable:	Gov V.S. Rebels(t+1)	
Model:	12 months	36 months
Variables		
Disaster exposure	0.0427*	0.0434*
	(0.0221)	(0.0221)
Capital-intensive sites	-0.0009	0.0170
	(0.0126)	(0.0341)
Labor-intensive sites	-0.0505	-0.0078
	(0.0609)	(0.0386)
Disaster exposure × Labor-intensive sites	0.1522*	0.1559*
	(0.0897)	(0.0901)
Disaster exposure × Capital-intensive sites	-0.2055**	-0.2137**
	(0.0884)	(0.0893)
Disaster exposure × Capital-intensive sites(expiring)	0.0178	0.1484*
	(0.0137)	(0.0792)
Disaster exposure × Labor-intensive sites(expiring)	-0.1452	-0.1505*
	(0.1017)	(0.0798)
Fixed-effects		
Township	Yes	Yes
Year-month	Yes	Yes
Fit statistics		
Observations	27,206	27,206
R^2	0.64212	0.64205
Within R^2	0.00593	0.00572

Clustered (Township & Year-month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Chapter 3

Anticipation, Commitment Problems and Civil War Violence: Evidence from Normal vs. Flash Droughts

Abstract

Why do actors fail to reach a negotiated outcome and engage in costly conflict? A leading account – bargaining during civil war – posits a commitment problem logic: when actors anticipate shifts in future power, commitments made today are not credible tomorrow, rendering preemptive strikes attractive. However, this logic has been difficult to assess empirically because anticipation is typically conflated with other factors. We develop a credible empirical research design that isolates the causal effect of anticipation by contrasting two climatic shocks with comparable agro-economic impact, but different predictability: anticipated normal droughts and unanticipated flash droughts. Focusing on Sub-Saharan Africa between 1990 and 2020, we find empirical support for the existence of commitment problems: violence increases specifically during the period preceding anticipated normal drought, but not before largely unanticipated flash droughts. The result yields a policy implication: prevention efforts should include the anticipation window, not merely the time after shocks materialize.

Keywords: bargaining during civil war, commitment problem, drought

Introduction

Civil conflict poses a basic paradox: because battle between armed actors is costly, they should prefer negotiated settlements that leave both sides better off. Why then does battle occur even when peace appears jointly preferable? Theories of bargaining during civil war

explain these battles as the result of a bargaining breakdown despite the availability of mutually preferable deals (Fearon, 1995; Walter, 2009). This perspective has become one of the most influential theoretical frameworks for understanding conflict. It sits alongside other prominent explanations, including cognitive-psychological explanations that emphasize misperception and biases (Jervis, 2017), domestic politics accounts that stress leaders' internal constraints and incentives (Mesquita & Smith, 2012; Mesquita et al., 2003), and constructivist perspectives that view conflict as identity-constituting (Wendt, 1992).

A central mechanism within the theory of bargaining during civil war is the commitment problem under anticipated power shifts, as formalized in Fearon (1995) and Powell (2006). When actors anticipate a future shift in relative power, agreements that would settle the dispute today cannot be credibly sustained tomorrow, and anticipating the future power disadvantage, actors may strike preemptively. This logic is not confined to international or civil conflict, and appears whenever parties move early to lock in terms before anticipated power shifts. For example, legislatures accelerate major measures ahead of coalition reshuffles (Baron & Ferejohn, 1989), politicians bargain over cabinet formation under shifting portfolio power (Laver & Shepsle, 1998), and litigants refuse settlement while expecting a more favorable precedent (Priest & Klein, 1984). Taken together, this makes bargaining a general account of the absence of negotiated settlements across politics and law, rather than merely an explanation of conflict.

Despite its theoretical prominence, the commitment problem remains difficult to test empirically: existing research designs struggle to isolate the anticipation itself and often conflate it with other factors, such as gradual economic decline, political instability, or increased militarization (Bell & Wolford, 2015; Bell & Johnson, 2015). An ideal empirical test would be to compare the occurrence of civil conflict before two shocks with the same impact but differing in their predictability.

Early quantitative efforts to identify commitment problems suffer from theoretical mismatch by studying observed power shifts such as economic recession or military defeat (Daxecker, 2011; Lemke, 2003). These measures identify what happens after shocks hit, not whether actors change conflict behavior because they anticipate it. Subsequent em-

pirical efforts improved in two ways: some attempt to model leaders' expectations about future power based on observable factors (Bell & Johnson, 2015; Dong, 2024). However, these measures are constructed from actor-manipulable inputs such as spending, alliances, and military mobilization that respond to looming conflict. Later research uses oil discovery to mark observable pre-shift windows and suggests a comparison between an anticipated shock and no shock (Bell & Wolford, 2015; Carey et al., 2022). However, territories in which oil discovery is possible and economically viable are plausibly different from the territories in which oil discovery is not. Also, companies are less likely to drill for oil in places with intense conflict or where they anticipate intense conflict and instability.

In this paper, we advance the empirical evaluation of commitment problems by leveraging variation in the predictability of two otherwise comparable climatic shocks. Specifically, we use normal droughts, which are slow-onset and become predictable as prolonged shortfalls in rainfall lead to drying soils and worsening crop and vegetation conditions, and flash droughts, which are rapid-onset with little advance notice. Recent drought episodes in Africa illustrate this difference. The 2011 East Africa normal drought unfolded gradually over successive rainy seasons. The October–December 2010 rains were weak and the following March–May 2011 rains also failed, so water shortages and poor pasture conditions built up over many months as each season underperformed (FEWS NET, 2011b; Funk et al., 2010). In contrast, flash drought episodes in the Awash River Basin in Ethiopia developed much more suddenly. Periods of unusually high heat quickly dried out soils and crops, so conditions changed from normal to severely stressed within just a few weeks rather than over multiple seasons (Getahun & Li, 2024).

Both normal and flash droughts are plausibly exogenous climatic shocks with comparable agro-economic impact, but differ in predictability. This contrast allows us to isolate the effect of anticipation in shaping conflict behavior. Theoretically, droughts are conceptualized as an agro-economic shock that can shape the expected power balance by weakening the organizational capacity of armed actors or altering the patterns of civilian cooperation and recruitment that sustain them (Theisen et al., 2011; Von Uexkull, 2014; Von Uexkull & Buhaug, 2021; Von Uexkull et al., 2016). As the prospect of such shifts becomes more predictable, commitments to negotiated settlements become less credible,

increasing incentives for preemptive violence.

Methodologically, we leverage differences in the predictability of two types of drought in a directed dyad-month design that links politically relevant ethnic groups to governments in Sub-Saharan Africa. Using actor-specific spatial measures of drought onset and conflict, we estimate how civil conflict responds in the period preceding drought onset. The design distinguishes between general pre-drought periods (covering both flash and normal droughts) and pre-drought periods in which the gradual onset of normal drought allows armed actors to anticipate the impending shock. If the commitment problem logic holds, civil conflict should increase specifically during pre-onset period of anticipated normal droughts, relative to the pre-onset period preceding largely unanticipated flash drought.

Consistent with the commitment problem mechanism, we find an additional increase in conflict of about 7.7% points when an approaching drought can be anticipated (normal droughts), compared with pre-onset period when a drought is largely unanticipated (flash drought). Accounting for baseline conflict dynamics during pre-drought periods, this anticipation effect corresponds to a net increase in civil conflict of roughly 2.5% points relative to dyad-months without drought exposure, where the baseline probability of civil conflict is 8.56%. We further examine alternative explanations, including the possibility that rising violence reflects competition over anticipated humanitarian assistance or opportunistic attacks facilitated by seasonal mobility conditions, and find that the results remain robust across these tests.

The main contribution of this paper is to offer a more credible empirical test of the commitment problem. By leveraging the contrast in predictability between normal and flash droughts, the design isolates anticipation itself as a driver of civil conflict. On the policy side, evidence that anticipation, not just its realized impacts, increases pre-drought conflict suggests the need to move from reactive crisis response toward forecast-based prevention. Advances in prediction technologies for climatic shocks, such as satellite-informed early-warning platforms and AI-enhanced climate forecasting, make such timing increasingly feasible. Policymakers and international mediators can integrate these tools to identify where anticipated shocks can alter local bargaining power and engage in preventive inter-

ventions. In practice, they can link data from platforms such as the Famine Early Warning Systems Network (FEWS NET), the Climate Hazards Group InfraRed Precipitation with Station data (CHIRPS), and the European Center for Medium-Range Weather Forecasts (ECMWF) with peacekeeping and mediation planning systems. In this way, early warning platforms can evolve from reactive relief tools into proactive instruments of conflict prevention.

Additionally, this paper helps reconcile mixed climate-conflict findings by showing that civil conflict increases before anticipated normal droughts, and provides empirical evidence for the emphasis on anticipation – not merely its impact – of climatic shocks that link to civil conflict (Bas & McLean, 2021; Sawada, 2024). This raises a concern about the parallel trends assumption that underlies the event-study and difference-in-differences approaches used to detect the impact of climatic shocks on conflict. The findings thus highlight the importance of accounting for expectations about future climatic conditions when assessing the conflict implications of climate change, especially as global warming is projected to intensify slow-onset and increasingly predictable hazards such as drought.

Commitment Problem and Preemptive Violence

Bargaining during civil war has become one of the most canonical frameworks for explaining conflict dynamics. Since its initial development, it has organized the study of conflict around a simple but powerful claim: because fighting is costly, violence occurs only when bargaining fails (Fearon, 1995; Powell, 2006; Reiter, 2003; Spaniel, 2024). Within this framework, several mechanisms can prevent mutually preferable agreements. Uncertainty arising from private information and incentives to misrepresent strength or resolve can impede cooperation, while commitment problems may render even attractive settlements unenforceable over time, particularly when actors anticipate future shifts in relative power (Fearon, 1995; Powell, 2006; Spaniel, 2024). Subsequent theoretical efforts have integrated these mechanisms into dynamic accounts that link conflict onset, wartime bargaining, and termination, and that clarified how institutions, information flows, and power trajectories shape the credibility of agreements. This literature has also generated observable implications regarding audience costs, third-party guarantees, and mediation (Powell, 2006;

Reiter, 2003; Spaniel, 2024). More broadly, the bargaining logic extends beyond armed conflict to other domains of distributive politics and law. In legislatures and cabinet formation, outcomes depend on asymmetric information, credibility, and expectations about future electoral strength (Baron & Ferejohn, 1989; Laver & Shepsle, 1998). In legal disputes, parties bargain "in the shadow of the law," with settlement shaped by beliefs about likely judgments and enforceability (Mnookin & Kornhauser, 1978; Priest & Klein, 1984). Similar dynamics have been identified in areas such as trade, regulation, and corporate restructuring (Reiter, 2003).

