



Does Violence End – or Transform? Ceasefires and the Changing Forms of Violence in Civil Wars

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Introduction

Ceasefires are commonly understood as instruments for reducing violence during civil wars. Yet whether a ceasefire reduces direct fighting does not necessarily tell us what happens to violence more broadly. Conflict-management research has traditionally focused on whether organized warfare ends and whether peace survives, while research on post-conflict violence demonstrates that political violence can persist or change form even when conventional state–rebel fighting declines. Ceasefires occupy an important position between these two literatures because they directly constrain hostilities without necessarily resolving the political incompatibilities, organizational fragmentation, or coercive incentives underlying a conflict.

This paper examines whether ceasefires change not only the level of violence but also its composition. I introduce the concept of violence transformation, defined as a change in the relative composition of violence across state–rebel battle, non-state, and civilian-targeting violence. A ceasefire may reduce violence overall while still producing such a transformation if battlefield violence declines more sharply than alternative forms of violence.

Using a monthly panel combining the ETH/PRIO Civil Conflict Ceasefire Dataset with the UCDP Georeferenced Event Dataset, this paper examines effective intrastate ceasefires between 1989 and 2020. The analysis tests whether ceasefires reduce battle violence, whether they reduce total conflict-related violence, and whether alternative forms of violence change relative to battlefield violence after a ceasefire. The results provide limited evidence of a general decline in violence but stronger evidence of violence transformation: battlefield violence declines more sharply than alternative forms of violence, particularly non-state violence. These findings suggest that understanding whether ceasefires “work” requires examining not only how much violence remains, but also what form that violence takes.

Background

The dominant approach to conflict-management research is what makes a case successful and how do we prevent renewed war. Licklider (1995), for example, compares negotiated settlements with military victories in terms of the likelihood of conflict recurrence, while Wagner (1993) approaches the question through a bargaining framework in which peace becomes possible when continued fighting is no longer expected to improve either side’s position. Toft (2009) similarly asks why some civil wars end and remain terminated while others recur, emphasizing that durable peace depends not simply on the distribution of postwar benefits but also on credible enforcement and security institutions. Mason (2007) likewise focuses on the conditions under which countries relapse into renewed civil war,

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identifying peacekeeping and other policy-manipulable factors as important determinants of post-conflict stability. Across these approaches, the central outcome of interest is generally whether organized warfare resumes and how long the resulting peace survives. This literature has made important contributions to understanding the durability of civil-war settlements. It has identified the role of security guarantees, third-party intervention, peacekeeping, enforcement, and the manner in which wars terminate in shaping the probability of renewed conflict. Yet measuring successful conflict management primarily through recurrence creates an important limitation. The absence of renewed civil war does not necessarily imply the absence of political violence. A conflict may no longer return to full-scale state-rebel warfare while remaining characterized by violence involving militias, splinter factions, civilians, or other armed organizations. In this sense, measures of war recurrence are well suited to identifying whether peace collapses back into civil war but less suited to examining how violence changes during the period between active warfare and more durable forms of peace.

On the other side of the field, there has been persistent research on violence in the post-conflict period. Darby (2001), Sisk (2009), Höglund (2008), and Boyle (2014) all examine the persistence or changing nature of violence during peace processes and post-conflict transitions. Rather than assuming that the reduction of conventional warfare produces an immediate transition to nonviolence, this literature emphasizes that violence can remain a feature of societies moving from war toward peace. In some cases, violence continues because political disputes remain unresolved; in others, armed actors attempt to influence negotiations, strengthen their bargaining position, resist implementation, or undermine the peace process (Zartman 2001; Sisk 2009; Darby 2001; Stichter and Vukovic 2021). Sisk, for example, argues that violence can be used by both negotiating parties and spoilers to influence the nature, pace, and outcome of negotiations. Darby similarly examines the persistence of violence during peace processes and the role of actors seeking to obstruct or manipulate political settlements. This literature also draws attention to changes in the actors responsible for violence. Bara (2020) shows that much postwar violence is not necessarily perpetrated by the principal parties that fought the preceding civil war but rather by new actors or actors that were not officially involved in terminating the original conflict. Case-based research provides similar evidence that violence can persist or change form outside conventional state-rebel battlefield confrontation during periods of conflict transition (International Crisis Group 2017; Boyle 2010; Autesserre 2009; Höglund 2005). In Sri Lanka, for example, research on the 2002–2005 ceasefire describes how violence continued through practices that did not always take the form of straightforward violations of direct state–rebel hostilities (Höglund 2005).

These findings show that the transition from war to peace cannot be understood entirely through a binary distinction between war and non-war. Violence can persist below the threshold of renewed civil war, and the actors, targets, and organizational structure of that violence may differ from those that characterized

the original conflict. Ceasefires hence occupy an important position between active warfare and more comprehensive forms of conflict settlement. However, ceasefires have received considerably less attention in the academic scene compared to peace agreements and conflict termination, often only included into the broader analyses of peacemaking rather than as a distinct object of study.

