

# Your Reputation Precedes You: Ceasefires and Cooperative Credibility During Civil Conflict

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## Abstract

How does the state's behavior in negotiations with one non-state group influence the behavior of other non-state actors? We argue that the dynamics of different conflicts within the same country are interdependent, and that a state develops a reputation through its interactions with each conflict party. This reputation provides a valuable source of information that other groups use to judge the state's cooperative intentions. When a state develops a reputation for cooperation, this increases the likelihood of (indirect) reciprocation from other groups. More specifically, we argue that when states enter into (and honor) a ceasefire with one rebel group, they demonstrate a credible reputation for cooperation. A credible reputation for cooperation, we posit, increases the likelihood that other conflict parties enter into ceasefires with the state, or that they de-escalate their military activities. We test our claims using the new civil conflict ceasefire dataset and find support for our argument.

## Keywords

reputation, ceasefires, civil war, bargaining, conflict dynamics, conflict management

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## Introduction

States often fight multiple groups simultaneously. Most civil conflicts now involve more than one non-state group, and states involved in civil conflict regularly fight more than one conflict concurrently (Pettersson, Högladh, and Öberg 2019). As conflict parties are strategic actors that factor in, and anticipate, the actions of other armed actors (Fjelde and Nilsson 2012), we should expect that the reputation a state develops through its interactions with one group is likely to influence how other groups behave towards it.

Surprisingly then, existing research tends to overlook the ways in which the reputation a state gains in its negotiations with one actor influences other concurrent processes. Commonly research considers the meta-negotiation process between the state and all challengers, and so fails to account for the varied negotiation processes that often occur simultaneously with the different groups, and how sequence and outcomes from one process may influence another. Alternatively, studies treat the dyadic interactions between the state and the different non-state parties as a series of relatively independent processes, which overlooks the system of conflicts within which the dyad is situated, and how the reputation gained in one process might affect behavior in the others.

Studies of civil conflict do however point to the importance of reputation in bargaining processes. In a seminal body of work, Walter (2006a, 2006b, 2009) shows that states' concerns for their reputation influence their propensity to grant concessions. Conversely, the decisions of ethnic groups to mobilize is shaped by their expectation that the government will concede, which is based largely on the governments reputation for conceding in the past (Walter 2006b; see also, Forsberg 2013; Cunningham 2014; Bormann and Savun 2018). Collectively this research suggests that reputation does indeed matter, but only insofar as it relates to the state's bargaining resolve (or lack thereof).

Reputation is a multifaceted concept, that can relate to different underlying characteristics. Beyond a reputation for resolve, states should also be able to develop a reputation for other traits, such as the credibility of their cooperative commitments. During civil conflict, the parties' inability to credibility commit to cooperating is a serious impediment to bargaining (Fearo 1995; Lake and Rothchild 1996; Walter 2001). Yet research from other fields, such as economics and international relations, shows that a state can mitigate this issue by developing a credible reputation for cooperation in its interactions with other actors (Crescenzi 2018). To date, however, studies of civil war peace processes have paid little attention to how the cooperative reputation that a state gains through its interactions with one party might subsequently shape the behavior of other conflict parties (notable exceptions are, Joshi and Quinn 2016; Quinn, Joshi, and Melander 2019). We develop a theoretical framework and empirical strategy to address this lacuna.

First, building on studies of reputational learning we argue that when non-state actors are worried about exploitation, they can use the state's actions towards

other similar non-state actors to assess their credibility. When a state credibly demonstrates a commitment to cooperation in its interactions with one non-state group, it develops a credible cooperative reputation that increases the likelihood of indirect reciprocation from other non-state conflict parties. Thus similar to [Quinn et al. \(2019\)](#); see also, [Joshi and Quinn 2016](#)), we argue that peacemaking is a dynamic and sequential process, in which what happens with one non-state group shapes progress with another.

Next, we develop an empirical strategy linking our argument to ceasefires in civil conflict. Ceasefires are a regular feature during civil conflict, and often an integral part of the bargaining process ([Clayton et al. 2022b](#)). States can use ceasefire to build a reputation for cooperation. Prior ceasefire research has revealed the important signaling role that ceasefires can perform during a peace process, but has until now focused only on the direct relationship between a state and non-state group ([Clayton and Sticher 2021](#)), or between non-state groups ([Duursma 2022](#); [Lundgren, Svensson, and Karakus 2022](#)). We extend this work, showing the broader role that ceasefires play in shaping the reputation of a state, and how this impacts other concurrent peace processes.

Specifically, we argue that when states enter into a ceasefire with one rebel group they develop a more credible reputation for cooperation, that increases the likelihood other non-state groups in the same country enter into a ceasefire with the state or choose to de-escalate the violence in a first step. This is particularly the case for those ceasefires that are most likely to generate a cooperative reputation, namely ceasefires during peace processes, including compliance provisions, and those that successfully stop the violence for some minimal period. We test our claims using the new Civil Conflict CeaseFire (CF) Dataset ([Clayton et al. 2022a](#)) which contains information on 2202 ceasefires across 66 countries, in 109 civil conflicts. We use this data in a time-series cross-sectional setup in which the independent variable – ceasefires in other dyads – is akin to a temporally lagged spatial lag (TLSL) that captures the interdependence between dyads that is at the heart of our theoretical argument (see, [Drole, Gandrud, and Williams 2019](#); [Wimpy, Whitten, and Williams 2020](#)). Using logistic and negative binomial regression models with dyad fixed effects, we find robust support for our argument. These results have important implications for the study of civil war, and show that beyond a reputation for resolve, a state can also develop a reputation for credible cooperation that increases the likelihood of subsequent cooperative interactions with other actors.

In what follows, we briefly discuss existing literature on bargaining and reputation, and then develop our argument of how states develop a reputation for credible cooperation, and the function that ceasefires perform in this process. We next set out our research design, and finally discuss the statistical results.

## Reputation

Reputation, generally speaking, is the sum of the beliefs or opinions that one party holds of another. It can relate to different characteristics or traits, and vary across actors and issues. Actors can shape their reputation through their behaviour, but as reputation is always the product of others' perceptions, there is often a gap between the reputation an actor desires and the one it has (Crescenzi 2018).

We focus specifically on indirect forms of reputation, whereby actors use the experiences of other similar actors as a sort of precedent to usefully inform their decisions. Indirect forms of reputation are vital to many online businesses, such as Airbnb, and Ebay, whereby customers judge the likelihood of a vendor honouring potential agreements based on their past behaviour towards other similar customers. Indirect forms of reputation generation have also been shown to perform important functions during international conflict and negotiations (see for example, Crescenzi 2018, 8; Jervis 2017; Leng 1983; Levy 1994; Melin and Grigorescu 2014).