A central mechanism within bargaining during civil war is commitment problems under anticipated power shifts. This logic proposes anticipation as a driver of civil conflict. When actors anticipate a future power shift, agreements that are acceptable today are not credibly enforceable tomorrow. The side anticipating to lose leverage has incentives to strike first to lock in a more favorable status quo. Formal models show that proactive strikes arise precisely because of concerns about how disadvantageous the future is (Powell, 2004, 2006). This anticipation-driven logic contrasts with the opportunistic violence logic, which follows a realized and unanticipated shift in power. In this case, the advantaged actor exploits a temporary window of superiority, fearing that the opposing side may soon recover and re-establish balance (Fearon, 1995; Kikuta, 2019).

Efforts to provide a credible empirical test for the commitment problem have developed along three strands. While each has moved the agenda forward, they face limitations. Some compare shock and non-shock contexts, where exposure may be endogenous. Others proxy leaders' expectations about future power shifts with observable indicators, risking the conflation of beliefs with strategic behavior.

Early studies of the commitment problem studied the impact of the realized shock. For example, Lemke (2003) uses historical trends in capability scores of states and evaluates the effect of power shift expectations on the probability of war, and Daxecker (2011) examines shocks such as economic recessions or military defeats as triggers of bargaining breakdown. In practice, these designs code the shock itself as the treatment and relate it to violent outcomes, meaning that they do not analyze actors' conflict behavior in the

pre-shock window. As have been indicated by Bell and Johnson (2015), these early studies create a theoretical mismatch: the commitment problem concerns what actors do before a shift, yet realized-shift designs mostly capture violence after these shocks materialize. In effect, they conflate two mechanisms: 1) actors' anticipation and 2) the aftermath impact of the shock once it hits, making actual observed shocks a weak proxy for preemptive motivation.

To address this mismatch, a second wave of research attempts to model leaders' expectations about future power shifts. Bell and Johnson (2015), for example, use observable indicators such as economic potential, international threats, alliance status, and regime types to predict future shifts in the distribution of military capacities. Similarly, Dong (2024) extends this approach by modeling military power as a linear function of observable factors and incorporating uncertainty as a moderator. These efforts provide a more direct measure of actors' anticipation of future power shifts, but still face endogeneity constraints. Indicators such as military spending, alliance formation, and other political choices are likely to increase in the run-up to anticipated conflict regardless of whether commitment problems are present. As a result, observing such changes prior to violence does not provide convincing evidence that anticipations about future power shifts themselves drive conflict. Moreover, as have been indicated by Bell and Johnson (2015), large anticipated shifts that significantly change behavior may never appear in the observational data before they can take effect. This censoring problem further attenuates the observed relationship between expected power shifts and conflict.

A more recent strategy tests the commitment problem by using oil discoveries and subsequent production to identify an observable anticipation window between discovery and extraction (Bell & Wolford, 2015; Carey et al., 2022; Langø et al., 2022). This approach improves on previous work by studying conflict behavior during a constructed anticipation period and finds a link to the onset of civil conflict (Bell & Wolford, 2015), dissidents (Carey et al., 2022), and coup d'état attempts (Langø et al., 2022). However, oil discovery is concentrated in locations where geological prospects and economic conditions make extraction viable, and companies may avoid drilling in areas characterized by intense or anticipated instability. As a result, territories in which oil discovery is possible

and economically viable may not be directly comparable to those without such prospects.

To mitigate these methodological concerns, we develop a design that leverages variation in the predictability of two otherwise comparable climatic shocks. Normal droughts unfold gradually and can be tracked through precipitation and evapotranspiration anomalies, while flash droughts develop rapidly with little lead time. Furthermore, these two droughts are otherwise comparable, as both depress soil moisture, reduce yields and rural incomes, and thus cause a negative agro-economic shock that shifts future power toward the less-affected side. By comparing periods preceding two shocks with similar material consequences but different predictability, the design identifies whether civil conflict increase is driven by actors' anticipation of future power shifts. Additionally, the shocks – normal and flash droughts – are not chosen by armed actors. Unlike modeling leaders' expectations about the future power shift based on actor-manipulable and observable factors, climatic shocks avoid incorporating strategic behavior into the anticipation measure.

Droughts-Power Shift Mechanism

Drought is conceptualized as an agro-economic shock that can shape the expected balance of power between governments and rebel groups during the civil war. By reducing rainfall and increasing evaporation, drought lowers crop yields, degrades pastures, and intensifies local water scarcity. These pressures influence armed actors through altering resource availability, recruitment opportunities, and patterns of civilian cooperation. Importantly, because the impacts of drought are distributed differently across territories, they can advantage some armed actors while constraining others. Anticipated shifts in relative power may thus undermine the credibility of negotiated settlements and heighten incentives for preemptive violence.

First, drought can influence organizational capacity by shrinking agricultural resources that finance armed actors. Rainfall shortfalls reduce crop output, livestock holdings, and rural trade, thus tightening fiscal constraints and raising the costs of provisioning the armed forces. Governments may face declining tax revenues and higher food procurement costs, while rebel groups can experience similar constraints when drought hits the areas

from which they extract resources (Von Uexkull, 2014; Von Uexkull et al., 2016). In Africa, this mechanism is particularly relevant because both governments and rebels depend, at least in part, on agrarian economies for revenue and logistics. For example, in West and East Africa, there are documented cases in which both sides rely on agrarian revenue – government via state crop and export taxation (such as Ghana's Cocoa Board monopoly system or Côte d'Ivoire's cocoa export tax), and rebel via checkpoint levies on grain, livestock, zakat on herds, and fees on rural markets (Schouten & Barnett, 2025). In Somalia's livestock-export corridors, drought years have sharply reduced livestock exports. World Bank estimates suggest a roughly 400 million loss. Similarly, because al-Shabaab (a rebel group in Somalia) systematically taxes corridor trade and livestock flows, the same contraction also narrows its takings from herd and road taxation (World Bank, 2020).

Second, drought can reshape civilian cooperation and recruitment. By reducing crop yields, food access, and rural income, drought undermines the economic viability of climate-sensitive livelihoods, particularly in rainfed agricultural regions (Von Uexkull, 2014). As legal earning opportunities deteriorate, the relative attraction of joining armed organizations may increase, lowering the opportunity cost of participation in violence, which may benefit whichever side is better positioned to exploit it (Von Uexkull, 2014; Von Uexkull et al., 2016). At the same time, food insecurity and perceived failures in relief provision can intensify civilians' grievances against authorities, whether governmental or insurgent, thus affecting patterns of local legitimacy and mobilization (Von Uexkull, 2014; Von Uexkull et al., 2016).

From a perspective of bargaining during civil war, the implication is that drought can generate anticipated changes in the power balance regardless of which armed actor benefits. When actors expect their future bargaining position to worsen, commitments to negotiated settlements become less credible. Under such conditions, incentives for preemptive violence increase, as actors may prefer to fight before the expected shift materializes and constrains their strategic options.

Normal vs. Flash Droughts

Normal and flash droughts serve as an ideal example for distinguishing anticipated from unanticipated shocks. As suggested above, both types of droughts are negative agro-economic shocks that shift the future power balance (Maconga, 2023; Von Uexkull et al., 2016). However, not every drought is the same: some emerge slowly and predictably, while others strike suddenly with little warning. This variation in predictability offers an example of different types of shocks and thus serves as a natural empirical test for commitment problems.

Normal droughts typically develop over months due to sustained reduction of precipitation or gradual increases in evapotranspiration, leading to water scarcity and possible agricultural failure. These patterns are not only visible through satellite or seasonal forecasts, but also recognizable by actors on the ground through some environmental cues such as drying vegetation, declining river levels, or disrupted planting cycles (Ford & Labosier, 2017; Iglesias et al., 2022; Kreibich et al., 2022). This makes these droughts anticipated even for local actors with limited access to formal meteorological data and early warning systems. As such, they may create conditions for strategic adaptation, including preemptive violence. A rebel group may attack before the drought weakens their food base. For example, in Somalia in 2016, seasonal outlooks forecast a drought months in advance. During this anticipation window, clashes between the Federal government and al-Shabaab are observed to be concentrated along road corridors as both sides repositioned before the scarcity (ACAPS, 2017; International Crisis Group, 2017; United Nations Security Council, 2016). Similarly, in Mozambique in 2015-2016, the El Niño-related drought was widely forecast. In anticipation of expected rainfall shortfalls, the government convoyed drought-response logistics, producing confrontations with Resistência Nacional Moçambicana (RENAMO) along key routes consistent with strategic moves ahead of future shocks (FEWS NET, 2016; Hanlon, 2016a, 2016b, 2016c).

Unlike normal droughts, which develop gradually and can be anticipated from sustained precipitation deficits or soil-moisture decline, flash droughts are defined by their rapid onset and intensification, leaving little time for actors to prepare (Christian et al., 2021;

Osman et al., 2021, 2024; Otkin et al., 2018). This sudden occurrence makes them an unanticipated shock. While they may provoke opportunistic violence, their unpredictability rules out preemptive action. Technically, flash droughts occur after a spike in temperature, low humidity, and intense evapotranspiration, causing soil moisture to collapse, often within a few weeks (Christian et al., 2024; Osman et al., 2024; Otkin et al., 2021). What makes flash droughts especially difficult to predict is that they can emerge after short wet periods, giving a misleading impression of stable or improving conditions. In these cases, average or above-average rainfall cannot offset rapid drying caused by high temperatures, low humidity, and intense solar radiation. This disconnect between precipitation and surface moisture loss makes flash droughts hard to anticipate both for formal monitoring systems and local actors that rely on environmental signals (Christian et al., 2024; Otkin et al., 2021; Qi et al., 2025).