Over the last two decades, a growing literature has sought to address this gap by examining ceasefires in their own right. One of the ways it is doing so is by showing that ceasefires can serve several distinct functions beyond simply ending fighting. Lane (2016) emphasizes their humanitarian role, such as enabling aid delivery, while Zartman (2001) and Rothchild (2002) treat ceasefires as conflict-management tools that can contain violence even when the underlying incompatibility remains unresolved. A larger literature examines ceasefires as instruments within peace processes: Smith and Barnaby (1996) argues that they can create conditions more conducive to negotiation, Höglund (2011) and Åkebo (2020) show how ceasefires can demonstrate armed groups' command and organizational cohesion, and Brickhill (2018) highlights their role in facilitating communication and trust between adversaries. Scholars also stress that ceasefires are not necessarily motivated by peaceful intentions. Stichter and Vukovic (2021) conceptualize them as bargaining instruments, while work by Toft (2010), Jarman (2004), and Höglund (2005) shows how actors may use pauses in fighting to regroup, consolidate control, or contain rather than eliminate violence. This has produced a related debate over timing: Touval (1995) favors earlier ceasefires to reduce immediate violence, whereas Mahieu (2007) argues that ceasefires may be more effective once the broad contours of a political settlement have emerged, since premature pauses can allow rearmament or reduce incentives for compromise. Moreover, research on ceasefire durability emphasizes institutional design: Fortna (2003, 2004, 2008) demonstrates that provisions increasing the credibility of commitments can prolong peace, while Potter (2004) and later practitioner-oriented research emphasize monitoring and verification as mechanisms for managing violations before they escalate into renewed fighting.

Recent research has also begun to move beyond the national-level state-rebel ceasefires shifting the attention to the broader conflict environment in which multiple armed relationships coexist. The first systematic analysis of how local ceasefires affect the dynamics of violence in civil war examined local ceasefires in Syria and found that violence is more likely to decline when agreements include stepwise implementation provisions (Lungeren, Svensson, and Karakus 2023). Duursma (2023) similarly broadens the ceasefire literature by examining agreements between non-state actors rather than only state-rebel dyads, identifying hundreds of non-state ceasefires in African civil conflicts and demonstrating that peacekeepers can facilitate their conclusion.

Despite these advancements, the field still lacks the vital empirical research on broader global patterns of ceasefires, which requires systematic and comprehensive data (Clayton et al. 2022, 1446). Although much of the research

described above was on “ceasefires”, the absence of a consistent definition and comprehensive comparative data has limited researchers’ ability to systematically study these often informal arrangements. Agreements labeled by the negotiators as “ceasefires” can differ considerably in their substantive provisions, while arrangements with similar characteristics are often recorded under entirely different terminology. Consequently, relying on formal labels alone can obscure meaningful variation between ceasefires and impede comparison across cases. The leading scholar in this topic, Govinda Clayton, defines “ceasefires as arrangements that include a commitment by at least one conflict party to stop violence” (Clayton et al. 2023a, 1282). The definition of ceasefires proposed by Clayton is a rather broad definition which covers a diverse range of arrangements – such as “cessation of hostilities”, “truces”, “armistices”, “windows of silence” and “humanitarian pauses” – that allows a more comprehensive analysis of global ceasefires across modern history. In addition, quantitative civil-war research, such as the Correlates of War and datasets by the Uppsala Conflict Data Programme, developed extensively around the causes, dynamics, and consequences of these intrastate conflicts. In 2023, the introduction of the ETH/PRIO Civil Conflict Ceasefire Dataset consequently created an opportunity to test whether findings drawn from individual cases generalize across conflicts and to examine how ceasefires alter conflict trajectories systematically. Existing empirical research using this dataset has primarily examined when ceasefires occur and how they shape subsequent conflict dynamics. Studies find that ceasefire onset is associated with higher conflict costs and political cover and that government ceasefires can encourage subsequent ceasefires and violence de-escalation in other conflict dyads (Clayton et al. 2023c; Bara and Clayton 2023). Another existing cross-national research finds that ceasefires are generally associated with transitions toward lower levels of battlefield violence (Williams et al. 2024). However, this work aggregates conflict dynamics at the country level and focuses exclusively on battle deaths, leaving unclear whether reductions in state-rebel battlefield violence correspond to reductions in overall violence or instead coincide with changes in the forms through which violence is carried out.

This paper builds on this developing research agenda by shifting attention from whether ceasefires merely stop violence toward which forms of violence change after they take effect. It examines whether direct state-rebel battle violence declines following ceasefires, whether total conflict-related violence changes once non-state and civilian-targeting violence are incorporated and whether those alternative forms of violence follow different post-ceasefire trajectories from battlefield violence. The central empirical question is thus not simply whether ceasefires “work”, but whether their effects are distributed unevenly across the different forms of violence that constitute a civil conflict.