Existing research on indirect forms of reputation in the context of civil conflict focus primarily on states' reputation for bargaining 'resolve'.<sup>1</sup> Put simply, this work centres on the idea that states can either gain a reputation for being resolved or unresolved based on their willingness to make concessions to armed challengers (Horowitz 1985, 279). Toft (2002, 2003) and Walter (2006a, 2009) show that states' concerns about their reputation for resolve often lead them to resist making any concessions, in particular in those contexts in which a 'weak' reputation potentially carries greater costs (e.g. when there exists more opportunity for other groups to mobilize).<sup>2</sup> The logic being that states fear that concessions to one group now is likely to harm their reputation, which is likely to provoke new challengers, which will then require more concessions in the future (Walter 2006a).<sup>3</sup> Walter shows that when states refuse to accommodate (i.e. generate a reputation for being resolved) this does indeed deter future challengers, whereas accommodating demands (i.e. generating a reputation for lacking resolve) increases the likelihood that other groups will mobilize in the future.

Cunningham (2011, 2014) expands on this work, showing that states that accommodate self-determination movements tend to subsequently offer more concessions towards other groups. Cunningham does not speak about the reputation of the state directly, but provides evidence that a reputation for concession-making does tend to lead to further concession-making (see also, Stedman 1997).<sup>4</sup>

In contrast, Forsberg (2013) casts doubts on these claims, arguing that ethnic groups are not more likely to initiate secessionist claims if other groups in close proximity achieve concessions. Similarly, Sambanis, Germann, and Schädel (2018) do not find that governments are more likely to accommodate or deny demands in cases with more potential challengers. Yet Bormann and Savun (2018) show that Walter's reputation argument does indeed hold up, despite the reservations noted in the prior research, when the precise scope conditions for the reputation for resolve theory are delineated, and point to the need to be clear on *when* and *to who* reputation is likely to be important.

Taken together, existing research demonstrates that concerns about reputation, in particular with regards to the state's resolve, shape the behaviour of conflict parties. Yet the focus of this work is almost entirely upon how a state's reputation for resolve is perceived by other *potential* conflict parties, and largely overlooks the impact on other groups *already* engaged in conflict with the state (though see, Nilsson 2010; Quinn, Joshi, and Melander 2019). Moreover, the exclusive focus on reputation for resolve largely overlooks the possibility that other forms of reputation might also develop and shape actors' behaviour.

### *Reputation for Cooperation*

Reputation can relate to different characteristics and traits beyond simply a reputation for resolve. Existing studies of inter- and intra-state conflict largely overlook the possibility of building a credible cooperative reputation during a peace process. Yet there exists an established body of work within business and economics that shows the importance of a cooperative reputation during bargaining. Similar to the bargaining problem during civil conflict (see, Fearon 1995; Walter 2001), economic research focuses on a context (i.e. the marketplace) defined by incomplete information, where the problem of asymmetric information makes it challenging to observe the true intentions of possible collaborators. One of the primary sources of information that is used to get around this is the reputation that an actor develops (Weigelt and Camerer 1988). Research shows that when an organisation develops a credible reputation for cooperation, they are more likely to enter into alliances (Barney and Hansen 1994; Das and Teng 1998) and agree more cooperative agreements in the future (Dollinger, Golden, and Saxton 1997). Developing a reputation for cooperation can often be costly, and entail risks to the actors involved, but if they deliver on commitments, they are then more likely to be trusted in the future (Allen 1984; Axelrod and Keohane 1985).

The effect of a cooperative reputation extends beyond the direct relationship between two actors. A reputation gained from dealing with one organisation, also appears to influence how other actors subsequently judge the trustworthiness of that actor (Das and Teng 1998; Dollinger, Golden, and Saxton 1997; Shapiro 1983). Actors are more willing to cooperate with actors who have developed a reputation for responding to cooperation with cooperation in other relationships (Crescenzi 2018, 33). On occasions, the state might have a clear intention to develop a cooperative reputation beyond the immediate interaction. On other occasions, this might simply be an unintentional consequence of progress or failures in another process. This argument is then a logical corollary to the resolve argument. Rather than only signal an actor's resolve, reputation can also serve as measure of cooperative commitment, and convey a willingness to enter into and honour an agreement or contract to other interested parties (Crescenzi 2018, 67; Quinn, Joshi, and Melander, 2019: 868).

## *Ceasefires and Reputation*

This raises the question of how an actor generates a cooperative reputation during civil conflict? And in what way might we empirically observe this phenomenon? We limit our focus to the state's reputation.<sup>5</sup> We expect that a state develops a cooperative reputation when one non-state conflict party observes the state undertaking cooperative behaviours towards another conflict party. The cooperative behaviour must be relatively public and so observable by other groups, and relatively significant, so as to signal a shift from the prior conflictual behaviour. Public statements and general commitments to cooperation are unlikely to carry sufficient weight once a conflict has escalated past a certain point, instead more specific information about recent state actions and behavior are likely to be required.

Intuitively, we might expect that the most significant generator of a state's cooperative reputation would occur with the signing of a peace agreement. Indeed, almost all studies of reputation have tended to focus either on agreements or similarly significant forms of accommodation (c.f. [Cunningham 2014](#); [Joshi and Quinn 2016](#)).

Yet a peace agreement is often the culmination of a long process of negotiations, in which the parties slowly come to an agreement. Thus, while a peace agreement surely does improve a state's cooperative reputation, it is also likely that other groups will have at least partially 'priced in' the impact of the agreement prior to its signing. Peace agreements are also relatively rare, and if successful mark the conclusion of peace negotiations. Focusing only on settlements therefore overlooks the wide range of other factors that might occur during negotiations that might also shape the state's reputation. We are concerned with how the reputation of a state shifts during a peace process, and how this in turn impacts the behaviour of other actors. We therefore require another indicator that is likely to shift states' reputation, but that is more a part of, rather than the end of, a bargaining process.

We focus instead on ceasefires as a driver of states' reputation. A ceasefire is an arrangement that includes an explicitly declared intention, by at least one belligerent, to suspend hostilities from a specific point in time ([Clayton et al. 2022c](#); [Clayton et al. 2022a](#)). Ceasefires are a common feature in almost all civil conflicts. Between 1989 and 2020, there were 2202 ceasefires across 66 countries, in 109 civil conflicts ([Clayton et al. 2022a](#)). Despite their frequency, we know very little about how ceasefires shape bargaining between conflict parties during civil conflict, and to the best of our knowledge, no work has yet explored the broader impact of ceasefires on other ongoing conflicts. We extend previous work in showing the broader role that ceasefires play in shaping the reputation of conflict parties, and how this impacts other political negotiations.