An illustration of different unpredictability but comparable agroeconomic impact can be observed by comparing two drought episodes in Africa. The 2010–2011 East Africa drought developed gradually across consecutive rainy seasons and was closely linked to persistent La Niña conditions. Seasonal monitoring systems and dynamical forecast models were able to anticipate rainfall failures in advance, making the event amenable to early warning and seasonal preparedness (Dutra et al., 2012, 2013). Despite this slower and more predictable onset, the agricultural consequences were severe. FEWS NET reported likely crop failure in marginal crop areas and documented staple cereal prices approaching or exceeding record levels in affected markets (FEWS NET, 2011a, 2011b). At the national level, FAO GIEWS reported that Kenya's 2011 maize production was officially estimated at 2.25 million tonnes, approximately 18 percent below the previous year due to dry weather, reflecting significant production shortfalls during the drought year (FAO GIEWS, 2011). These agricultural losses translated into market stress, with staple food prices rising sharply across the region.

In contrast, in South Africa, a "high-intensity, rapid-onset flash drought" intensified from mid-January to late-March 2024 during the critical maize growth period (Malherbe et al., 2025). Unlike the East Africa case, early-season rainfall initially created expectations of a normal harvest, but extreme heat and rapid soil-moisture depletion sharply curtailed

the effective response window for farmers and planners. The shock thus materialized within weeks rather than months, limiting the scope for anticipatory adjustment. The agricultural consequences were similarly substantial. Official estimates reported sharp seasonal production losses, with the Crop Estimates Committee placing 2023/24 summer grains and oilseeds at 15.45 million tonnes (23% decline year-on-year) and maize at 12.80 million tonnes, while USDA FAS also reported major maize losses, including a 28% decline in white maize production (Sihlobo, 2024; USDA Foreign Agricultural Service, 2024). Taken together, these cases support the key assumption underlying this study: while normal and flash droughts differ in predictability, both can generate large, direct losses in agricultural production with associated market stress and food-security risk.

To further support the distinction between normal and flash droughts empirically, we examine how rainfall and vegetation conditions change around the onset of each drought type (see more in Appendix C). The Standardized Precipitation–Evapotranspiration Index (SPEI) captures rainfall shortages adjusted for temperature, while the Normalized Difference Vegetation Index (NDVI) measures vegetation greenness and is a proxy for agricultural damage. Figure 3.1 plots the average change in these indicators relative to the drought onset month (t=0) across all grid cells. Normal droughts show a clear and gradual decline in both rainfall and vegetation greenness in the months leading up to onset, suggesting that worsening conditions are visible in advance. Flash droughts, in contrast, show little change before onset but a sharp drop at or immediately after onset. Despite these differences in timing, post-onset vegetation losses are similar across the two drought types. This pattern indicates that normal and flash droughts have comparable agricultural impacts but differ in how predictable they are.

As an additional empirical test of the material comparability of the two types of droughts, we examine their effects on local staple food prices at the subnational level (ADM1) in Sub-Saharan Africa (see more in Appendix D). Using monthly price data from the Famine Early Warning Systems Network (FEWS NET), we construct an index based on the log prices of major staple crops in Africa, including white maize, red sorghum, millet, cassava, and mixed beans. The results show that both normal and flash droughts are associated with statistically significant increases in local food prices, with a similar magnitude, indicating

comparable impacts on increasing price of agricultural production.

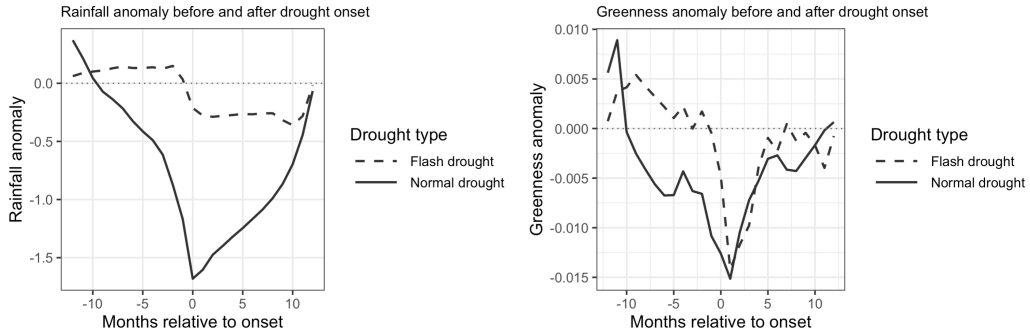

Figure 3.1: Rainfall and Vegetation Dynamics Around Normal and Flash Drought Onsets

Note: The figure shows average changes in SPEI and NDVI relative to the drought onset month ($t = 0$). Lines represent averages across all grid cells and drought events of each type.

Research Design

To identify the effect of anticipation about future power shifts on conflict, we leverage differences in the predictability of two otherwise comparable drought shocks - normal and flash droughts. We distinguish between periods preceding any droughts (including both flash and normal droughts) and pre-onset periods in which a gradual normal drought allows actors to anticipate the impending shock. The empirical analysis is conducted at the dyad level between politically relevant ethnic groups and states in Sub-Saharan Africa between 1990 and 2020.

We focus on Sub-Saharan Africa due to its frequent exposure to droughts and sustained patterns of armed conflict. In many African countries, armed actors depend heavily on rainfed agriculture for financing and sustaining their operations. Governments rely on crop boards, export taxes, and rural revenues, while insurgents extract resources through levies on harvests, market trade, and livestock. This reliance makes droughts an immediate signal of shifting their power bases and thus relative bargaining power. Droughts have become more frequent and severe in recent decades, with projections indicating continued exposure to both normal and flash droughts (IPCC, 2023; Mahlalela et al., 2020). The geographical distribution of both types of droughts is shown in Figure 3.2.

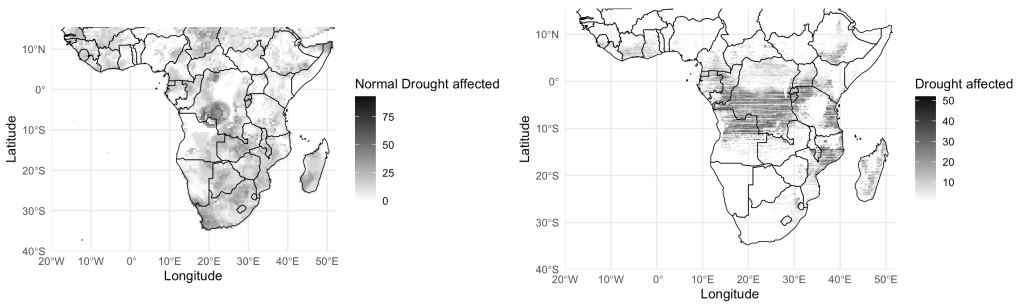

Figure 3.2: The Geographical Distribution of Both Types of Droughts in Sub-Saharan Africa

Note: The maps show the total number of drought occurrences in each grid cell over the study period. The left panel shows normal droughts measured at a 0.5° grid resolution, while the right panel shows flash droughts measured at a 0.1° grid resolution. Shading reflects how often each location experienced a drought, with darker areas indicating more frequent exposure.

Unit of analysis

This study uses the dyad-month as the unit of analysis, where each dyad consists of a politically relevant ethnic group and the government of the state in which the group resides. Politically relevant ethnic groups are identified using the Ethnic Power Relations (EPR) dataset. A group is classified as politically relevant if "either at least one significant political actor claims to represent the interests of that group in the national political arena" or if "group members are systematically and intentionally discriminated against in the domain of public politics" (Vogt et al., 2015, p. 1331). We adopt the ethnic group–government dyad rather than the rebel group–government dyad directly because spatial data on the location and activity areas of rebel groups is not available across countries and over time. In contrast, the EPR dataset provides spatially referenced information on politically relevant ethnic groups, which allows us to link droughts to the populations from which armed actors typically draw support and resources. Groups that are not politically relevant are excluded from the sample. In addition, we exclude dyads in periods in which the ethnic group is classified as dominant or holds a monopoly over governmental power, as such groups form part of the ruling coalition and cannot function as non-state opposition actors.

Outcomes

To measure civil conflict at a dyad level, we link each politically relevant ethnic group to one or more rebel groups using the ACD2EPR 2021 dataset, which connects UCDP conflict actors (from ACD version 20.1) to ethnic groups in the EPR-Core 2021 dataset

(Wucherpfennig et al., 2012). A rebel group is considered linked to an ethnic group if either 1) it explicitly claims to represent the group or 2) recruits fighters from it. Rebel groups without such a link are excluded from that group's dyad. This enables us to aggregate violent events – originally recorded between individual rebel groups and the government up to a dyad level between the ethnic group and the government, a method used by Von Uexkull et al. (2016).

The main dependent variable is the occurrence of civil conflict, drawn from the UCDP Georeferenced Event Dataset (GED). We include only events classified as state-based armed conflict, defined as "a contested incompatibility concerning government and/or territory where the use of armed force occurs between two parties, at least one of which is the government of a state" (Davies et al., 2025). A dyad is coded as experiencing violent events if any such events occur between the government and a rebel group linked to that ethnic group, and zero, otherwise.

Normal drought

This study leverages differences in the predictability of two otherwise comparable climatic shocks - normal and flash droughts. Normal droughts are identified using the Standardized Precipitation-Evapotranspiration Index (SPEI) – a widely used drought indicator that accounts for both precipitation deficits and atmospheric demand (evapotranspiration). Unlike the simpler Standardized Precipitation Index (SPI), SPEI accounts for temperature effects, making it better suited to capture drought severity under climate change, especially important in the African context where warming trends intensify water stress (Beguería et al., 2014; Vicente-Serrano et al., 2010a).

Thresholds for interpreting SPEI values are conventionally defined as moderate ($-1 \leq$ SPEI < -1.5), severe ($-1.5 \leq$ SPEI < -2), and extreme dry conditions (SPEI ≤ -2). We define the onset of a drought episode as SPEI ≤ -2, identifying rare and highly persistent moisture deficits most likely to cause major agricultural and livelihood shocks (Beguería et al., 2014; Vicente-Serrano et al., 2010b). This threshold also corresponds to conditions that are publicly visible and widely recognized as true drought crises, which can plausibly trigger political and social crisis (Birkland, 1998; Lesk et al., 2016; Liberato

et al., 2021). The twelve months preceding such an onset are defined as the anticipation window. This window captures the gradual accumulation of water deficits that increasingly signal impending crisis to local actors through worsening harvests, declining rainfall, and deteriorating vegetation conditions. As robustness checks, we also estimate models using drought episodes as SPEI ≤ -1.5 (see more in Appendix K).