Violence Transformation

For this paper, we would like to introduce the concept of violence transformation, which we define as a change in the relative composition of violence across different forms of violent activity. This paper focuses on three forms:

state–rebel battle violence, civilian-targeting violence, and non-state violence. This conceptualization builds on research showing that transitions away from conventional warfare can involve changes in the nature and perpetrators of violence rather than an immediate disappearance of violence altogether (Darby 2001; Boyle 2014; Bara 2020). A reduction in the total number of violent events does not necessarily indicate that all forms of violence decline at the same rate. If state–rebel battle violence decreases substantially while non-state violence decreases only slightly, for example, total violence may decline while non-state violence simultaneously becomes a larger component of the remaining conflict environment. The key element of violence transformation is therefore composition. Event counts indicate whether individual forms of violence increase or decrease, whereas a compositional comparison captures whether the balance among those forms changes. Violence transformation consequently does not require alternative forms of violence to increase in absolute terms. The starting point for this argument is that ceasefires directly constrain battlefield interaction. At their most basic level, ceasefires establish a commitment by conflict parties to suspend direct fighting. Their most immediate effect should therefore occur in the form of violence that they are explicitly designed to restrict: state–rebel battlefield violence. Although the strength and institutionalization of these commitments vary considerably across ceasefires, the commitment to suspend hostilities is common to the concept itself (Clayton et al. 2022; Clayton et al. 2023). We therefore expect that the introduction of a ceasefire will be associated with a reduction in direct battlefield violence.

H1: Ceasefires reduce battle events.

Because battlefield violence constitutes a major component of conflict-related violence, a reduction in direct state–rebel fighting should also reduce the overall level of violence surrounding a ceasefire. However, a ceasefire does not necessarily eliminate the political incompatibilities or coercive incentives that produced violence in the first place. Territorial competition, bargaining disputes, security dilemmas, organizational fragmentation, and competition among armed groups may continue after direct hostilities are suspended (Sisk 2009; Darby 2001; Wagner 1993; Toft 2009). Other forms of violence may therefore persist even while battlefield violence falls. This distinction motivates an examination of whether the reduction in the violence most directly targeted by a ceasefire is sufficient to produce a broader decline once non-state and civilian-targeting violence are also considered.

H2: Ceasefires reduce total conflict-related violence once non-state and civilian-targeting violence are included.

The persistence of these underlying conflict dynamics also provides reason to expect that ceasefires will not constrain all forms of violence equally. First, ceasefires bind particular actors, while spoilers, splinter factions, and armed organizations excluded from an agreement may remain capable of using violence. Research on postwar violence demonstrates the importance of actors outside the original conflict dyad (Bara 2020), while the spoiler literature shows how

factions opposed to a political settlement may continue violence in an attempt to undermine the process (Darby 2001). Second, even organizations formally participating in a ceasefire may not exercise complete control over all associated factions, allowing violence to continue despite commitments made by political or military leaders. Third, the costs imposed by a ceasefire may differ across forms of violence. Direct battlefield engagements between signatories are relatively visible violations of a commitment to suspend hostilities, whereas other forms of coercion may be less directly attributable to the participating parties. Research on indirect and delegated violence, for example, suggests that political actors may rely on armed groups that are not subject to the same formal constraints, thereby distancing themselves from direct responsibility (Boyle 2014; Carey and Mitchell 2017). Finally, violence may retain strategic value during negotiations even when open battlefield confrontation becomes more costly, as actors continue to seek bargaining leverage or attempt to shape the emerging political settlement (Sisk 2009; Darby 2001).

These mechanisms imply a differential effect of ceasefires across forms of violence. State–rebel battle violence should be the form most directly constrained by a ceasefire, while non-state and civilian-targeting violence may be less strongly affected. Importantly, this expectation does not require either alternative form to increase in absolute terms. Rather, if battlefield violence decreases more sharply than alternative forms of violence, the composition of the remaining violence will shift away from direct state–rebel confrontation.

H3: After a ceasefire, conflict violence shifts away from direct state–rebel battlefield violence toward alternative forms of violence, such that battle violence decreases relative to non-state and/or civilian-targeting violence.

The three hypotheses therefore capture distinct dimensions of post-ceasefire violence. H1 examines whether the form of violence most directly targeted by a ceasefire declines. H2 asks whether this reduction extends to conflict-related violence as a whole once alternative forms of violence are included. H3 moves from the level of violence to its composition, testing whether battle, non-state, and civilian-targeting violence follow systematically different post-ceasefire trajectories.

Finally, ceasefires differ substantially in the strength of the constraints they impose. The ETH/PRIO framework distinguishes between cessations of hostilities, ceasefires containing compliance mechanisms, and definitive ceasefires. These arrangements represent increasing levels of institutionalization, ranging from relatively simple commitments to suspend fighting to agreements incorporating monitoring, verification, disarmament, demobilization, and links to broader political settlements (Clayton and Sticher 2021; Clayton et al. 2023). Rather than treating these classes as separate concepts, this paper uses them to examine heterogeneity in the proposed mechanism. If violence transformation arises partly because ceasefires constrain direct battlefield interaction more strongly than other forms of violence, then the extent of this transformation may vary with the strength and institutionalization of the ceasefire. Differences across

ceasefire classes are therefore examined as an extension of the central argument.