Ceasefires can occur at any point during a civil conflict, and serve a number of different purposes ([Clayton et al. 2022a](#); [Clayton et al. 2022c](#); [Clayton, Nathan, and Wiehler 2021](#)). One common function is signaling cooperative intentions to an opponent ([Clayton and Sticher 2021](#)). Declaring a ceasefire is a (relatively) costly signal that a conflict party intends to (at least) temporarily suspend violent hostilities, and

shows a willingness to consider non-military alternatives. This can create an environment more conducive to negotiation, where, for example, it is easier for a non-state group to soften their demands. Thus, while it is challenging to determine the underlying purpose of the ceasefire for each conflict party, all else being equal, we believe that ceasefires are an example of what Crescenzi (2018, 79) calls ‘competence in cooperative contexts’ and therefore offer a useful indicator of cooperative intent that we expect to increase a state’s credible cooperative reputation.<sup>6</sup>

More specifically, we expect that when a state enters into a ceasefire with a non-state group they generate a more credible reputation for cooperation with other non-state groups not involved in the ceasefire, as simply declaring a ceasefire marks a significant shift from prior conflictual interactions. This additional information increases the likelihood that other non-state groups will respond cooperatively towards the state.

This argument differs somewhat from prior research focused upon the state’s reputation for resolve. The ‘resolve’ literature focuses mainly on how states’ actions influence the behaviour of other *potential* challengers, and say far less about the reactions of other already *active* armed groups (e.g. Bormann and Savun 2018; Walter 2006a, 2006b).<sup>7</sup> In this sense, our argument is not directly comparable to this prior work, as we are concerned with how state actions towards one *active* armed group influences the behaviour of other *active* armed groups.<sup>8</sup>

We envisage two potential pathways for indirect reciprocal cooperation: Firstly, other non-state groups may be more willing to enter into a ceasefire with the state, as the proxy group has demonstrated that the state is likely to cooperate in this endeavor. However, ceasefires can take time to negotiate, and are not the only option available for a non-state group to reciprocate. Secondly, the group might instead choose a de-facto ceasefire, or de-escalation of the conflict. Thus, rather than immediately enter into another agreement, the non-state group could instead signal their openness to consider cooperative interactions through a de-escalation of violence. Thus, like conflict begets conflict (Walter 2004), peaceful gestures also beget further peaceful gestures, because previous cooperative actions signal a desire for cooperation and the capacity to deliver something tangible.

From this discussion we derive two hypotheses:

**Hypothesis 1a:** *During civil conflict, when the state enters into a ceasefire with one non-state conflict party, we expect an increased likelihood of other non-state groups entering into a ceasefire.*

**Hypothesis 1b:** *During civil conflict, when the state enters into a ceasefire with one non-state conflict party, we expect to observe a decrease in violence in other conflict dyads.*

Perception is a key facet of reputation, which is not easy for the state to control. Therefore, even when a state seeks to generate a cooperative reputation through good behavior (i.e. entering into a ceasefire), this might still be (rightly or wrongly) interpreted as a potential trap, or disingenuous move. While it may be difficult to identify

the underlying purpose of a ceasefire agreement *ex ante* (Clayton, Nathan, and Wiehler 2021), some ceasefires arguably signal more of a cooperative intent than others, and so are more likely generate cooperative reputation. Ceasefires are a diverse collection of agreements which vary depending on the context in which they are declared and the purpose they serve (Clayton et al. 2022c; Bara, Clayton, and Rustad 2021). We can make use of this variation between ceasefires (in terms of the signals they send) to examine our causal mechanism. If ceasefires beget ceasefires because they signal a government's cooperativeness, and willingness to move towards peaceful resolutions, then ceasefires that have the clearly stated intention of supporting a peace process should be more relevant for others trying to assess a state's reputation for cooperative behaviour than ceasefires that have a narrower and often time-limited purpose (such as facilitating elections or humanitarian aid).

***Hypothesis 2a:** During civil conflict, when the state enters into a peace process ceasefire with one non-state conflict party, we expect an increased likelihood of other non-state groups entering into ceasefire, compared to ceasefires with other stated purposes.*

***Hypothesis 2b:** During civil conflict, when the state enters into a peace process ceasefire with one non-state conflict party, we expect to observe a decrease in violence in other conflict dyads, compared to ceasefires with other stated purposes.*

The content of the ceasefire is also likely to shape how the arrangement is perceived by other actors. States may generate a more credible reputation for cooperation when they enter into more costly ceasefire commitments. Specifically, a mere cessation of hostilities without any provisions to monitor compliance should give less of a boost to a state's reputation than a ceasefire with compliance mechanisms, such as monitoring or verification. Monitored ceasefires represent a more significant and costly commitment from the state, they tend to be more durable, are more effective at helping to deescalate violence, and create structures to help build trust between the conflict parties (Buchanan, Clayton, and Ramsbotham 2021; Clayton, and Sticher 2021; Fortna, 2004). We therefore expect that ceasefires with compliance are more likely to influence reputation, and therefore increase the likelihood of indirect reciprocity.

***Hypothesis 3a:** During civil conflict, when the state enters into a ceasefire with compliance provisions with one non-state conflict party, we expect an increased likelihood of other non-state groups entering into ceasefire, compared to ceasefires without compliance provisions.*

***Hypothesis 3b:** During civil conflict, when the state enters into a ceasefire with compliance provisions with one non-state conflict party, we expect to observe a decrease in violence in other conflict dyads, compared to ceasefires without compliance provisions.*

When a ceasefire in one dyad is followed by a ceasefire in another dyad, it could be argued that rather than being the result of something that the rebels learned about the



government's reputation from the earlier ceasefire, it is instead that the same factors that motivated a government to sign the first ceasefire also motivated it to seek the subsequent ceasefire. There are many reasons that might motivate a state to seek a ceasefire (Clayton et al. 2022c), and it is possible that the factors that increase a state's willingness to enter a ceasefire might extend to more than one group concurrently.

Yet a state-sided explanation cannot fully explain the behaviour of the non-state groups. Our argument seeks to explain why non-state groups, not the state, opt to enter into a ceasefire. It is of course possible that the state offers the same incentives to a number of non-state groups to enter into a ceasefire, and that this provides a strong motivation for both the initial and subsequent non-state group to enter into an agreement. However, after one group has entered into a ceasefire, other actors are still likely to observe the states behaviour, update their views on the state's reputation, and use this information to assess any arrangement offered by the state. Meaning that reputation should still here play a role.

