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RESEARCH ARTICLE

Does flexibility promote the implementation of civil war cease-fire agreements?

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Abstract

Transition from war to peace often leads to new challenges. Conflict scholars suggest that these challenges lead groups to be unable to commit credibly and suggest mechanisms for decreasing the fear of being the victim, and increasing the costs, of reneging. However, international law and international political economy scholars debate the utility of making agreements flexible. This paper argues that provisions intended to increase the flexibility of agreements are detrimental to implementation because they operate under the assumption that groups are in a repeated game, and because they can lead to an even more severe commitment problem. Using a newly collected dataset on civil war cease-fire agreements, duration analyses suggest agreements with more flexibility-enhancing provisions exhibit a higher likelihood of violations. Although provisions calling for third-party enforcement – a mechanism for reducing fear and increasing costs – seem to decrease the likelihood of violations, this effect disappears when flexibility-enhancing provisions are considered.

Keywords: Cease-fire; flexibility; implementation

1. Introduction

Scholars of international law argue that agreements are critical for peace. To the extent that agreements are actually purposeful endeavors by warring groups to overcome the problems of ending violence, particular design elements may increase or decrease the likelihood of successful implementation (Bell, 2006, 2008; Guzman, 2008). Civil war scholars have identified the information and commitment problems as critical obstacles to peaceful resolution and have examined whether specific features of agreements that are intended to help groups overcome these well-known problems actually do in fact increase the likelihood of successful implementation (Walter, 2002; Fortna, 2004; Hartzell and Hoddie, 2007; Jarstad and Nilsson, 2008; DeRouen *et al.*, 2009; Mattes and Savun, 2009, 2010). This line of research has contributed substantially to the movement of the field of civil war studies away from a debate about whether agreements matter, to a focus on the question of under what conditions they matter more or less, and how alternative mechanisms aimed at overcoming the information and commitment problems make successful implementation more or less likely.

While existing scholarship has furthered our understanding of the relationship between agreement design and implementation in myriad ways, several inherent weaknesses suggest avenues for improvement. First, previous studies on civil war agreement design, such as Walter (2002) and Hartzell and Hoddie (2007), tend to look at agreements that have already reached the final stages of the peace process, excluding various negotiated arrangements achieved outside the final bargaining attempts. However, in reality groups reach agreements – particularly cease-fire agreements – at various times during a peace process. Table 1 shows the distribution of the timing of cease-fire agreements in

Table 1. Distribution of the timing of cease-fire agreement from 1989 to 2008

Timing of cease-fire agreement	Frequency
During war	35
Date war ended	5
After war ended	10

Note: Data are from the Uppsala Conflict Data Program Armed Conflict Dataset (Gleditcsh *et al*., 2002).

relation to the timing of cessation of fighting. Among 50 civil war cease-fire agreements from 1989 to 2008, 35 (70%) were signed during the war, 5 (10%) were signed at the time fighting ceased, and 10 (20%) were signed after fighting ceased. This table indicates that if the factors affecting agreement formation at the final stages of the peace process are systematically related to the inclusion of specific provisions within preliminary arrangements, then existing empirical studies on the relationship between agreement design and agreement implementation are, at best, missing an important piece of the overall story. At worst, the failure to capture important components of the preliminary steps toward a final agreement may provide a misleading picture of how elements of agreements relate to their success. Any attempt to deal seriously with this issue should make a concerted effort to extract relevant information from all agreements, including those that do not take place at the final stages of negotiations between groups attempting to end war. As a first step, at least one contribution of this paper is that it focuses on cease-fire agreements reached at various stages of the peace process.

Second, previous civil war studies have focused on a very limited set of mechanisms within agreement provisions as being important for overcoming the commitment problem. In the context of civil war, some have emphasized the emergence of new challenges (e.g., power shifts, leadership changes, changes in public opinion) as often providing incentives for actors who are signatories to an agreement to change course and possibly renege. These scholars have tended to focus on groups' security concerns, leading naturally to a prescriptive emphasis on provisions that decrease this fear about the future behavior of opponents (e.g., Walter, 2002; Fortna, 2004; Hartzell and Hoddie, 2007; Jarstad and Nilsson, 2008; DeRouen *et al*., 2009; Mattes and Savun, 2009). By contrast, rather than focusing exclusively on these fear-reducing provisions, a large body of empirical work on international law and international political economy that deals with international agreements between states has debated the role of flexibility-enhancing provisions to cope with new challenges during the implementation phase. The debate here has to do with whether agreements should contain provisions allowing for the commitment to them, if necessary, to be relaxed temporarily on the path to implementation in order for signatories to adjust their behavior to new challenges during the implementation phase (e.g., Abbott and Snidal, 2000; Koremenos, 2001, 2005; Koremenos *et al*., 2001; Rosendorff and Milner, 2001; Kucik and Reinhardt, 2008; Pelc, 2009; Thompson, 2010). A further contribution of this paper is to explore the applicability of this proposed mechanism, thus far understudied by civil war scholars, to civil war cease-fire agreement implementation.