The Data

The dataset used for this paper is adapted from a combination of the Uppsala Conflict Data Program’s Georeferenced Events Dataset (Version 26.1) and the Civil Conflict Ceasefire Dataset (CFD) by Govinda Clayton and Claudia Wiehler. The final data panel used for the analysis includes all 191 intrastate war ceasefires that went into effect from 1989 - 2020; many ceasefire declarations that were recorded in the CFD did not go into effect and were not included in the final analysis of this paper. To be included in the CFD as a ceasefire, the arrangement needs to fulfill all three following conditions: “includes a declaration (written or verbal) in which at least one actor commits to stopping violence”; “specifies an exact point in time at which the arrangement will be initiated”; “includes a commitment to stop rather than only reduce violent activity”.

In each ceasefire, there are often many agreements because an agreement is created between each dyad within the conflict. For our data set within the 191 intrastate war ceasefires, there are 1890 individual ceasefire agreements: of them, 1424 (75%) of the agreements are Class 1; 347 (18%) of the agreements are Class 2; 119 (6.2%) of the agreements are Class 3. The representation of each class in this sample is roughly the same as the original sample presented in the CFD with no concerns of endogeneity. The split between the different classes of ceasefires is distinctly unequal but is also a realistic representation of the nature of ceasefires in global conflict. Restricted forms of ceasefires are naturally more rare, as it requires more resources and attention that is not available every moment in every. While, hence, it is justified to maintain the current sample distribution, it must be noted that the results for Class 2 and 3 have to be interpreted with greater uncertainty in comparison to Class 1 due to the smaller sample size.

An analysis of transformation requires an examination of before and after the ceasefire, so the binary variable Post-Ceasefire is used as the key independent variable used across all the models. A 12 month window was chosen for this study because a year is just long enough to capture more than the immediate shocks but short enough to still remain under the short-run ceasefire effect while also removing endogenous errors caused by changing seasons.

The total number of violent events is our main dependent variable of this study, which is also associated with three other dependent variables based on the GED –state-based conflict, non-state conflict, and one-sided violence – for more specific analysis. While the number of deaths is also an available option, the core question of the transformation of violence requires specific focus on the frequency of the violence occurring rather than the lethality. Therefore, total death numbers are only used in this study as robustness checks for specific models.

Each row in the panel represents a month relative to a ceasefire agreement between a specific dyad. An example of how the panel has been disaggregated is shown in Figure 1. While in this example the relative months continue until up to 12 months after the ceasefire, many ceasefires do not have observations up

to 12 months post-ceasefire because another ceasefire is put into effect within a year after the original ceasefire.

Table 1: Violent Events Surrounding the January 2019 Ceasefire

Pre-ceasefire period												
Relative month	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1
Violent events	6	10	9	5	24	18	2	7	16	8	4	4

Ceasefire and post-ceasefire period													
Relative month	0	1	2	3	4	5	6	7	8	9	10	11	12
Violent events	2	13	0	45	17	8	17	22	15	22	7	11	10

Note: Tables show the total violent events occurring in each relevant month from the ceasefire by the Government of Libya and the Forces of the House of Representatives in 2019 January. Relative month 0 denotes the month in which the ceasefire took effect. Negative values represent months before the ceasefire and positive values represent months after the ceasefire.

In the case for the 2019 ceasefire between the Government of Libya and the Forces of the House of Representative, only the 2nd month after the ceasefire had a record of zero violent events. However, in the dataset, having zero violent events in a month make up the majority of the data while also having extreme outliers with some months exceeding 2000 events, creating an incredibly right-skewed distribution.

Once again, this unequal distribution is not ideal for data analysis, yet is an important realistic representation of the nature of civil wars. Therefore, to answer the hypotheses, we will be mainly using negative binomial models which are a type of linear regression model that addresses right-skewed, non-negative count data for better interpretation and analysis. In addition, because we disaggregated the observations down to the monthly level, most observations are not independent from each other as they are about the same ceasefire and/or actors. Standard errors for all the models are hence clustered by the UCDP dyad. For our covariates, we include six variables: partial ceasefire, humanitarian purpose, conflict duration, NSA fraction, mediator Negotiation, and Fixed Ceasefire.

Results

Hypothesis 1: Ceasefires reduce battle events

We begin by testing Hypothesis 1: ceasefires reduce battle events. Table 2 presents the results of the negative binomial models in which the monthly number of state–rebel battle events is the dependent variable. The first column reports the results for all ceasefires, while the remaining three columns estimate the post-ceasefire change separately for each ceasefire class.

Figure 1 shows a visible decline in actor-adjusted mean battle events around the

month of a ceasefire. However, the regression results show that this decline is not statistically significant across ceasefires generally. For the pooled sample, the Post-Ceasefire coefficient is -0.124, corresponding to an incidence rate ratio of approximately 0.883, or an estimated 11.7% reduction in battle events. This estimate is not statistically significant. The same is true for Class 1 and Class 2 ceasefires: Class 1 shows essentially no estimated change, while Class 2 is associated with an estimated 14.5% decline, but neither coefficient reaches conventional levels of statistical significance.