This does however suggest another important implication: beyond simply entering into a ceasefire, the outcome of the arrangement is likely to be pivotal, with 'successful' ceasefires more likely to increase the state's cooperative reputation, and thus raise the likelihood of subsequent ceasefire and cooperative reciprocation. This is similar to Quinn et al. (2019), who showed that when the state demonstrates a reputation for honoring agreements, this is more likely to lead to more actors entering into agreements. In contrast, if the state defects and reneges on its cooperative commitments to other actors, it may well generate a reputation for cheating, that might make future cooperation more challenging (Axelrod and Hamilton 1981; Joshi and Quinn 2016; Pollock and Dugatkin 1992). It is of course possible that ceasefires breakdown due to the challenging context (Braithwaite and Butcher 2022), and that even a 'failed' ceasefire in a challenging context might improve cooperative reputation. But in the event that an agreement fails very quickly, or does little to stop violence, it seems plausible to expect secondary groups to draw more cautious conclusions on the cooperative behaviour of the state if offered similar incentives to enter into a ceasefire. Given the huge variation in ceasefire arrangements it is challenging to identify general criterions of success (Clayton, Nathan, and Wiehler 2021). However, we can assume that in most cases a reasonable metric of success is length of time that the agreement endures. We therefore expect that ceasefires that last at least for some minimal duration impact more positively on cooperative reputation, and so are more likely to be followed by subsequent ceasefires or violent de-escalation.

***Hypothesis 4a:** During civil conflict, a more durable ceasefire between a state and one non-state conflict party increases the likelihood of other non-state groups entering into ceasefire with the state, compared to very short-lived ceasefires.*

***Hypothesis 4b:** During civil conflict, a more durable ceasefire between a state and one non-state conflict party increases the likelihood of other non-state groups decreasing violence in other conflict dyads, compared to very short-lived ceasefires.*

## Research Design

We test our hypotheses with a time-series cross-sectional research design, using dyadic conflict data and the new Civil Conflict CeaseFire (CF) Dataset (Clayton et al. 2022a). Our *sample* are all civil war dyads (i.e. one rebel group fighting the government) in the Uppsala Conflict Data Program (UCDP) Dyadic Dataset, v.19.1 (Harbom, Melander, and Wallensteen 2008; Pettersson, Högladh, and Öberg 2019) that were ever active between 1989 and 2018.<sup>9</sup> We analyse each dyad from the month an episode of fighting starts until 2 years after it ends, or until fighting resumes. We include these post-war months both because they are often just interludes between two periods of fighting, but also because dropping them could bias our results, for if fighting in one dyad ends because of a ceasefire, we cannot just drop this dyad the moment this happens, else we cannot test our main theoretical expectation, namely that this ceasefire may have an impact on the likelihood of de-escalation in other dyads down the road. We observe the dyads monthly. Our unit of analysis is therefore a dyad-episode, and our unit of observation a dyad-episode-month. Because our theoretical argument only applies to contexts in which governments are fighting multiple insurgencies, we limit our analyses to times at which more than one dyad was active (or in the two post-war years) in a country.<sup>10</sup>

## Dependent Variable

The dependent variable for the ceasefire hypotheses (i.e. 1a, 2a, 3a, 4a) is a binary variable that records whether the non-state (rebel) group of a dyad entered into a ceasefire agreement in the analysis month. The data is from the CF dataset. We consider unilateral ceasefires by the non-state group, and bi/multilateral ceasefires between the non-state group and the government. What matters for the dependent variable is whether *the non-state group* responds cooperatively towards the state as a result of a state's prior interaction with other rebels in the country, hence unilateral ceasefires by the state are not included in the dependent variable. The dependent variable for the de-escalation hypotheses (i.e. 1b, 2b, 3b, 4b) is a monthly count of battle deaths in a dyad. This data is from the UCDP GED Global Version 19.1 (Sundberg and Melander 2013). With this data, it is of course not possible to say whether it was the non-state group that *initiated* violence de-escalation (or escalation), but we can assume that an overall reduction in battle-field violence in a dyad requires at the minimum a willingness for restraint on the side of the non-state actor, which is in line with our theoretical expectations.

## Independent Variable

At the heart of our theoretical argument is the idea that there is interdependence between dyads, i.e. that the behaviour of armed actors is influenced by what happens in other dyads in a country (see also, Metternich and Wucherpfennig 2020). Our independent variable is a binary that records whether the government recently

entered into a ceasefire agreement in at least one *other* “neighbouring” dyad i.e. any other conflict dyad in the country.

For the models that have ceasefire (by the non-state conflict party) as a dependent variable, this independent variable is very similar to a Temporally Lagged Spatial Lag (TLSL), because ceasefires in one dyad are influenced by earlier ceasefires in other dyads (and in turn influence the likelihood of later ceasefires in those dyads again). Temporally lagged spatial lags are often used to test diffusion processes (Droic, Gandrud, and Williams 2019). Olar (2019), for instance, uses TLSL to show that autocracies adjust their levels of repression based on observed levels of repression by similar regimes; other authors have no theoretical interest in the TLSL but use it to control for spatial dependency (e.g. Basedau and Pierskalla 2014; Crabtree, Darmofal, and Kern 2015). We have a theoretical interest, but our independent variable is not an “exact” TLSL variable. Firstly, while our dependent variable requires that the *non-state actor* in a dyad is part of a ceasefire, our independent variable requires that the *government* was part of ceasefires in neighbouring dyads. It is only for bilateral ceasefires that our IV is really a TLSL. Secondly, TLSL are often operationalized as the sum or average of earlier outcomes in neighbouring units, while our independent variable takes on a value of one if at least one neighbouring dyad recently had a ceasefire. This is because ceasefires are rare, and the sum does not differ much from our binary specification.<sup>11</sup> We control for the number of dyads to account for the increased likelihood of a ceasefire when there are more dyads. We use the same independent variable to test the de-escalation hypotheses. In this situation, our independent variable is not a TLSL, because the dependent variable is battle-deaths.

In terms of the temporal lag, our main models focus on ceasefires in neighbouring dyads in the previous 6 months, but we also test three- and 12-month windows. Moreover, the main models include a time decay. That is, the independent variable takes on the value of 1 if a ceasefire agreement was signed in a neighbouring dyad in the previous month, but this value linearly reduces until it reaches 0 for cases in which the last ceasefire is more than 6 months ago. While reputation is probably a feature that is gained over repeated and longer-term behaviours, the decay accounts for the likely diminishing effect of events over time.