Because we use twelve-month Standardized Precipitation–Evapotranspiration Index (SPEI-12), which captures cumulative moisture conditions over the preceding year, we allow short interruptions of less than twelve months to be bridged and treated as part of the same drought sequence rather than separate events. This approach recognizes that transient recoveries in precipitation do not necessarily alleviate long-term soil moisture deficits or their economic and social consequences. We vary the bridging length between 0 and 12 months to confirm that the results are not sensitive to this assumption in the robustness checks (see more in Appendix J).

Drought exposure is spatially merged with the government or potentially rebel-influenced territories and is coded at the dyad–month level. The rebel-influenced territories are defined by the polygon(s) of the politically relevant excluded groups that are not dominant and do not have a monopoly in the EPR–Geo dataset. The government-influenced territories include (1) polygons of politically relevant ethnic groups coded as dominant or a monopoly in the EPR dataset, and (2) "white areas" representing politically irrelevant ethnic groups. The actor is coded as affected if at least 10% of its territory is under severe drought in a given month. The 10% threshold ensures that droughts meaningfully affect the actor's area of control, avoiding false positives due to minor or border-only impacts. For robustness, we further test the impact using different thresholds from 5% to 20% in 1% increments. The resulting normal drought onset indicator equals 1 if a new drought period, meeting these spatial and temporal criteria, begins for a dyad in that month, and 0 otherwise (see more in Appendix H).

Flash drought

Flash droughts are identified using an adaptation of the Soil Moisture Volatility Index (SMVI), a method developed to detect the rapid onset and intensification of drought

conditions (Osman et al., 2021, 2024). Unlike normal droughts, which evolve gradually, flash droughts are quick burn — within weeks, a very hot, windy, and dry-air spell (high evaporation-pressure deficit) makes plants use water much faster, and thus root-zone soil moisture plunges even if precipitation has not yet been far below normal. This quick development leaves little time for strategic adaptation. The SMVI captures this by detecting sudden drops in root-zone soil moisture (RZSM), which reflects water availability for plants and is closely tied to agricultural and livelihood impacts.

In practical terms, a flash drought begins when the short-term average soil moisture (over 5 days) falls sharply below the longer-term baseline (over 20 days) and remains so for at least four consecutive 5-day periods (about 20 days in total). At the same time, soil moisture levels must fall below the climatological 20th percentile for that location and time of year, indicating unusually dry conditions compared to historical norms. The severity of each flash drought is calculated as the cumulative shortfall from this percentile threshold and converted into a standardized score (z-score) to allow comparison across locations. In the main analysis, we include only severe or extreme flash droughts ($z \geq 0.5$). While the original SMVI was developed using U.S. and global datasets, we adapt it here for the African context by using Famine Early Warning Systems Network Land Data Assimilation System (FLDAS) soil moisture data. FLDAS provides 0.1° (10 km) resolution monthly estimates of root-zone soil moisture, calibrated for African hydrological and agroecological conditions. This ensures that the flash droughts identified reflect local climate dynamics rather than assumptions from non-comparable regions.

As with normal droughts, flash drought exposure is coded at the dyad–month level. Flash droughts are considered relevant to a dyad only if at least 10% of the territory influenced by the rebel group or the government is affected. Short interruptions in flash drought periods are bridged if they last less than one month, consistent with the typical duration of flash drought development (2–4 weeks).

Finally, to ensure a clear analytical distinction between anticipated and unanticipated shocks, we test the temporal dependence between the onsets of normal and flash drought, and the results suggest no statistical concern (see more in Appendix L). We also study

the correlation between units coded as normal and flash droughts. Only about 1.13% of dyad-months experience both simultaneously among all dyad-time periods. More specifically, while about 5.2% of normal drought months are also flash drought months, around 25% flash droughts happen during ongoing normal droughts. This suggests that these two types of droughts are distinct. This overlap is also consistent with Otkin et al. (2018) and Christian et al. (2024)'s definition that flash droughts can either mark the onset of a broader drought episode or represent a period of rapid intensification within an existing drought. To focus solely on the former type, we recode flash drought months that coincide with ongoing normal drought to zero, thus excluding embedded rapid-intensification episodes and retaining only stand-alone flash drought events.

Model specification

We estimate a window-based specification that exploits variation in the predictability of drought onset. The design compares dyad-months in the twelve months preceding anticipated normal droughts to dyad-months preceding largely unanticipated flash droughts, as well as to periods without impending drought exposure. To implement this treatment–comparison framework, we construct two pre-onset indicators: one capturing the twelve-month period before any drought onset (including both flash and normal droughts), and a second capturing the additional anticipation window specific to gradual normal droughts. The coefficient on the general pre-drought indicator thus estimates the effect of impending but largely unanticipated flash drought relative to drought-free periods, while the coefficient on the normal-drought pre-onset indicator captures the additional effect of anticipation associated with predictable drought onset. To examine whether civil conflict also differs during and after drought onset, we further include indicators for the month of drought onset and for the twelve-month post-onset period, each defined separately for any drought and for normal drought. The model includes dyad and time fixed effects, and standard errors are clustered accordingly. The model specification is as follows:

$$
\begin{aligned}
\text{Violence}_{it} = &+ \beta_1 \,\text{Onset(Any Drought)}_{it} + \beta_2 \,\text{Onset(Normal Drought)}_{it} \\
&+ \beta_3 \,\text{PostWindow(Any Drought)}_{it} + \beta_4 \,\text{PostWindow(Normal Drought)}_{it} \\
&+ \ \beta_5 \,\text{PreWindow(Any Drought)}_{it} + \beta_6 \,\text{PreWindow(Normal Drought)}_{it} \\
&+ \alpha_i + \gamma_t + \varepsilon_{it}.
\end{aligned} \tag{3.1}
$$

Violence_{it} denotes the occurrence of civil conflict in dyad i during month t. $\text{Onset(Any Drought)}_{it}$ and $\text{Onset(Normal Droughts)}_{it}$ indicate whether any or normal drought start in month t. $\text{PostWindow(Any Drought)}_{it}$ and $\text{PostWindow(Normal Drought)}_{it}$ are indicators for whether any or normal drought occurred within twelve months after month t. $\text{PreWindow(Any Drought)}_{it}$ and $\text{PreWindow(Normal Drought)}_{it}$ are indicators for whether any or normal drought occurred within the twelve months prior to month t. α_i and γ_t are dyad and time fixed effects, respectively, and ε_{it} is the error term. If the anticipation of a future shock generates preemptive violence, the effect of the pre-onset period of normal drought (β_6) should be positive and statistically significant, indicating an additional effect of anticipation.

Additionally, because a dyad can experience multiple droughts, this setting introduces the risk of bias from "forbidden comparisons"- cases where already-treated units are mistakenly used as controls for later-treated units, thus producing invalid counterfactuals (de Chaisemartin & D'Haultfoeuille, 2025; de Chaisemartin et al., 2024). In the drought context, treatment effects may also exhibit persistence: droughts can have carryover effects that require a recovery period before outcomes return to baseline. Consequently, periods coded as just below the drought threshold may still experience residual agricultural shocks. Moreover, anticipation and aftermath windows may overlap when a new drought occurs before the impact of the previous one has fully dissipated. To address these concerns, we adopt a staggered-adoption design with a rolling treatment window. Specifically, we anchor treatment on each dyad's first drought event and drop all periods following any subsequent droughts and dyads with an incomplete post-window. We also drop the dyad that experienced droughts twelve months before treatment to avoid contamination from earlier droughts.

As a robustness check, we also estimate a specification that separately models the anticip-

ation effects for normal and flash droughts. This comparison evaluates whether violence increases specifically before normal droughts rather than flash drought, serving as a placebo (see more in Appendix E).

Results

Overall, the empirical findings provide evidence of the existence of the commitment problem. Civil conflict is more likely before the anticipated normal drought, relative to the period before unanticipated flash drought.

Consistent with the commitment problem, civil conflict is driven by the anticipation of normal droughts. As shown in Figure 3.3, being in the pre-onset period of an anticipated normal drought is associated with a statistically significant increase in the probability of civil conflicts of approximately 7.7% points ($p < 0.05$). This effect captures the incremental anticipation effect relative to pre-onset periods preceding largely unanticipated flash droughts, indicating that anticipation of impending shocks increases preemptive violence. Taking into account baseline conflict effect during pre-onset period of any droughts, this anticipation effect translates into a net increase in civil conflict of roughly 2.5% points compared to an 8.56% baseline dyad-months without drought exposure.

In contrast, the coefficient for the pre-onset period of any drought, which primarily reflects impending but unanticipated flash drought conditions, is negative and statistically insignificant. Substantively, the estimate is small and in the opposite direction, indicating no evidence that civil conflict rises during pre-drought periods when shocks are not anticipated. Similarly, we find no evidence that civil conflict rises during and after drought onsets. Taken together, these results provide supporting evidence for the commitment problem. The differential timing of civil conflict - before anticipated power shifts but not unanticipated ones - is consistent with a commitment problem in which actors use violence to lock in relative advantage before their bargaining power declines.

These findings are reinforced by alternative specifications reported in the Appendix E that model the pre-onset period of flash drought as a placebo, separately from normal droughts.

We find a rise in civil conflict only in the pre-onset period of anticipated normal droughts, whereas pre-onset periods of flash droughts show no statistically significant increase in civil conflict.