Table 2: Ceasefires and State-Rebel Battle Events

	All Ceasefires	By Ceasefire Class		
	All Ceasefires	Class 1	Class 2	Class 3
Post-Ceasefire	-0.124 (0.102)	-0.001 (0.146)	-0.157 (0.248)	-1.292*** (0.255)
Ceasefire Controls	Yes	Yes	Yes	Yes
Conflict Duration	Yes	Yes	Yes	Yes
Dyad-Clustered SE	Yes	Yes	Yes	Yes
N	49,426	33,351	10,922	5,153
Log Likelihood	-144274.91	-103378.75	-33984.70	-7236.46
AIC	288575.82	206783.51	67995.40	14498.92
Dyad Clusters	220	195	86	84

Notes: Negative binomial regression coefficients are reported. Standard errors clustered by UCDP dyad are shown in parentheses. The dependent variable is the monthly number of state-rebel battle events. Ceasefire controls include partial ceasefire status, humanitarian purpose, NSA fraction, mediator negotiation, and fixed-duration ceasefire status. Conflict duration is included as a standardized logged control, together with an indicator for missing conflict-duration values. The All Ceasefires model uses the full ceasefire sample, while class-specific models are estimated separately within each ceasefire class. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; two-tailed tests.

Class 3 ceasefires show a substantially different pattern. The Post-Ceasefire coefficient is -1.292 and is statistically significant at the 1% level. Exponentiating this coefficient gives an IRR of approximately 0.275, meaning that expected battle-event counts are about 72.5% lower after a definitive ceasefire, holding the included covariates constant. This is a large substantive decline. Although Class 3 observations make up a smaller portion of the sample and should therefore be interpreted with greater caution, the result provides strong evidence that the most institutionalized form of ceasefire is associated with a substantial reduction in direct state-rebel battlefield violence.

The results therefore provide conditional rather than general support for Hypothesis 1. There is insufficient evidence that ceasefires as a whole reduce battle events, or that Class 1 and Class 2 ceasefires produce statistically distinguishable reductions. The evidence is much stronger for Class 3 definitive ceasefires, which are associated with a major post-ceasefire decline in battle violence.

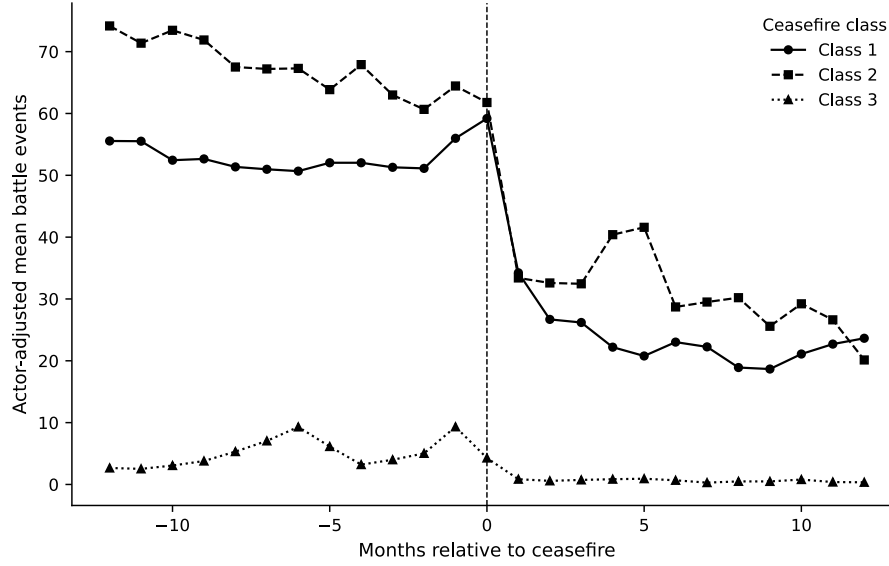


Figure 1: Months Relative to Ceasefire

Hypothesis 2: “Ceasefires reduce total conflict-related violence once non-state and civilian-targeting violence are included”

Hypothesis 2 broadens the analysis beyond direct state–rebel fighting by asking whether ceasefires reduce total conflict-related violence once non-state and civilian-targeting events are also included. Table 3 reports the Post-Ceasefire coefficient from separate negative binomial models for total events, battle events, civilian-targeting events, and non-state events. As in the previous analysis, the first column pools all ceasefires, while the remaining columns estimate the relationships separately by ceasefire class.

Table 3: Ceasefires and Conflict-Related Violence

	All Ceasefires	By Ceasefire Class		
	All Ceasefires	Class 1	Class 2	Class 3
Post-Ceasefire: Total Events	-0.082 (0.091)	0.033 (0.125)	-0.142 (0.200)	-0.968*** (0.180)
Post-Ceasefire: Battle Events	-0.124 (0.102)	-0.001 (0.146)	-0.157 (0.248)	-1.292*** (0.255)
Post-Ceasefire: Civilian-Targeting Events	0.041 (0.152)	0.154 (0.144)	-0.199 (0.135)	-0.672*** (0.137)
Post-Ceasefire: NSA Events	0.119 (0.114)	0.239** (0.096)	0.165 (0.180)	-0.205* (0.118)
Ceasefire Controls	Yes	Yes	Yes	Yes
Conflict Duration	Yes	Yes	Yes	Yes
Dyad-Clustered SE	Yes	Yes	Yes	Yes
N	49,426	33,351	10,922	5,153
Log Likelihood	-167341.09	-118040.48	-39399.25	-11036.13
AIC	334708.18	236106.95	78824.51	22098.27
Dyad Clusters	220	195	86	84