The paper proceeds as follows. In Section 2, I briefly discuss previous research on the implementation of civil war cease-fire agreements. In Section 3, I discuss the implications for the civil war context of arguments from international law and international political economy about the importance of agreement features that either enhance or reduce flexibility. Following this, I apply those implications to an empirical examination of civil war cease-fire agreements from 1989 to 2008. The findings suggest that the inclusion of flexibility-enhancing provisions increases the likelihood of agreement violation, while provisions intended to reduce fear and provisions intended to enhance information flows have weak or null effects. Following the empirical analysis, I conclude with a discussion of the implications of these findings for future research.

2. Previous research

Scholars examining the consequences of agreement design have identified a variety of factors that lead to difficulty for successful implementation (e.g., Abbott and Snidal, 2000; Koremenos, 2001, 2005;

Koremenos *et al*., 2001; Rosendorff and Milner, 2001; Walter, 2002; McCaffrey, 2003; Fortna, 2004; Hartzell and Hoddie, 2007; Kucik and Reinhardt, 2008; Mattes and Savun, 2009; Mattes and Savun, 2010), with some concentrating on those factors falling under the general category of the information problem (e.g., Fortna, 2004; Mattes and Savun, 2010). In general, the information problem occurs because groups hold private information and have incentives to misrepresent this information to achieve a better deal at the bargaining table (Fearon, 1995).

However, in the context of civil war, the critical role of the information problem in explaining failures of cooperation has been debated. Specifically, some scholars argue that groups at the bargaining table do not have a severe information problem. The reasoning behind this argument is that while groups might initially go to war because of a lack of information about opponents, through war and bargaining groups gain more accurate information about their adversaries, and therefore the information problem diminishes over time (Wagner, 2000; Slantchev, 2003; Fearon, 2004; Powell, 2004). An implication of this reasoning is that the information problem should be less of a concern as groups fight longer wars, as groups fight more frequently, or as third parties involve themselves. Empirically, according to the data collected for this study, among all countries in the time period under examination, the vast majority of countries experienced a war longer than 10 years, or multiple wars among the same combatants. In many instances, the same signatories signed more than one agreement or a third party became involved.[1]

While there are conflicting arguments about the importance of the information problem once warring groups have already engaged in war, scholars generally agree on the important role of the commitment problem after war has occurred and bargaining has been initiated. Thus the role of the commitment problem and its implications for agreement design require more scrutiny. The next section will discuss why the commitment problem is important in the implementation phase of civil war cease-fire agreements, then look at the mechanisms that previous studies of civil war have emphasized as potential ways to overcome the commitment problem, and finally examine the efficacy of certain understudied mechanisms.

3. New challenges, the commitment problem, and flexibility

Signing agreements leads to transition from war to postwar peace, and environmental changes during that transition might lead to new challenges. Previous studies suggest various challenges groups might face when they attempt to implement a signed deal. First, a challenge could be triggered by an agreement itself. Among other arrangements, cease-fire agreements typically require disarmament and demobilization (DD) programs. In some instances, such programs specify only the demobilization and disarmament of rebel groups (e.g., Guatemala's Agreement on the Definitive Ceasefire signed on 4 December 1996), while in others they require demobilization and disarmament of both the government and rebel groups (e.g., Liberia's Cotonou Agreement signed on 25 July 1993). Depending on the specific details of DD programs specified in particular agreements (e.g., the group that must disarm and demobilize), power shifts after agreements will be unavoidable.

Second, politics within a group can cause new challenges during transition. While many studies of civil war take each warring group as a single homogeneous actor in the name of simplicity, in reality each group is necessarily a coalition of heterogeneous preferences. As such, the views of some coalition partners may not be aligned with the bargaining representatives. With respect to rebel groups, the method of acting toward the furtherance of their goals (e.g., violence vs negotiation) is the primary reason for the emergence of splinter groups. As negotiations are likely initiated by actors who are at least willing to attempt non-violent means of reaching objectives, power struggles between pro-violence groups and pro-negotiation groups can deteriorate. For example, the United Liberation

[1]In addition, reading news articles to gather data for this study, it was often the case that journalists would cite various people in the field (e.g., foreign officers, mediators, and business persons) to report the best possible estimates of power and willingness to fight, and their reports sometimes contrasted with warring groups' public claims. Such reports might provide additional information to combatants.