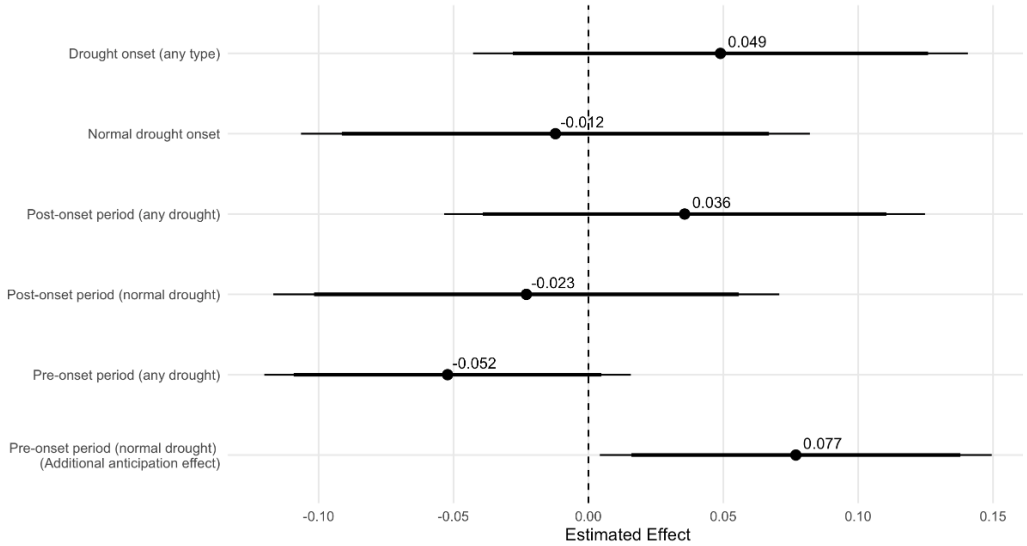

Figure 3.3: Conflict Effects of Drought Onset, Post-Onset Window, and Anticipation

Note: Coefficients are estimated from two-way fixed effects models of monthly conflict events that isolates the anticipation mechanism. The onset indicators capture the occurrence of any drought or normal drought event in the specific month. The pre-onset period indicators equals one if a drought or a normal drought is expected to occur within the subsequent 12 months, and the post-onset period for any droughts equals one if a drought occurred within the previous 12 months. In addition, the specification includes a pre-onset period for normal droughts indicator, which isolates the anticipation effect associated with anticipated normal droughts. Points denote coefficient estimates. Thick horizontal bars represent 90% confidence intervals and thin bars represent 95% confidence intervals.

Robustness

Some robustness checks are conducted to rule out other alternative explanations for the increase in civil conflict before normal droughts, but not flash droughts, and thus better isolate anticipation as the main driver.

One alternative explanation to the anticipation mechanism is the pre-positioning of humanitarian aid ahead of normal but not flash droughts (see more in Appendix F). The literature on resource predation suggests that aid can increase violence by creating opportunities for looting or by making aid convoys and storage facilities attractive targets for armed groups (Crost et al., 2014; Nunn & Qian, 2014). Thus, increasing pre-drought vi-

olence may instead reflect opportunistic attacks on aid resources, which could themselves be an expression of strategic anticipation.

To empirically test this alternative, we use attacks on aid workers as a placebo outcome. If pre-drought violence were mainly driven by aid-related targeting rather than the strategic anticipation, we would expect an increase in attacks on aid personnel before normal droughts. We test this using data from the Aid Worker Security Database (AWSD), aggregated to the state-month level and merged with our dyad-level dataset. The results show no statistically significant impact on the placebo outcome.

A second potential alternative explanation concerns dry-season mobility, which may facilitate military movement before normal droughts (see more in Appendix G). In the run-up to normal droughts, extended dryness can improve road conditions, reducing the cost of moving fighters and enabling opportunistic offensives, and serving as an opportunity-based mechanism independent of anticipation. To address this, we include road density data from Müller-Crepon et al. (2021) and interact road density at the state level with the leads of normal droughts. A mobility-based explanation predicts stronger pre-onset effects where road density is higher. Similar effects across low- and high-road-density areas would suggest mobility is not the main driver. The results show no statistically significant impact for the interaction term.

Finally, a series of robustness checks confirms that the main findings are not artifacts of modeling or measurement choices. We adjust the drought exposure threshold (5–20%) to test sensitivity to coverage cutoffs (see more in Appendix H), and vary the temporal aggregation window of the event study (three to twelve months) to ensure results are not driven by period definition (see more in Appendix I). We also employ alternative bridging rules to verify that results are not tied to a specific design choice (see more in Appendix J), and use a different threshold (SPEI ≤ -1.5) to define normal droughts (see more in Appendix K). Across these tests, coefficient signs remain stable, indicating that the observed anticipatory effect is robust to alternative specifications.

Conclusion

This paper revisits commitment problems within the theory of bargaining during civil war: when actors expect to lose relative power, they may strike first rather than accept worse terms later. In existing empirical efforts to test this theory, credible identification strategies remain elusive. The earliest effort by Lemke (2003) uses realized shocks (recessions, defeats, territorial loss) as proxies for expectations and studies their impact on future conflict behavior, which identifies effects when the shock hits, not because actors anticipate it. Subsequent empirical efforts aim for a better causal identification by either providing forecast-based measures that build direct predictions of future power (Bell & Johnson, 2015; Dong, 2024) or using a windfall design that uses oil discoveries to mark an observable pre-shift window (Bell & Wolford, 2015; Carey et al., 2022). However, both suffer from the endogeneity constraint. The forecast measure builds forecast indices of future power from spending, alliances, and mobilization, and these factors are actor-manipulable and often move precisely as conflict looms. The windfall design uses oil discoveries and production to mark a pre-shock window, but the comparison still collapses to units headed into a shift vs. those that are not. Across approaches, anticipation is polluted with co-moving economic and political factors, leaving identification elusive.

We develop a new research design that isolates the anticipation effect on civil conflict by exploiting differences in the predictability of two comparable droughts. Specifically, we use normal droughts, which are slow-onset and anticipated, and flash droughts, which are rapid-onset with little advance notice. Both are exogenous climatic shocks and are materially comparable, but they differ in predictability. Consistent with the commitment problem, we find that civil conflict increases during the pre-onset period of normal droughts, indicating an additional anticipation effect, relative to the effect during the pre-onset period of unanticipated flash drought.

As with other subnational studies, it is important to consider how evidence from one region can inform broader civil-war contexts (Blattman & Miguel, 2010). Although this paper focuses on Africa, the contribution is not limited to drought or to this region. Rather, drought provides an empirical setting in which anticipated shifts in the balance of power

can be observed and identified. Similar dynamics may arise in other contexts where actors foresee changes in relative capabilities driven by predictable shocks. Such dynamics are found not only in agrarian regions in South and Southeast Asia (e.g., Myanmar's Dry Zone, northeast India, Nepal's Terai), the Middle East (e.g., Yemen's highlands, Syria's wheat belt), and parts of Latin America (e.g., Colombia's agricultural zones), but also in environments shaped by other predictable disruptions. Anticipated power shifts may similarly emerge in response to forthcoming policy reforms, electoral transitions, resource discoveries or depletion, infrastructure expansion, or the gradual onset of other environmental hazards. In such settings, the contrast between anticipated and unanticipated shocks can generate incentives for preemptive violence analogous to those studied here.

Finally, existing theoretical work on commitment problems highlights the importance of conditioning factors that can exacerbate or mitigate preemptive incentives. Preemptive violence should be most likely when the anticipated shift is large and rapid, and when credible guarantees are weak (Powell, 2006). Institutions that share power or enable third-party monitoring can mitigate these risks (Fortna, 2008; Hartzell & Hoddie, 2003). Also, drought–conflict research shows that drought effects concentrate where agricultural dependence is high and political exclusion is severe, and attenuate where adaptation capacity, such as irrigation, market access, or humanitarian reach, is stronger (Buhaug, 2010; Cederman et al., 2011; Von Uexkull et al., 2016). A natural next step is to interact the lead time with these moderators, varying exposure asymmetry, and the magnitude of anticipated power shifts. The findings also carry implications for conflict prevention policy. If anticipated shocks alter local bargaining power before their material impacts fully unfold, then advances in climatic prediction technologies create opportunities for earlier and more targeted intervention. Forecast-based approaches could incorporate information from satellite-informed early-warning platforms, such as FEWS NET, CHIRPS precipitation monitoring, and ECMWF seasonal forecasts, into peacekeeping deployment decisions, mediation planning, and humanitarian risk assessments. In this way, predictive climate information can help shift policy responses from reactive crisis management toward proactive conflict prevention, particularly in regions where livelihoods and armed mobilization remain closely tied to climate-sensitive economic activities.

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Appendix

1. Main models
    A. Main event study model with aggregated lead and lag windows
    B. Event study model with twelve lags and leads
    C. Test of differential predictability and comparable impacts across two types of droughts: NDVI and SPEI
    D. Test of similar agricultural impacts across two droughts: staple food prices
    E. Placebo design using flash drought exposure as the separate variables
2. Alternative theoretical explanations
    F. The rapacity mechanism over the pre-positioned aid
    G. The opportunistic attack due to dry-season mobility
3. To address the concern about the result sensitivity to the design choices
    H. Using different threshold for coding the drought exposure
    I. Using different lengths of anticipation and post windows
    J. Using different bridging rules for consecutive droughts
    K. Using different thresholds to define normal droughts
    L. Testing temporal dependence between onsets of flash and normal drought

Appendix A: Main event study model with aggregated lead and lag windows

Table 3.A1: The Effect of Droughts on Civil Conflicts (Main Model)

Dependent Variable:	Civil Conflicts
Model:	(1)
Variables	
Onset (any drought)	0.0490
	(0.0468)
Onset (normal drought)	-0.0122
	(0.0481)
Post-onset (any drought)	0.0357
	(0.0455)
Post-onset (normal drought)	-0.0230
	(0.0478)
Pre-onset (any drought)	-0.0522
	(0.0347)
Pre-onset (normal drought)	0.0769**
	(0.0371)
Fixed-effects	
Dyad	Yes
Month	Yes
Fit statistics	
Observations	11,395
R^2	0.53831
Within R^2	0.00208

Clustered (Dyad & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix B: Event study model with twelve lags and leads

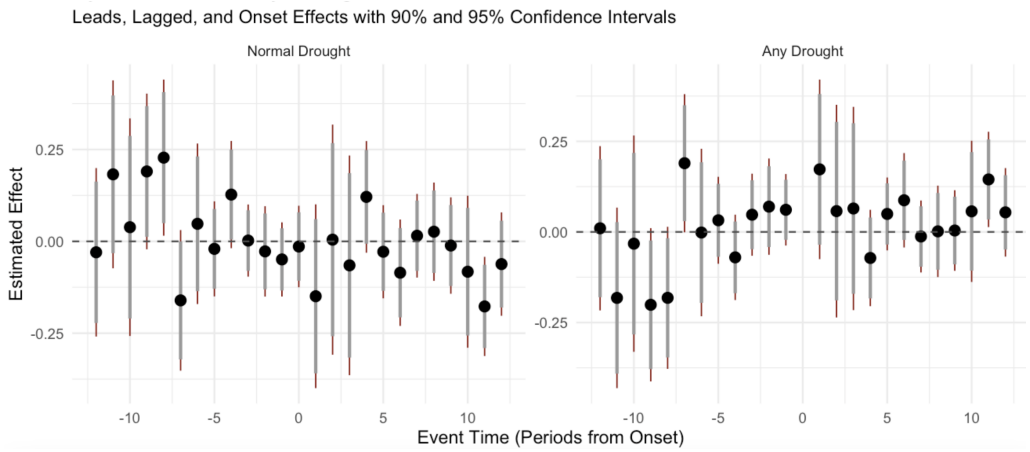

Figure 3.A1: A Dynamics Model with Twelve Leads and Lags

Note: This figure presents the dynamic effects of normal and flash droughts on violence from an event-study model including twelve leads and twelve lags relative to drought onset. Each point represents the estimated coefficient for a given period before or after the onset of drought, with vertical bars indicating 90% and 95% confidence intervals.