We also control for ceasefires in neighbouring dyads to which the government was not a party. Very importantly, if the effect of ceasefires on non-signatory rebel groups indeed stems from the state’s reputation, we should not see an effect of these rebel-only ceasefires.

To test hypotheses 2 through 4, we make use of the detailed manner in which each ceasefire in the CF dataset is coded. We use the *p\_peaceprocess* variable to distinguish ceasefires related to peace processes from ceasefires that had other purposes (humanitarian ceasefires, election ceasefires, holiday ceasefires and other). With the *ceasefire\_class* variable we separate ceasefires that include mechanisms for compliance from weaker ceasefires that include no provision to monitor or ensure compliance.

Ceasefire duration or success is a multifaceted concept that is hard to capture systematically (Clayton, Nathan, and Wiehler 2021). To test hypotheses 4a and 4b we focus on a

**Table 1.** Frequency of Ceasefire Types, 1989-2018.

Variable	N	Proportion	TLSL
Any ceasefire	25,932	4.09%	20.26%
Government ceasefire	25,932	3.07%	15.94%
Peace process ceasefire	25,932	2.30%	11.69%
Ceasefire with compliance	25,932	1.15%	5.41%
Longer ceasefire (>1md)	25,932	2.47%	13.63%

relatively simple and observable indicator of success, namely whether the ceasefire lasted past the month in which it was declared, using the ceasefire *end date* from the CF dataset.<sup>12</sup> The end date variable codes the date on which a ceasefire ended based on the reported statements of the conflict parties and/or international actors. More than half of all ceasefires “fail” in the very month they are declared, and so this seems a valid proxy for a minimal level of success. One could think of other measures of success, especially a longer-term suspension of violence, or the achievement of some particular objective (e.g. peace talks). However, non-state conflict parties judging a government’s cooperative reputation do not necessarily have that long-term perspective. Recall that we look at the impact of a government ceasefire within a window of between one and 6 months. In the first month after the government enters into ceasefire with a rebel group, other groups do not know how it will develop in the long run, but they will know if it failed almost immediately, hence our focus on short-term failure. We reserve for future research a more detailed analysis of how different conceptions of ceasefire success might influence reputation.

Table 1 shows how frequent these different types of ceasefires are in our data. The column *proportion* shows how many percent of observations (dyad-months) have a particular type of ceasefire. Overall, only about 4% of dyad-months are ceasefire months, but a large share of these are ceasefires to which the government is part. Among these government ceasefires, peace process ceasefires are common, while successful ones are unfortunately rare. The column *TLSL* shows the frequency of the TLSL. This indicates how often our independent variable takes on a value of one, i.e. at least one neighbouring dyad entered into a particular type of ceasefire agreement in the past 6 months. Because some countries have up to 10 dyads active simultaneously, government ceasefires in the neighbourhood that give non-state groups an opportunity to observe the cooperative behaviour of the government are not so rare, and happen in almost 16% of dyad-months.

### Models and Control Variables

We use a logistic regression model with dyad-episode fixed effects to test the hypotheses with ceasefire onset as a dependent variable, and a negative binomial regression model to test the de-escalation hypotheses.<sup>13</sup> The fixed effects account for any

unobserved differences between dyads and most closely model the before-after claim made in our hypotheses.

While time-constant differences between dyads are thus accounted for, we still need to control for dynamic confounders that may influence the dynamics of violence or the likelihood of a ceasefire in a dyad. Because our independent and dependent variables refer to different dyads (the analysis dyad and its neighbours, respectively), only factors that affect multiple dyads are relevant, and we therefore primarily look at time-varying country-level factors.

Firstly, the presence of other challenges is likely to impact government behaviour (Braithwaite and Butcher 2022), we therefore control for whether there was a new war onset or the entry of a new dyad into an existing conflict in the country in the half year before the first possible neighbouring ceasefire date ( $t_{12}$  to  $t_7$ ).<sup>14</sup> This accounts for the presence new challengers that might shape the government's behaviour towards existing rebels. Data is from the UCDP Dyadic Dataset, v.19.1 (Harbom, Melander, and Wallensteen 2008; Pettersson, Högbladh, and Öberg 2019).

Secondly, we account for government leadership changes that might shift behaviour towards non-state groups by including a variable coded 1 for all months in a year during which a new head of government came into power, no matter what the regime type, using data from V-DEM v.10 (Coppedge et al. 2019; Pemstein et al. 2019).<sup>15</sup>

Thirdly, we include a binary variable of whether a peacekeeping mission was present in the country in the half year before the first possible neighbouring ceasefire date ( $t_{12}$  to  $t_7$ ). Peacekeepers may mitigate violence levels and potentially provide mediation support and logistics that might make a ceasefire more feasible (Duursma 2022).

We use data by Bara and Hultman (2020), which include not only UN, but non-UN missions. The original data covers only 1993-2016, so we extend the variable back to 1989 using lists by Bellamy and Williams (2015) and Mullenbach (2013) and forward to 2018 using the Yearbooks by the Stockholm International Peace Research Institute (2020).

Fourthly, for the ceasefire hypotheses, we also include the natural log of average monthly battle deaths in the analysis dyad in the past half year, as violence levels will be one likely dyad-specific explanation for why a dyad enters into a ceasefire or not. Similarly, for our de-escalation hypotheses we include a binary of whether a ceasefire came into effect in the dyad in the past half year, as this would likely affect the dynamics of violence in that conflict. We are aware that given the interdependency of dyads that is at the heart of our theoretical argument, both of these control variables may actually be influenced by our independent variable (cessafires in the neighbourhood), possibly making them mediator variables rather than confounders. We therefore also test our hypotheses without these variables.

Fifthly, we control for how many other cessafires with the government a dyad has already had. Looking to the state's behavior with other groups is arguably more important for rebels that do not have their own lessons from a history of cooperation attempts to draw upon (Crescenzi 2018; Sechser 2010).

Finally, we account for time dependence within dyads by including a cubic polynomial of analysis time in all models (Carter and Signorino 2010).