Front of Assam (ULFA) fought against the Government of India for Assam independence. Negotiation attempts in the early 1990s led to the emergence of different opinions about negotiations. After the ULFA declared a unilateral cease-fire in December of 1991 and agreed to amnesty from the government, pro-violence factions of the ULFA led by Paresh Baruah called for continued armed struggle until the Indian government conferred independence. Even after successful bargaining, the war continued and eventually led to the emergence of a splinter group calling itself ULFA-I (for Independence). In this example and others, the heterogeneous preferences of rebel groups affected the leadership that would have been charged with implementing a signed agreement.

With respect to the government, domestic groups such as opposition parties, the military, and the legislature can have widely varying preferences about the implementation of whatever agreement was signed. In some cases, for instance, the sitting government may face the resistance of the military. For example, the Sierra Leone government reached its first agreement with the Revolutionary United Front in November of 1996. After the agreement was signed by the representatives at the bargaining table, it became clear that the army was not on board with the agreement's terms, eventually leading to a coup in May of 1997 that made the agreement obsolete. Cases like Sierra Leon tell us that heterogeneous preferences within governments might lead to changes in governments' leadership.

Third, governments sometimes face criticisms about the terms reached at the bargaining table from groups that generally support them. For example, in 1990–1991, the government of El Salvador negotiated and reached multiple agreements with Farabundo Marti National Liberation Front (FMLN), including one cease-fire agreement. As the contents of the peace agreement became clear in November 1991, a political activist group, Crusade for Peace and Jobs, traditionally aligned with President Alfredo Cristiani's rightist Nationalist Republican Alliance Party, deeply criticized the president, going so far as to call him a traitor. Further, recently the Colombian government and the Revolutionary Armed Forces of Colombia signed a peace agreement in 2016, but the government faced a lack of public support, most prominently displayed when a popular referendum to ratify the terms of the agreement failed at the polls. While a variety of factors might affect a government's reaction to these forms of public resistance to agreements, low public support might lead governments to calculate that war termination through bargaining will not be a viable political solution.

These studies suggest that groups may face a variety of new challenges – power shift, leadership change, public opinion change – when they attempt to implement the negotiated terms of a cease-fire agreement. Scholars concern themselves with such situations, which the warring groups themselves likely could not have fully anticipated at the time of signing the agreement. According to previous studies, these new challenges can lead to a commitment problem (e.g., Fearon, 1998; Walter, 2002; Powell, 2004; Kirschner, 2010). Specifically, groups might not be able to credibly commit to keeping the terms of a negotiated settlement because some groups will have an incentive to renege on an agreement's terms as new challenges arise (Walter, 2002; Fortna, 2004; Hartzell and Hoddie, 2007; Jarstad and Nilsson, 2008; DeRouen *et al.*, 2009; Mattes and Savun, 2009).

To overcome the commitment problem, civil war scholars have emphasized efforts to reduce the fear of becoming the victim of reneging and increase the costs imposed on reneging behavior (e.g., Walter, 2002; Hartzell and Hoddie, 2007; Mattes and Savun, 2009). The basic logic here is that when groups face new challenges, some groups may feel insecurity and fear about the possible actions of their opponents. This is especially concerning for groups that experience a power decrease, or expect their power to be decreased, and hence see or expect to see a weakening of their bases of support. These groups might consider preventive reneging to be beneficial, while still being in a better position to cope with a reneging group. At the same time, other groups may consider new challenges as opportunities to improve their bargaining positions. With increased power and boosted political support, groups may think that reneging is a better option. Previous civil war termination studies suggest that provisions for institutions and third-party enforcement included in agreements can help to reduce fears of being the victim of reneging and increase the cost of reneging (e.g., Walter, 2002; Hartzell and Hoddie, 2007).

While civil war scholars exclusively emphasize mechanisms to reduce fear and increase the cost of reneging as important factors for overcoming the commitment problem, researchers in international

law and international political economy have debated mechanisms intended to enhance flexibility to better cope with new challenges (e.g., Abbott and Snidal, 2000; Koremenos, 2001, 2005; Rosendorff and Milner, 2001; McCaffrey, 2003; Kucik and Reinhardt, 2008; Thompson, 2010). Some suggest that enhancing flexibility can aid cooperation (i.e., implementation of an agreement's terms) among actors in the face of rising challenges (Abbott and Snidal, 2000; Koremenos, 2001, 2005; Koremenos *et al*., 2001; Rosendorff and Milner, 2001; McCaffrey, 2003; Kucik and Reinhardt, 2008; Thompson, 2010). The basic idea here is that when groups (typically states) know that there are some conditions under which the commitment to an agreement can be relaxed on the path to implementation in order for signatories to adjust their behavior to handle a new challenge during the implementation phase, they are also more likely to be cooperative with respect to implementation. Therefore, the inclusion of provisions that allow for the relaxing of some commitments might increase the likelihood that relevant groups will be sincere actors when working toward agreement drafting and implementation.