Appendix C: Test of differential predictability and comparable impacts across two types of droughts: NDVI and SPEI

To empirically support that flash and normal droughts show similar agricultural impacts but differ in their hydroclimatic predictability, we conduct a robustness test using vegetation data and monthly water-balance climate data. Vegetation stress is measured using Normalized Difference Vegetation Index (NDVI) derived from the Visible Infrared Imaging Radiometer Suite (VIIRS), which provides monthly surface greenness estimates. Hydroclimatic conditions are measured with the Standardized Precipitation–Evapotranspiration Index (SPEI), derived from monthly precipitation and potential evapotranspiration (PET), capturing the water-balance processes leading to drought formation.

For each grid cell (at 0.5 × 0.5 resolution), we identify the month when either a flash drought or a normal drought begins. To ensure comparability across time and space, we compute cell-specific monthly climatologies for both NDVI and SPEI over the full study period and convert all observations into anomalies by subtracting the local long-term monthly mean. We then re-align each cell's anomaly time series relative to its onset date using a relative month index spanning from –12 months (pre-onset) to +12 months (post-onset). Averaging across all events generates onset-centered anomaly trend for flash and normal droughts, separately for NDVI and SPEI.

Using these onset-centered anomalies, Figure A2 and A3 summarize how vegetation conditions (NDVI) and hydroclimatic stress (SPEI) evolve before and after drought onset for both flash and normal droughts. The NDVI trend shows that the two drought types produce very similar declines in vegetation greenness following onset, suggesting comparable agricultural impact. By contrast, the SPEI trending reveals a marked difference in pre-onset hydroclimatic behavior: normal droughts exhibit a gradual and sustained water deficit in the months leading up to onset, whereas flash droughts show little to no drying signal prior to the rapid decline at onset. Thus, the figure visually shows that flash and normal droughts share a similar impact but differ in their predictability.

To further support these visual patterns, we compare the 12-month NDVI and SPEI

changes around onset using Welch's t-tests. The results show that NDVI impacts are statistically indistinguishable across drought types (t = 0.40, $p = 0.69$), with mean NDVI declines of –0.0013 (flash) and –0.0019 (normal). This confirms that both flash and normal droughts impose similar agricultural stress. In contrast, SPEI changes differ strongly between the two drought types (t = 9.66, $p < 0.01$). Normal droughts experience larger cumulative SPEI declines (–0.50) than flash droughts (–0.08), indicating that normal droughts are preceded by gradual, predictable hydroclimatic drying, while flash droughts emerge with more sudden hydroclimatic shifts. This asymmetry is also visible in the onset-centered SPEI figure: normal droughts show a distinct downward trend well before onset, whereas flash droughts do not.

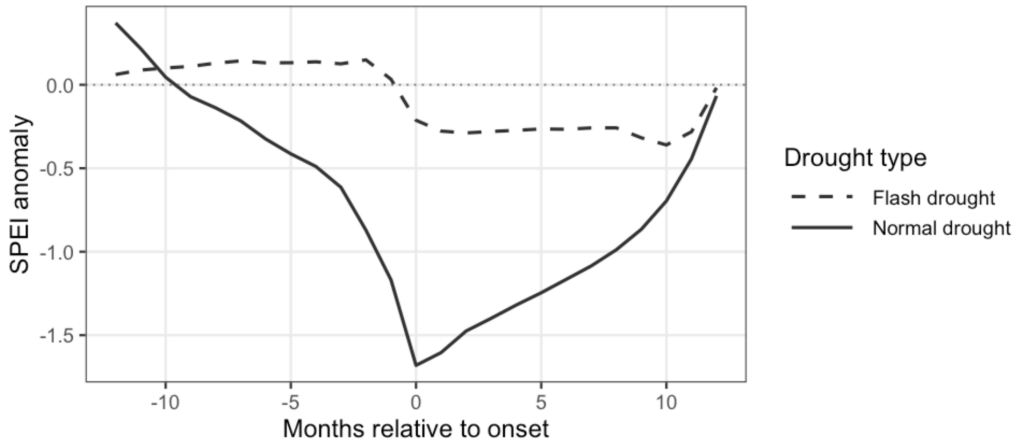

Figure 3.A2: SPEI Anomaly Trending Before and After Flash and Normal Drought Onset

Note: This figure plots the average 12-month trending of the SPEI around the onset of flash and normal droughts. SPEI anomalies are computed as deviations from the long-run monthly mean within each grid cell. A lower anomaly reflects increasingly dry conditions. Solid lines represent normal droughts, while dashed lines represent flash droughts.

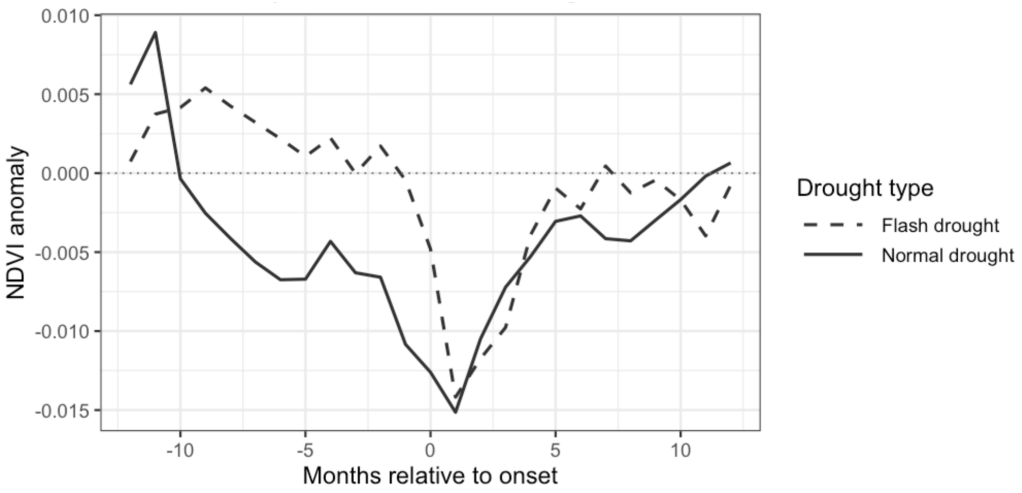

Figure 3.A3: NDVI Anomaly Before and After Flash and Normal Drought Onset

Note: This figure plots the average 12-month trending of the NDVI around the onset of flash and normal droughts. NDVI anomalies are computed as deviations from the long-run monthly mean within each grid cell. Positive values indicate above-normal vegetation greenness, while negative values indicate vegetation stress. Solid lines represent normal droughts, and dashed lines represent flash droughts.

Appendix D: Test of similar agricultural impacts across two droughts: staple food prices

As an additional robustness check, we examine whether exposure to drought conditions affects local food market outcomes. This test considers any month in which an ADM1 region experiences either normal drought or flash drought conditions.

The dependent variable is a staple food price index constructed using monthly price data from FEWS NET. For each ADM1 region and month, we compute the average of the logarithmic prices of five major staple crops: white maize, red sorghum, millet, cassava, and mixed beans. When prices for some crops are missing, the index is computed as the mean of available log prices. Formally, the outcome variable is defined as

$$\text{StaplePrice}_{it} = \frac{1}{K_{it}} \sum_{k \in \mathcal{K}_{it}} \log(P_{kit}), \tag{3.2}$$

where P_{kit} denotes the price of staple crop k in region i and month t, and $\mathcal{K}_{it}$ denotes the set of crops with available price observations.

The key explanatory variables are indicators for whether an ADM1 region is experiencing normal drought or flash drought conditions in a given month.

We estimate two-way fixed-effects regressions including ADM1 and month fixed effects. Standard errors are clustered at the ADM1 level (and by time where applicable). The results show that both normal and flash droughts are associated with statistically significant increases in local food prices, with a similar magnitude, indicating comparable impacts on increasing price of agricultural production.

Table 3.A2: The Effect of Droughts on Staple Food Price

Dependent Variable:	Staple Price (Log)
Model:	(1)
Variables	
Normal Drought	0.0034**
	(0.0014)
Flash Drought	0.0028***
	(0.0010)
Fixed-effects	
ADM1	Yes
Month	Yes
Fit statistics	
Observations	10,002
R^2	0.94980
Within R^2	0.00236

Clustered (ADM1 & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix E: Placebo design using flash drought exposure as the separate variables

We also adopt a windowed event-time model as the main model specification. This approach analyzes how both normal and flash droughts affect the timing and onset of violent activities. The model includes dyad and time-fixed effects and clusters standard errors at the corresponding levels to address confounding.