## Results

Table 2 presents the results for the main hypothesis tests (H1). Models 1 and 2 test hypothesis 1a, Models 3 and 4 hypothesis 1b. There is support for both hypotheses both with and without the confounders. First, non-state conflict parties are more likely to agree to a ceasefire when the state has recently entered into a ceasefire with another non-state group (H1a). Second, such ceasefires also have the potential to de-escalate *violence* in dyads that are not part of the ceasefire (H1b).<sup>16</sup> The effects are substantial. Expressed in odds ratios (OR = 1.458), rebel groups are 46% more likely to enter into ceasefire shortly after government ceasefires compared to the baseline. And in terms of violence de-escalation, we can expect a 25% reduction in battle deaths in a dyad in the 6 months after a government ceasefire in other dyads.<sup>17</sup>

Equally important, there is no evidence that ceasefires to which the government is *not* part have a positive effect on the cooperative behaviour of other non-state groups. If at all, the opposite is the case (Models 3 and 4). This is important because our argument is not that ceasefires per se impact the behaviour of non-signatory actors in other dyads. It is cooperative reputation developed through the ceasefire which induces others cooperative behaviour. Had we found that all ceasefires have this effect, independent of whether the government is a party or not, this would have cast doubt on the causal mechanism proposed in our argument.

The results in Table 3 present estimates for different ceasefire types. The findings largely support hypotheses 2-4, lending further support to our theoretical argument. Only government ceasefires with the stated purpose of supporting a peace process make ceasefires or violence de-escalation in other dyads more likely, but not ceasefires that were signed for other purposes (Models 5 and 6). Furthermore, less “costly” government ceasefires that contain no provisions to ensure compliance do not increase the likelihood that other rebels in the country declare a ceasefire, while ceasefires with compliance provisions have this effect (Model 7). However, focusing on de-escalation as a measure of non-state group cooperative behaviour, this difference is less clear, as ceasefires with and without compliance mechanisms both seem to have the predicted effect, though the substantive effect is much larger for ceasefire with compliance (Model 8).

Finally, we also argued that actual compliance with a ceasefire has an impact on a state’s reputation. While the break-down of a ceasefire is not necessarily the government’s fault, we can assume that a cooperative reputation is less likely to develop when ceasefires fail to reduce violence on the battlefield. And indeed, Models 9 and 10 show that very short ceasefires, that is, ceasefires that ended in the very month in which they were signed, are not associated with more cooperative behaviour in neighbouring dyads, while longer ceasefires are.

We take these results as clear support for our hypotheses, and the theoretical argument that when states enter into and honour relatively costly ceasefires with one rebel group, they demonstrate a credible reputation for cooperation, which can help to

**Table 2.** Estimates for Government Ceasefires, 1989-2018.

	(1)	(2)	(3)	(4)
	DV: CF	DV: CF	DV: Violence	DV: Violence
<b>Government CF in other dyads</b>	<b>0.29*</b> <b>(0.17)</b>	<b>0.38**</b> <b>(0.18)</b>	<b>-0.37***</b> <b>(0.04)</b>	<b>-0.27***</b> <b>(0.04)</b>
CF without gov. in other dyads	0.14 (0.27)	0.23 (0.28)	0.10* (0.05)	0.12** (0.05)
New conflict/challenger		0.03 (0.11)		0.03 (0.03)
Change in government		0.10 (0.12)		0.01 (0.03)
Peacekeeping		0.00 (0.18)		-0.25*** (0.03)
Violence in dyad past 6 months (ln)		0.17*** (0.02)		
CF in dyad past 6 months				0.21*** (0.03)
Number of prev. gov. CF in dyad		-0.03* (0.02)		-0.03*** (0.00)
Number of dyads in the country	-0.19*** (0.05)	-0.14** (0.05)	0.04*** (0.01)	0.04*** (0.01)
Time	-0.05*** (0.01)	-0.02*** (0.01)	-0.01*** (0.00)	-0.01*** (0.00)
Time <sup>2</sup>	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)
Time <sup>3</sup>	-0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)
Intercept			-2.01*** (0.04)	-1.82*** (0.05)
N	9406	8714	19,153	17,393

Note: Standard errors in parentheses. \* =  $p < 0.1$ ; \*\* =  $p < 0.5$ ; \*\*\* =  $p < 0.01$ . Models 1/2: Logistic regression. Models 3/4: Negative binomial regression.

resolve information asymmetries and so increase the likelihood of other conflict parties entering into a ceasefire and deescalating violence towards with the state.

### Robustness of Results

We run a number of tests to assess the sensitivity of our results to alternative model specifications. Firstly, we test all models with a time-constant independent variable instead of the time decay, and we also use three- and 12-month windows instead of the 6-month window. We do not necessarily expect our results to be fully robust to these

**Table 3.** Estimates for Different Government Ceasefire Types, 1989-2018.

	(5)	(6)	(7)	(8)	(9)	(10)
	DV: CF	DV: Viol.	DV: CF	DV: Viol.	DV: CF	DV: Viol.
<b>Peace process CF</b>	<b>0.80***</b> (0.21)	<b>-0.33***</b> (0.05)				
<b>Other purpose CF</b>	<b>-0.09</b> (0.29)	<b>-0.06</b> (0.07)				
<b>CF with compliance</b>			<b>0.82***</b> (0.27)	<b>-0.34***</b> (0.07)		
<b>CF without compliance</b>			<b>0.09</b> (0.22)	<b>-0.19***</b> (0.05)		
<b>Longer CF</b>					<b>0.61***</b> (0.20)	<b>-0.26***</b> (0.05)
<b>Shorter CF (&lt;1md)</b>					<b>-0.09</b> (0.33)	<b>-0.12</b> (0.08)
CF without government	0.27 (0.28)	0.09* (0.05)	0.30 (0.28)	0.09* (0.05)	0.29 (0.28)	0.09* (0.05)
New conflict/challenger	0.02 (0.11)	0.03 (0.03)	0.03 (0.11)	0.03 (0.03)	0.02 (0.11)	0.03 (0.03)
Change in government	0.08 (0.12)	0.01 (0.03)	0.10 (0.12)	0.01 (0.03)	0.08 (0.12)	0.01 (0.03)
Peacekeeping	0.01 (0.18)	<b>-0.26***</b> (0.03)	0.02 (0.18)	<b>-0.26***</b> (0.03)	-0.00 (0.18)	<b>-0.26***</b> (0.03)
Violence in dyad past 6 months (ln)	<b>0.18***</b> (0.02)		<b>0.18***</b> (0.02)		<b>0.18***</b> (0.02)	
CF in dyad past 6 months		<b>0.21***</b> (0.03)		<b>0.21***</b> (0.03)		<b>0.21***</b> (0.03)
Number of prev. gov. CF in dyad	<b>-0.03*</b> (0.02)	<b>-0.03***</b> (0.00)	<b>-0.03*</b> (0.02)	<b>-0.03***</b> (0.00)	<b>-0.03*</b> (0.02)	<b>-0.03***</b> (0.00)
Number of dyads in the country	<b>-0.14***</b> (0.05)	<b>0.04***</b> (0.01)	<b>-0.14**</b> (0.05)	<b>0.04***</b> (0.01)	<b>-0.14**</b> (0.05)	<b>0.04***</b> (0.01)
Time	<b>-0.02***</b> (0.01)	<b>-0.01***</b> (0.00)	<b>-0.02***</b> (0.01)	<b>-0.01***</b> (0.00)	<b>-0.02***</b> (0.01)	<b>-0.01***</b> (0.00)
Time <sup>2</sup>	<b>0.00***</b> (0.00)	<b>0.00***</b> (0.00)	<b>0.00***</b> (0.00)	<b>0.00***</b> (0.00)	<b>0.00***</b> (0.00)	<b>0.00***</b> (0.00)
Time <sup>3</sup>	<b>-0.00***</b> (0.00)	<b>-0.00***</b> (0.00)	<b>-0.00***</b> (0.00)	<b>-0.00***</b> (0.00)	<b>-0.00***</b> (0.00)	<b>-0.00***</b> (0.00)
Intercept		<b>-1.82***</b> (0.05)		<b>-1.82***</b> (0.05)		<b>-1.82***</b> (0.05)
N	8714	17,393	8714	17,393	8714	17,393