Notes: Negative binomial regression coefficients are reported. Standard errors clustered by UCDP dyad are shown in parentheses. The first four rows report the post-ceasefire coefficient from separate models of total, state–rebel battle, civilian-targeting, and non-state violence events. Ceasefire controls include partial ceasefire status, humanitarian purpose, NSA fraction, mediator negotiation, and fixed-duration ceasefire status. Conflict duration is included as a standardized logged control, together with an indicator for missing conflict-duration values. The All Ceasefires models use the full ceasefire sample, while class-specific models are estimated separately within each ceasefire class. N, log likelihood, and AIC refer to the total-events model. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; two-tailed tests.

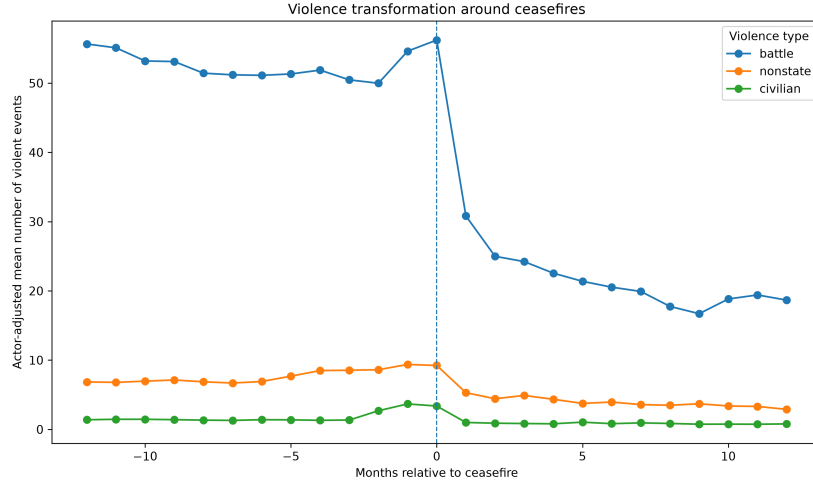


Figure 2: Violent Events Before and After Ceasefires by Violence Type
Notes: The figure reports actor-duplication-adjusted mean monthly counts of battle, non-state, and civilian-targeting events during the 12 months before and after a ceasefire. Month 0 denotes the month in which the ceasefire took effect. Observations following a subsequent ceasefire are excluded from the relevant ceasefire spell.

The results for total violent events closely resemble those for battle violence. Across all ceasefires, the Post-Ceasefire coefficient is -0.082, corresponding to an estimated 7.9% reduction in total conflict-related events. However, this relationship is not statistically significant. Class 1 is associated with a small estimated increase of approximately 3.4%, while Class 2 is associated with an estimated decline of approximately 13.2%; neither relationship is statistically significant. These results therefore provide no evidence that Class 1 or Class 2 ceasefires systematically reduce the overall number of violent events once non-state and civilian-targeting violence are incorporated.

Again, the clearest exception is Class 3. The Post-Ceasefire coefficient for total events is -0.968 and is statistically significant at the 1% level. This corresponds to an IRR of approximately 0.380, meaning that total conflict-related event counts are estimated to be about 62.0% lower following a definitive ceasefire, holding the included covariates constant. The magnitude of this reduction is substantial, although it is smaller than the corresponding 72.5% decline in battle events.

The component-specific models help explain why the total-violence result is more complicated than the battle-violence result. For the pooled sample, none of the separate Post-Ceasefire coefficients for civilian-targeting or non-state events are statistically significant. The coefficient for civilian-targeting events is 0.041, corresponding to an estimated 4.2% increase, while the coefficient for non-state events is 0.119, corresponding to an estimated 12.6% increase. These estimates should not be interpreted as evidence that ceasefires increase either form of

violence because both are statistically indistinguishable from zero.

There are, however, several notable class-specific patterns. Among Class 1 ceasefires, non-state violence increases by approximately 27.0% after the ceasefire and the coefficient is statistically significant at the 5% level. Class 3 ceasefires show the opposite absolute pattern: battle events decline by approximately 72.5%, civilian-targeting events by approximately 48.9%, and non-state events by approximately 18.5%. The civilian-targeting decline is significant at the 1% level, while the non-state decline is significant only at the 10% level.

These results are important because they show that examining total violence alone can conceal substantial differences across its component forms. Class 3 ceasefires are associated with reductions across all three types of violence, but the estimated decline is much larger for direct battle violence than for non-state violence. Conversely, Class 1 ceasefires show no significant change in total or battle violence but are associated with an increase in non-state events. These comparisons are suggestive of different post-ceasefire trajectories across forms of violence, although the separate negative binomial models do not by themselves establish whether those differences are statistically significant relative to one another. That relative comparison is tested directly under Hypothesis 3.