We aggregate the lead and lag indicators for both normal and flash droughts into summary variables, capturing whether any drought event occurred in the twelve months before (anticipation window) or after (post-onset window) the onset month. Because constructing these twelve-month windows requires a full year of data on both sides of the onset, units that do not have complete lead or lag periods necessarily produce missing values. We thus exclude observations with incomplete twelve-month windows from the analysis. The model specification is as follows:

$$\begin{aligned}\text{Violence}_{it} = {} & \beta_1 \,\text{AnticipationWindow(Normal Droughts)}_{it} + \beta_2 \,\text{AnticipationWindow(Flash Droughts)}_{it} \\ & + \beta_3 \,\text{PostWindow(Normal Droughts)}_{it} + \beta_4 \,\text{PostWindow(Flash Droughts)}_{it} \\ & + \beta_5 \,\text{Onset(Normal Droughts)}_{it} + \beta_6 \,\text{Onset(Flash Droughts)}_{it} + \alpha_i + \gamma_t + \varepsilon_{it}.\end{aligned} \tag{3.3}$$

Here, Violence_{it} denotes the occurrence of violence in dyad i during month t. $\text{AnticipationWindow}_{it}$ are indicators for whether a normal or flash drought occurred within the twelve months prior to month t, respectively. PostWindow_{it} are indicators for whether a normal or flash drought occurred within twelve months after month t. $\text{Onset(Normal Droughts)}_{it}$ and $\text{Onset(Flash Droughts)}_{it}$ indicate whether a drought began in month t. α_i and γ_t are dyad and time fixed effects, respectively, and ε_{it} is the error term.

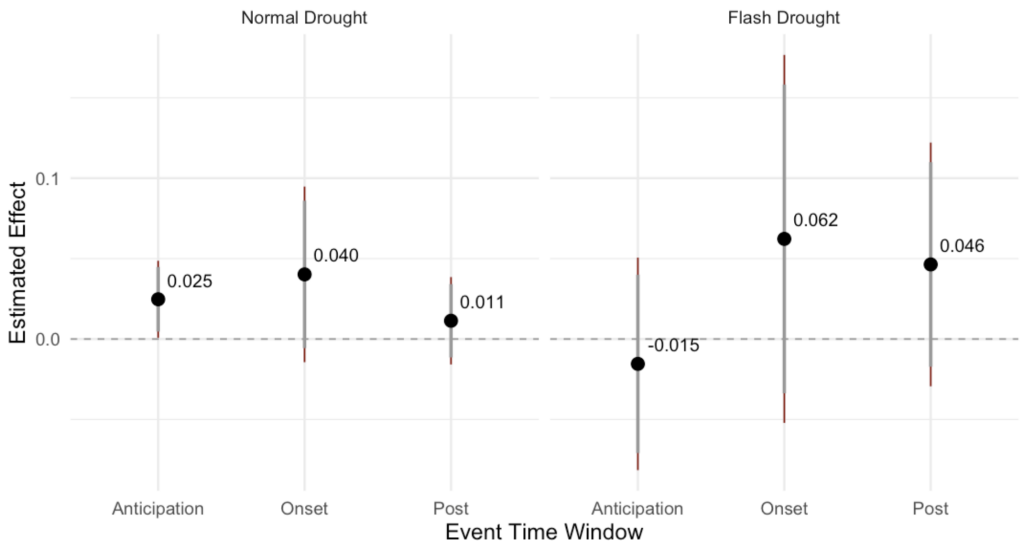

Figure 3.A4: Aggregated Dynamics Model with Anticipation and Post-Drought Windows

Note: This figure summarizes the estimated effects of normal and flash droughts on violence from an aggregated dynamics model. Instead of including twelve separate leads and lags, drought exposure is aggregated into two windows: an anticipation window, defined as whether a drought occurs within the following twelve months, and a post-drought window, defined as whether a drought occurred within the previous twelve months. Each point represents the estimated effect for the corresponding window, and the vertical bars denote the 90% and 95% confidence intervals.

Table 3.A3: The Placebo Test - Effect of Droughts on Civil Conflicts

Dependent Variable:	Civil Conflicts
Model:	(1)
Variables	
Post-onset (Normal)	0.0114
	(0.0139)
Post-onset (Flash)	0.0464
	(0.0386)
Pre-onset (Normal)	0.0247**
	(0.0122)
Pre-onset (Flash)	-0.0155
	(0.0337)
Onset (Normal)	0.0402
	(0.0279)
Onset (Flash)	0.0623
	(0.0584)
Fixed-effects	
dyad	Yes
time	Yes
Fit statistics	
Observations	11,491
R^2	0.53301
Within R^2	0.00204

Clustered (Dyad & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix F: The rapacity mechanism over the pre-position aid

Table 3.A4: The Effect of Droughts on Attacks on Aid

Dependent Variable:	Attack on aid worker
Model:	(1)
Variables	
Onset (any drought)	-0.0222
	(0.0192)
Onset (normal drought)	0.0170
	(0.0209)
Post-onset (any drought)	-0.0054
	(0.0036)
Post-onset (normal drought)	-0.0121*
	(0.0068)
Pre-onset (any drought)	-0.0119
	(0.0087)
Pre-onset (normal drought)	-0.0068
	(0.0100)
Fixed-effects	
Dyad	Yes
Month	Yes
Fit statistics	
Observations	11,395
R^2	0.79064
Within R^2	0.00242

Clustered (Dyad & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix G: The opportunistic attack due to dry-season mobility

Table 3.A5: The Effect of Droughts on Civil Conflicts, Interacting with Road Density

Dependent Variable:	Civil Conflict
Model:	(1)
Variables	
Onset (any drought)	0.0420
	(0.0439)
Onset (normal drought)	0.0016
	(0.0510)
Post-onset period (any drought)	0.0314
	(0.0478)
Post-onset period (normal drought)	-0.0181
	(0.0501)
Pre-onset period (any drought)	-0.0592
	(0.0415)
Pre-onset period (normal drought)	0.0969**
	(0.0454)
Road density	-0.0171**
	(0.0070)
Pre-onset period (normal drought) × Road density	-0.0016
	(0.0024)
Fixed-effects	
Dyad	Yes
Month	Yes
Fit statistics	
Observations	9,273
R^2	0.53732
Within R^2	0.00718

Clustered (Dyad & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix H: Using different threshold for coding the drought exposure

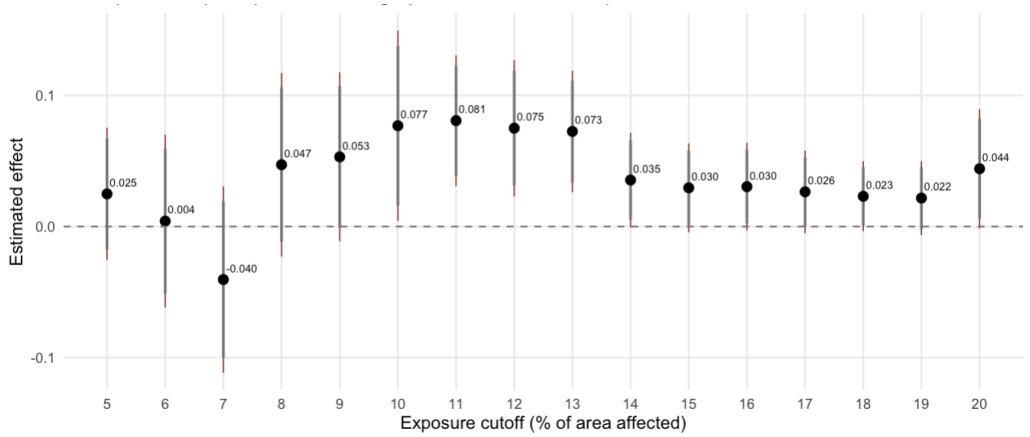

Figure 3.A5: Anticipation Effects of Normal Droughts Across Alternative Drought Exposure Extent

Note: This figure shows the estimated effects of the pre-onset period for normal droughts, defined as whether a normal drought occurs within the following 12 months, on violence, across different levels of drought exposure ranging from 5% to 20%. Each point represents the estimated coefficient for a different exposure threshold, and the vertical bars denote the 90% and 95% confidence intervals.

Appendix I: Using different lengths of anticipation and post windows

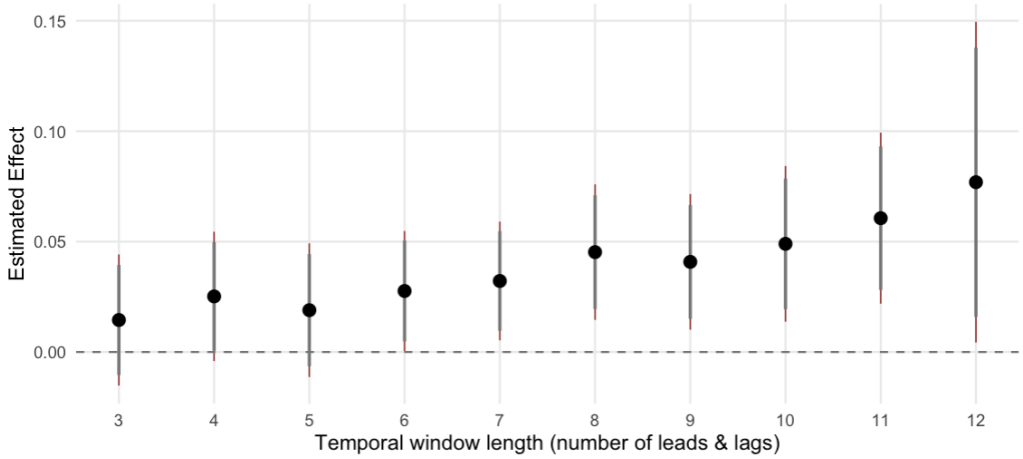

Figure 3.A6: Anticipation Effects of Normal Droughts Across Alternative Temporal Windows

Note: This figure shows the estimated effects of the anticipation window for normal droughts, defined as whether a normal drought occurs within the following 3 to 12 months, on violence. Each point represents the estimated coefficient for a different anticipation horizon, and the vertical bars denote the 90% and 95% confidence intervals.