Note: Standard errors in parentheses. \* =  $p < 0.1$ ; \*\* =  $p < 0.05$ ; \*\*\* =  $p < 0.01$ . Models 5/7/9: Logistic regression. Models 6/8/10: Negative binomial regression.



alternatives, but see these tests as a glimpse into the temporal dynamics of reputation-building and the signaling of peaceful intent.

Secondly, as outlined further above, we exclude the potential mediator variables from the models. Specifically, we exclude the battle deaths variable from the models that have ceasefire as an outcome, and a dyad's own ceasefire variable from the models that have battle deaths as an outcome.

Thirdly, we test whether the results change if we include all active dyads – not just those in multi-actor contexts – to make sure that we do not introduce bias by dropping countries and conflicts with only one active rebel group. By definition, the independent variable in these dyads can only take on the value of zero, i.e. no ceasefire in other dyads, and we accordingly do not expect massive changes to the results here.

Fourthly, we add an additional control variable, namely whether mediation was going on in the country in the half year before the first possible neighbouring ceasefire date ( $t_{-12}$  to  $t_{-7}$ ). As with peacekeeping, mediation will make the conclusion of a ceasefire in the country more likely, and it may influence the dynamics of violence in other dyads in either direction (Duursma 2022; Clayton 2013) Perhaps the increased international attention incentivizes restraint even among non-state armed groups that are not part of the mediation effort. At the same time, armed groups often use violence to get a foot in a peace process, which may lead to escalation. The reason this variable is not in the main models is that the Civil War Mediation Dataset (DeRouen, Bercovitch, and Pospieszna 2011) is only available up until the year 2012, leaving us with many missing observations.

Fifthly, we test the government change confounder, which is an annual variable, with a 1-year lag to make sure that government change actually happens before any changes to the independent or dependent variable.

Sixthly, we test our hypotheses on the conflict rather than the country level. This means that we assume that rebels primarily take the state's actions towards other dyads in the same conflict over the same incompatibility (not the whole country) into account. There is the possibility that non-state groups in one conflict find the state's behavior towards rebel groups that have very different ideologies and/or aspirations not a suitable proxy for how the state might behave with them. If this is the case, the findings on the conflict-level should actually be stronger than if government ceasefires all over the country are considered.

Table 4 reports the outcome of these tests for the models that have ceasefire as a dependent variable, Table 5 for the de-escalation models.<sup>18</sup> The columns are the different models, the rows the robustness tests. A star indicates that the results are fully robust, i.e. either statistically significant and going in the same direction as the main results, or not statistically significant neither in the main model nor the robustness check. A plus or minus sign indicates a change to the statistical significance of the main results. If the estimate in the main model is not statistically significant but becomes so in the robustness check, while going in the same direction, we indicate this with a plus

**Table 4.** Results of Robustness Tests for DV Ceasefire.

Model:	2	5	7	9
Type of Ceasefire:	Gov.	Peace process	Other purpose	Compl. No compl. Long Short
IV: No time decay (6md)	*	*	*	* * * *
IV: 3-month window	-	*	*	* * * *
IV: 12-month window	-	*	*	* * * *
Drop pot. mediator vars.	-	*	*	* * * *
Sample w/single dyads	*	*	*	* * * *
Control for mediation	*	*	*	* * * *
Gov. change: 1-y lag	*	*	*	* * * *
Connectivity = Same conflict	-	*	*	* * * *

Note: \* = comparable results; + becomes statistically significant ( $p < 0.1$ ); - no longer statistically significant ( $p < 0.1$ ); ! direction of effect changes (if statistically significant on  $p < 0.1$ ).

sign. If an estimate in the main model is statistically significant but no longer so in the robustness check, we indicate this with a minus sign.

As even a quick glance at the robustness tables shows, our results are very stable. However, there are two findings worth discussing. The first is that the coefficient for government ceasefires in Model 2 is not statistically significant in all tests (see Table 4). This is important to note because this is our main test of the first hypothesis that government ceasefires in other dyads increase the likelihood that a non-state group also enters into ceasefire. However, support for this hypothesis is still strong across the board of all robustness tests as soon as we distinguish between stronger (costlier) and weaker government ceasefires based on their purpose, compliance mechanisms, and duration. The conclusion we draw is: Ceasefires do beget ceasefires, but we are most certain about this relationship for the type of ceasefires in which the government signals the most costly cooperative behavior, in line with our theory.