The results therefore provide conditional support for Hypothesis 2. There is no statistically significant evidence that ceasefires generally reduce total conflict-related violence once non-state and civilian-targeting events are included. However, definitive Class 3 ceasefires are associated with an approximately 62% reduction in total violent events. This suggests that the ability of ceasefires to reduce violence more broadly is concentrated among the most institutionalized ceasefire arrangements rather than being a general feature of ceasefires.

Hypothesis 3: ‘After a ceasefire, conflict violence shifts away from direct state-rebel battlefield violence toward alternative forms of violence, such that battle violence decreases relative to non-state and/or civilian-targeting violence’

The first two hypotheses examined whether ceasefires are followed by changes in the level of battle and total conflict-related violence. Hypothesis 3 instead examines whether ceasefires change the composition of violence: whether non-state or civilian-targeting violence follows a different post-ceasefire trajectory relative to state-rebel battle violence. This distinction is important because violence transformation does not require alternative forms of violence to increase in absolute terms. As established in the conceptual framework, transformation can occur if battle violence simply declines more sharply than the other forms of violence.

To make these relative changes directly comparable, the data are stacked so that each observation represents a ceasefire episode $\times$ dyad $\times$ month $\times$ violence type. Because this creates repeated observations within the same conflict dyads, we estimate a stacked Poisson generalized estimating equation (GEE) model with robust standard errors clustered by UCDP dyad. Battle violence is treated as the reference category, allowing the model to test whether the post-ceasefire

change in non-state and civilian-targeting violence differs significantly from the corresponding change in battle violence.

Table 4: Post-Ceasefire Changes in the Composition of Violent Events

	IRR	% Change	95% CI	<i>p</i> -value
<i>Post-ceasefire effects by violence type</i>				
Battle violence	0.722	-27.8%	[0.616, 0.846]	< 0.001
Non-state violence	0.912	-8.8%	[0.808, 1.029]	0.136
Civilian-targeting violence	0.935	-6.5%	[0.667, 1.311]	0.695
<i>Relative transformation compared with battle violence</i>				
Non-state vs. battle	1.264	+26.4%	[1.087, 1.469]	0.005**
Civilian-targeting vs. battle	1.295	+29.5%	[0.950, 1.765]	0.102

Notes: Estimates are incidence rate ratios (IRRs) from stacked Poisson generalized estimating equation (GEE) models with robust standard errors clustered by UCDP dyad. Battle violence is the reference violence type. The transformation contrasts test whether the post-ceasefire change in each alternative form of violence differs from the corresponding change in battle violence. Holm-adjusted *p*-values are reported for the two transformation contrasts. The adjusted *p*-value for non-state versus battle violence is 0.005; for civilian-targeting versus battle violence it is 0.102. Models include controls for ceasefire class, armed-group fractionalization, mediation, partial ceasefire status, fixed duration, humanitarian and peace-process purposes, conflict duration, missing conflict duration, and region.

***p* < 0.01 after Holm adjustment.

Table 4 first reports the estimated post-ceasefire change for each form of violence separately. Battle violence shows the clearest decline, with an IRR of 0.722, corresponding to an estimated 27.8% reduction after a ceasefire. This decline is statistically significant at the 0.1% level. Non-state violence also declines, but by only 8.8%, while civilian-targeting violence declines by approximately 6.5%. Neither of these latter changes is independently statistically significant.

These results already suggest that the three forms of violence do not follow the same post-ceasefire trajectory. However, Hypothesis 3 is not primarily concerned with whether each form changes independently; it asks whether alternative forms of violence change relative to battle violence. The transformation contrasts in the lower half of Table 4 test this directly.

The estimated non-state-versus-battle IRR is 1.264. This means that, after a ceasefire, non-state violence becomes approximately 26.4% more prevalent relative to battle violence than it was before the ceasefire. The Holm-adjusted *p*-value is 0.005, providing strong evidence that the post-ceasefire trajectory of non-state violence differs from that of battle violence. Importantly, this does not mean that ceasefires cause non-state violence to increase in absolute terms. Non-state violence actually declines slightly after ceasefires; it simply declines much less sharply than battle violence. The result therefore represents a change in the composition of violence rather than an absolute increase in non-state conflict.