Appendix J: Using different bridging rules for consecutive droughts

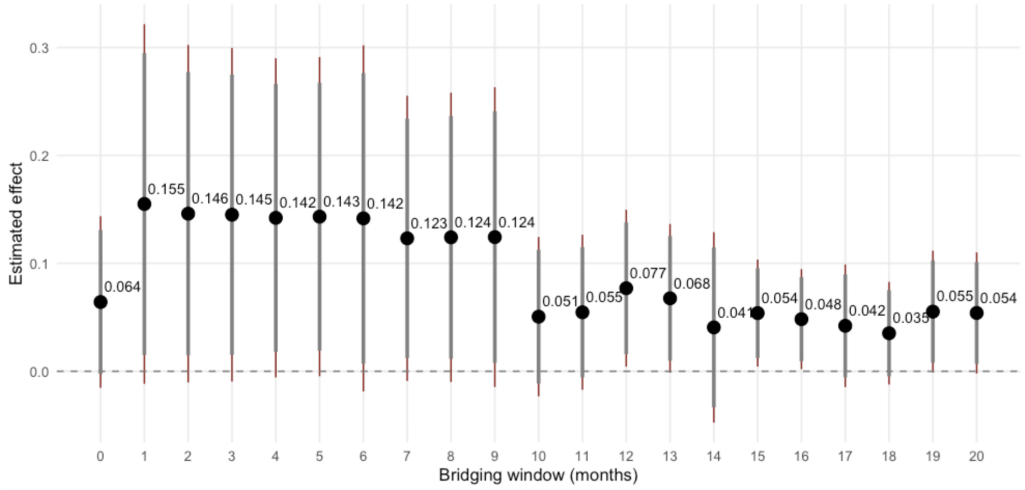

Figure 3.A7: Anticipation Effects of Normal Droughts Across Alternative Bridging Rules

Note: This figure shows the estimated effects of the anticipation window for normal droughts on violence across different bridging rules. The bridging rule determines the maximum temporal gap allowed between consecutive drought episodes for them to be considered part of the same continuous drought event. Each point represents the estimated coefficient under a different bridging rule, and the vertical bars denote the 90% and 95% confidence intervals.

Appendix K: Using different thresholds to define normal drought

Table 3.A6: The Effect of Droughts on Civil Conflicts, with a Different Threshold for Normal Drought

Dependent Variable:	Civil Conflict
Model:	(1)
Variables	
Onset (any drought)	-0.0583
	(0.0448)
Onset (normal drought)	0.0700
	(0.0480)
Post-onset (any drought)	-0.0155
	(0.0652)
Post-onset (normal drought)	0.0192
	(0.0641)
Pre-onset (any drought)	-0.1776***
	(0.0468)
Pre-onset (normal drought)	0.1938***
	(0.0464)
Fixed-effects	
Dyad	Yes
Month	Yes
Fit statistics	
Observations	11,687
R^2	0.42767
Within R^2	0.00229

Clustered (Dyad & Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This figure shows the estimated effects of the anticipation window for normal droughts on violence, using SPEI -1.5 as the threshold to define the normal drought exposure.

Appendix L: Testing Temporal Dependence Between Onsets of Flash and Normal Drought

Table 3.A7: Lagged Predictability Between Onsets of Flash and Normal Drought: The Onset of Flash Drought as the Outcome

Dependent Variable:	Flash Drought	
Model:	(1)	(2)
Variables		
Normal Drought (lag1)	0.0065*	
	(0.0036)	
Normal Drought (lag2)	0.0089	
	(0.0061)	
Normal Drought (lag3)	0.0012	
	(0.0032)	
Normal Drought (lag4)	0.0018	
	(0.0022)	
Normal Drought (lag5)	0.0085	
	(0.0074)	
Normal Drought (lag6)	0.0034	
	(0.0032)	
Normal Drought (lag7)	-0.0027*	
	(0.0016)	
Normal Drought (lag8)	-0.0054***	
	(0.0018)	
Normal Drought (lag9)	-0.0058**	
	(0.0026)	
Normal Drought (lag10)	-0.0037***	
	(0.0014)	
Normal Drought (lag11)	0.0032	
	(0.0045)	
Normal Drought (lag12)	-0.0027**	
	(0.0013)	
Normal Drought (12-month window)		0.0011
		(0.0015)
Fixed-effects		
Dyad	Yes	Yes
Month	Yes	Yes
Fit statistics		
Observations	69,987	70,186
R^2	0.05988	0.05900
Within R^2	0.00091	3.19×10^{-5}

Clustered (dyad & time) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 3.A8: Lagged Predictability Between Onsets of Flash and Normal Drought: The Onset of Normal Drought as the Outcome

Dependent Variable:	Normal Drought	
Model:	(1)	(2)
Variables		
Flash Drought (lag1)	0.0063	
	(0.0124)	
Flash Drought (lag2)	0.0041	
	(0.0092)	
Flash Drought (lag3)	0.0013	
	(0.0144)	
Flash Drought (lag4)	0.0061	
	(0.0118)	
Flash Drought (lag5)	0.0089	
	(0.0119)	
Flash Drought (lag6)	0.0047	
	(0.0090)	
Flash Drought (lag7)	0.0104	
	(0.0140)	
Flash Drought (lag8)	0.0033	
	(0.0063)	
Flash Drought (lag9)	-0.0013	
	(0.0101)	
Flash Drought (lag10)	0.0020	
	(0.0071)	
Flash Drought (lag11)	-0.0033	
	(0.0070)	
Flash Drought (12-month window)		0.0058*
		(0.0034)
Fixed-effects		
Dyad	Yes	Yes
Month	Yes	Yes
Fit statistics		
Observations	70,109	70,109
R^2	0.04063	0.04064
Within R^2	0.00010	0.00011

Clustered (dyad & time) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A potential concern is that normal and flash droughts may not be independent but sequential manifestations of the same underlying climatic process.

To assess this possibility, we estimate dyad-month fixed-effects models examining whether lagged onsets of normal drought predict subsequent flash drought onsets and vice versa. We include up to twelve monthly lags and cluster standard errors by dyad and time.

The results show no obvious relationship. Lagged normal drought onsets do not consistently increase the probability of subsequent flash drought onsets, and lagged flash drought onsets do not predict subsequent normal droughts.

Conclusion

This thesis answers a central question: under what conditions, and through what mechanisms, do natural disasters affect conflict dynamics during ongoing civil war? It integrates literature on strategic interaction between armed actors with the study of natural disasters and identifies three conditions under which natural disasters affect conflict dynamics, including spatial configurations of territorial control, the presence of third-party commercial actors, and armed actors' anticipation of future shocks. In doing so, the thesis contributes to a growing body of literature on climate change, natural disasters, and conflict, an area marked by increasing policy relevance and persistent academic empirical disagreement, by clarifying conditions under which natural disasters affect conflict dynamics.

This thesis also speaks to the wider literature on conflict escalation and de-escalation. First, the first paper examines a crucial but less studied conflict outcome of shifts in territorial control, and finds that the shift in territorial control can occur even in the absence of overt fighting. Second, the second paper expands the analytical focus beyond armed actors alone and shows how wartime strategic environments can be shaped by third-party commercial actors with the resources to influence strategic decisions by armed actors. Third, using variation in the predictability of two otherwise comparable climatic shocks - anticipated normal and unanticipated flash droughts, the third paper provides more credible empirical evidence for anticipation-based commitment problems.

Beyond the contributions presented here, this thesis points to several directions for future research. An important avenue concerns how anticipatory governance responses to environmental risk shape conflict dynamics. While this thesis primarily focuses on how realized natural disasters affect armed actors' strategic incentives, the third paper highlights the importance of expectations about future climatic shocks. In many crisis settings, such expectations are increasingly structured by formal early-warning systems that forecast impending environmental risks. Prominent examples include the Famine Early Warning

Systems Network (FEWS NET) and the Integrated Food Security Phase Classification (IPC), which provide regularly updated assessments of food insecurity and drought risk. Changes in early-warning classifications or alerts constitute observable signals about the severity of climatic crises. By shaping expectations about humanitarian mobilization, state preparedness, and international attention, these signals may influence the strategic calculations of armed actors and conflict dynamics. Despite the growing prominence of early-warning systems in climate-risk governance, systematic research on how such anticipatory signals affect conflict behavior remains limited. Future work could thus examine whether and under what conditions early-warning escalations alter patterns of violence.

A second direction is to examine the mechanisms linking disasters to conflict dynamics. Frequently cited mechanisms, such as recruitment, grievance mobilization, and organizational disruption, remain underspecified and weakly tested. Future research could examine how disasters affect recruitment strategies, how armed actors exploit or manage grievances, and how disasters affect internal organizational structures, command-and-control, and cohesion of armed actors, including both government and non-state actors. Advancing this agenda will require combining fine-grained conflict data with measures that capture civilian responses and organizational change.

A third set of research directions relates to debates on conflict escalation and de-escalation. While this thesis focuses primarily on battle-related violence and territorial control shift, it cannot directly observe how armed actors shift across the broader range of strategic choices available to them. Future research could thus examine how environmental shocks induce substitution across different forms of violence and military strategies, rather than simply increasing or decreasing overall levels of one conflict outcome. For example, scholars can study shifts from ground-based engagements toward alternative forms of military action, such as air or drone strikes, or the strategic use of low-intensity tactics including temporary ceasefires, negotiated pauses, and selective or periodic violence in the aftermath of natural disasters. Investigating these substitutions of strategies would contribute to a more nuanced understanding of escalation and restraint as strategic outcomes shaped by changing operational environments.

Additionally, future research can build on the thesis's treatment of territorial control as a spatial configuration rather than a single continuous quantity. Following recent calls to conceptualize territorial control as dynamic and spatially heterogeneous (Blair et al., 2024), a natural extension is to treat the change in territorial configuration as an outcome in its own right. Moving beyond baseline typologies, future research could study whether shocks accelerate fragmentation, consolidation, corridor formation, or enclave patterns of control over time. However, empirically, systematic data on territorial control remain limited and are often available only for specific conflicts, such as recent mapping efforts in Afghanistan, Myanmar, and the Philippines (Blair et al., 2024). Advances in remote sensing, conflict mapping, and machine-learning classification may gradually improve measurement, but feasibility constraints remain.

Finally, this thesis highlights methodological opportunities for conflict research. The satellite-based and geospatial measures developed here contribute to more precise and exogenous measures of natural disasters. More broadly, advances in remote sensing enable the measurement of infrastructure damage, mobility constraints, and logistics capacity, and provide a measurement solution to some research objectives that are difficult to observe directly (Dietrich et al., 2024; Sticher et al., 2024; Sticher et al., 2023). For example, satellite imagery could be used to assess post-disaster road damage or infrastructure disruption as proxies for logistical weakening, enabling more direct tests of theories linking organizational capacity to conflict behavior. Similar approaches could be applied to the study of economic activity, displacement, or territorial consolidation.

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