The second finding worth discussing concerns the models that have de-escalation as a dependent variable (Table 5). While there is robust evidence for our main claim that a government ceasefire tends to lead to violence de-escalation in other dyads, support for our suggested mechanism is weaker here. In the robustness tests, even very short ceasefires and ceasefires with other purposes than supporting a peace process occasionally have statistically significant violence-mitigating effect. Hence, while ceasefires to which the government is *not* part definitely have no violence-mitigating effect on other dyads, some of the robustness tests suggest that there is not much of a difference in impact between strong/costly and weaker government ceasefires. Based on these results, we believe it possible that an additional *mechanism* may also be at work to explain the demonstrated relationship

**Table 5.** Results of robustness tests for DV De-escalation.

Model:	4	6	8	10
Type of Ceasefire:	Gov.	Peace process	Other purpose	Compl. No compl. Long Short
IV: No time decay (6md)	*	*	+	* * * *
IV: 3-month window	*	*	*	* * * *
IV: 12-month window	*	*	+	* * * *
Drop pot. mediator vars.	*	*	+	* * * *
Sample w/single dyads	*	*	*	* * * *
Control for mediation	*	*	*	* * * *
Gov. change: 1-y lag	*	*	*	* * * *
Connectivity = Same conflict	*	*	*	* * * *

Note: \* = comparable results; + becomes statistically significant ( $p < 0.1$ ); - no longer statistically significant ( $p < 0.1$ ); ! direction of effect changes (if statistically significant on  $p < 0.1$ ).

between government ceasefires in one dyad and violence de-escalation in other dyads.

## Conclusions

Reputation has long been understood to shape inter-state bargaining, but has been largely overlooked in studies of civil war. What little focus has been given to reputation in civil conflict focuses almost exclusively on the state's efforts to develop a reputation for resolve. We show that states can also develop a reputation for cooperation, and that this can be an important, and influential, source of information for other armed groups. When the state enters into a ceasefire with one non-state group, they generate a reputation that increases the likelihood of other groups (indirectly) reciprocating. This also shows the important signaling function that ceasefires perform during a peace process, and that the characteristics and outcome of a ceasefire really matter (Bara, Clayton, and Rustad, 2021). More generally, we show that what happens in the state's interactions with one non-state actor is likely to condition the behavior of other non-state groups. Thus, conceptualizing civil war in such a way as to ignore the multiple interactions between the different actors and processes is problematic. We have shown the reciprocal benefits that a state receives on account of a cooperative reputation, but no conflict party would want to be seen as always giving into their opponents. This suggests a more nuanced picture in which a state might attempt to develop a reputation for rewarding cooperation but punishing violence, or being open to some forms of concession, but resolved on other issues. Future work should then continue to explore the independencies between concurrent civil conflicts, and identify other forms and sources of reputation that might also shape conflict parties attempts to find peace.

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## Supplemental Material

Supplemental material for this article is available online.

## Notes

1. This process is not unique to civil conflict, and is also often a feature of inter-state war. In the Cold War, for example, the United States and Soviet Union invested significant resources attempting to develop a credible reputation for being resolved, i.e. unwilling to concede to any of the wishes of their opponent. Indeed, deterrence theory is built on conflict parties developing a resolved and credible reputation with their adversary, in particular with regards to delivering on threats of violence (c.f. Mercer 1997; Schelling 1966).
2. Research focusing on conflict management efforts also reach similar conclusions (Greig and Regan 2008; Melin and Svensson 2009; Keels and Greig 2019).
3. This is normally discussed as a type of “demonstration effect”, meaning that the state’s response to one conflict party provides a demonstration to other similar parties about the costs and benefits of different approaches.
4. However, she also finds that when the state grants concessions to more united movements, they tend to last longer until violence returns or more concessions are needed.
5. This is based on the assumption that for the state there is not a suitable proxy against which they can consider the past behaviour of the non-state group. For non-state groups cannot develop a reputation by interacting with another state, but the state can develop a reputation by interacting with other ‘similar’ non-state groups.

6. It is often challenging to determine the ‘real’ underlying purpose of a ceasefire, as, for example, humanitarian ceasefires might be developed as an initial confidence building measure on the path towards more significant discussions. While peace process ceasefires might be used as a tool to score some military advantage.
7. A number of studies have, however, already explored how state concessions to one group influence subsequent behaviour see (Nilsson 2010; Quinn, Joshi and Melander 2019).
8. That said, the central tenant of ‘resolve’ based arguments is that states resist making concessions to avoid appearing weak. According to this logic, concessions that are made to one non-state groups should, if anything, lead states attempting to maintain a broader reputation for resolve to resist subsequent cooperative gestures and, if anything, escalate rather than limit violence in other dyads. In essence, almost the opposite to what we argue above. However, we do not here attempt to develop a theory of how a reputation for resolve might extend to other active armed groups, and offer this only as one possible interpretation by way of illustrating where our argument potentially differs.
9. All dyads in Syria are dropped from our analyses because information on the dependent variable is only available between 2016 and 2018 from a special version of the UCDP Georeferenced Dataset (GED). We use the beta version of the CF data, which covers the period 1989-2018.
10. We also test whether the results change if we include all dyads to make sure that we do not introduce bias by dropping countries and conflicts with only one active rebel group.
11. Specifically, whenever there was any ceasefire in neighbouring dyads in the past half year, it was just one ceasefire in 77% of instances; two ceasefires in another 17% of instances, and more than two ceasefires in 6% of instances.
12. If the end date is missing, it is unclear how long the ceasefire was in place, hence we coded these as ceasefires that did *not* fail within the month they were declared, assuming that if they clearly failed quickly, that would have been reported.
13. The negative binomial model is used because the dependent variable is overdispersed.
14. There is a time lag of between one and 6 months between the “treatment” (a ceasefire in in the neighbourhood) and the outcome in the analysis dyad. To mitigate concerns of post-treatment bias, we lag some of our confounders by half a year. That is, from the perspective of the dyad that is analysed, these confounders are measured over a half-year period from  $t_{12}$  to  $t_7$ .
15. We also test this variable with a 1-year lag, but the results do not substantively change.
16. Note that the results in Model 4 also seem to suggest that the opposite is the case for a dyad’s *own* ceasefires, i.e., the estimates for that control variable suggest that violence increases after a dyad has entered into a ceasefire agreement. However, we should refrain from this interpretation. Control variables should generally not be interpreted. These are included to get unbiased estimates for the main effects, and if we would have wanted unbiased estimates for the control variables, we would have needed to control for factors that confound their relationship with the dependent variable (Cinelli and Hazlett 2020). In this case, for instance, we would have needed to account for violence levels before the ceasefire, which are likely correlated with violence right after the ceasefire, partially explaining the result we find here.

17. From 0.18 to 0.14 battle deaths, holding all other covariates at their means. This is calculated using the margins command to estimate the predicted number of events, *assuming that the fixed effects are zero*, hence the low predicted battle-death numbers (given that mean battle deaths in the sample are 36). When using a negative binomial regression without fixed effects, the predicted number of events goes from 32 without a government ceasefire in other dyads to 15 with a government ceasefire in other dyads, i.e., a more than 50% reduction.
18. The full results tables for these tests are in the [Supplementary Appendix](#).

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