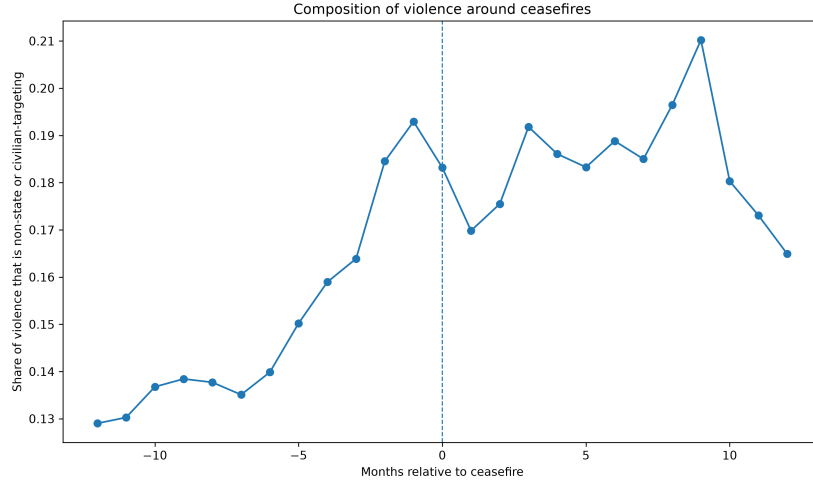


Figure 3: Share of Conflict Violence Composed of Alternative Forms of Violence
Notes: Alternative violence is defined as the combined number of non-state and civilian-targeting events. The figure reports the actor-duplication-adjusted share of relevant violent events attributable to these alternative forms of violence in each month relative to the ceasefire.

Civilian-targeting violence shows a similar estimated direction. Relative to battle violence, civilian-targeting violence becomes approximately 29.5% more prevalent following a ceasefire. However, the Holm-adjusted p-value of 0.102 means that this difference does not reach conventional levels of statistical significance. The evidence for transformation is therefore considerably stronger for non-state violence than for civilian-targeting violence.

Figure 3 provides a complementary visualization of this compositional shift by showing the share of conflict-related violence accounted for by non-state and civilian-targeting events combined. Although there is an immediate decline around the ceasefire month, the alternative share of violence is generally higher during much of the post-ceasefire period than during the earlier pre-ceasefire months. The figure itself does not establish statistical significance, but the pattern is consistent with the relative-transformation estimates reported in Table 4.

Table 5: Alternative Measures of Post-Ceasefire Violence Transformation

Test	Ratio	% Change	95% CI	<i>p</i> -value
Alternative vs. battle	1.281	+28.1%	[1.108, 1.482]	< 0.001
Alternative share of violence	1.171	+17.1%	[1.043, 1.316]	0.008

Notes: The first row reports the transformation IRR from a stacked Poisson GEE comparing **combined non-state and civilian-targeting** events with battle events. The second row reports the odds ratio from a grouped binomial GEE estimating the post-ceasefire change in the odds that a violent event belongs to the alternative category rather than battle violence. Robust standard errors are clustered by UCDP dyad.

Table 5 tests this pattern more directly by combining non-state and civilian-targeting violence into a single category of alternative violence. The first test compares the post-ceasefire trajectory of this combined category with that of battle violence. The resulting ratio of 1.281 indicates that alternative violence becomes approximately 28.1% more prevalent relative to battle violence after a ceasefire. This result is statistically significant at the 0.1% level.

The second test asks a slightly different question: whether this differential change actually alters the composition of violence. The estimated odds ratio of 1.171 indicates that, following a ceasefire, the odds that a violent event belongs to the alternative category rather than battle violence increase by approximately 17.1%. This relationship is also statistically significant, with a *p*-value of 0.008. The two tests therefore capture related but distinct aspects of transformation: the first establishes that alternative and battle violence follow different post-ceasefire trajectories, while the second shows that this difference translates into a measurable change in the overall composition of violence.

Taken together, these results provide strong support for Hypothesis 3, particularly for the transformation toward non-state violence. Battle violence declines substantially after ceasefires, while non-state and civilian-targeting violence decline much less sharply. The difference between non-state and battle violence is statistically significant, and the combined alternative-violence tests provide additional evidence that the share of violence occurring outside direct state–rebel battlefield confrontation increases after ceasefires. Rather than showing that ceasefires simply displace battle events into other forms of violence, the results indicate that ceasefires are associated with uneven reductions across violence types, producing a measurable transformation in the composition of the remaining conflict environment.

Conclusion

This paper examined whether ceasefires change the composition of violence during civil wars rather than simply whether they reduce fighting. The results provide limited support for a general reduction in battle or total conflict-related violence across all ceasefires, although definitive Class 3 ceasefires are associated with substantial reductions. The clearest finding instead concerns violence

transformation. Battle violence declines substantially following ceasefires, while non-state and civilian-targeting violence decline less sharply. Non-state violence consequently becomes significantly more prevalent relative to battle violence, and the combined share of alternative violence also increases following ceasefires.

These findings complicate a binary understanding of conflict management as a transition between war and peace. Ceasefires directly constrain state–rebel confrontation, but they do not necessarily constrain every form of violence to the same extent. Importantly, this does not mean that ceasefires cause alternative violence to increase. Rather, different forms of violence follow different post-ceasefire trajectories, changing the composition of the conflict environment even as violence may decline overall.

The analysis remains limited in its ability to identify the mechanisms producing this transformation. The cross-national data can establish that forms of violence change differently after ceasefires, but cannot determine whether these patterns result from splinter groups, spoilers, delegation, organizational fragmentation, or other processes. Future research connecting violent events more directly to the actors participating in and excluded from ceasefire agreements would therefore help explain why these compositional shifts occur. Nevertheless, the findings demonstrate that evaluating ceasefires solely according to whether they stop direct fighting misses an important part of their consequences: ceasefires may not simply reduce violence, but transform the violence that remains.

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