However, the applicability of flexibility-enhancing mechanisms requires further consideration. Specifically, some scholars including Koremenos *et al*. (2001) are more cautious about advocating flexibility-enhancing mechanisms to cope with new challenges for at least two reasons. First, scholars who recommend flexibility as a mechanism to cope with new challenges usually assume the possibility of repeated interactions in which both parties understand the long-term benefits of mutual commitment to the agreement alongside the short-term gains to be had by one party reneging (Downs and Rocke, 1995; Abbott and Snidal, 2000; McCaffrey, 2003; Kucik and Reinhardt, 2008).[2] Therefore, for researchers interested in emphasizing the role of flexibility, the issue revolves around how to design agreements in ways such that one-time, temporary, violations do not lead to spirals of violations and violence by adversaries. In the context of civil war, however, the implicit or explicit assumption of continuous interactions after one-time violations does not accurately capture the inherent risk embedded in being the victim of a violation. Existing studies point out that DD of warring groups is necessary to end civil war; furthermore, the necessity that some group, typically the rebel group if only one side must disarm and demobilize, must give up their military power leads to the time-inconsistency problem. According to previous studies (e.g., Walter, 2002; Glassmyer and Sambanis, 2008), warring groups tend to agree to a settlement because they believe a negotiated settlement to be beneficial in the present situation. However, when groups lose military power, they may not be able to defend themselves from a counterpart's one-time violation, and thus they may be unable to participate in future negotiations and fighting. That is, for many warring groups in civil wars, the risk of suffering a one-time violation from the counterpart, while they themselves remain committed to the terms of the deal, could be existential. This suggests that warring groups in civil war might weigh fewer of the long-term benefits than groups interacting in non-security situations.

Second, enhancing flexibility may serve to amplify the commitment problem. As stated above, cease-fire agreements often specify DD programs for at least some groups. DD programs alone are expected to lead groups to anticipate power shifts. Provisions that allow for further discussion of the details of agreements or for future negotiations on new or old unresolved issues open up the possibility that current terms may change. This generates an even more complex expected power shift and renders any original expected power shift obsolete. When groups are not certain whether current terms will be affected by future changes, they may passively refrain from implementing the current terms or even actively pursue renewed violence to gain bargaining leverage for an unspecified future settlement. All of the points of previous discussion lead naturally to the hypothesis that cease-fire agreements containing provisions that might enhance flexibility are less likely than those without such provisions to be successfully implemented.

[2]For instance, Kucik and Reinhardt (2008: 477–478) contend, 'The opportunity to temporarily escape from contractual obligations – without incurring excessive retaliation from other partners – may encourage states to enter into deeper cooperative agreements and sustain those commitments over time.'

I expect that provisions for enhancing flexibility in an agreement are mutually reinforcing. When there are more flexibility-enhancing provisions in a cease-fire agreement, those agreements are more likely to fail to be implemented because of increased uncertainty. For example, if a provision specifies an amendment of the lines of separation without provisions specifying that signatories are bound to the agreement (e.g., Croatia's Cease-fire Agreement signed on 29 March 1994), there should be a greater chance of failing than when an amendment mandate on constitutional, legislative, or regulatory provisions is present, with a provision specifying that groups are bound to the agreement (e.g., Burundi's Global Cease-fire Agreement signed on 16 November 2003). While an individual provision may enhance flexibility and thus increase uncertainty, having more such provisions in an agreement may lead to higher flexibility and thus increase the likelihood of agreement failure due to higher uncertainty.

4. Research design

To study the relationship between the design of cease-fire agreements and their implementation, I adopt the civil war cease-fire agreement as the unit of analysis. Prior to this study, comprehensive information about civil war cease-fire agreement provisions and flexibility-enhancing provisions was not publicly available. To gather this information, I relied primarily on the United Nations Peacemaker Database to collect the original text of cease-fire agreements drafted during the various stages of the civil war peace process.

According to the Uppsala Conflict Data Program (UCDP), at least one criterion that defines a civil war is the existence of a violent conflict between a government on one side and one or more rebel groups on the other. In this paper, I employ this same basic definition, but expand it to include cases in which violent conflict took place between a rebel group and an interim government (e.g., Liberia's Interim Government of National Unity in 1993), rather than the regular government. A further defining criterion is that war is considered to be terminated when a conflict fails to reach 25 casualties within a year. Again, I use this basic framework but expand its scope slightly. Specifically, some civil wars are not claimed by domestic political leaders (e.g., government or rebel leaders) or officials from international organizations (e.g., UN officers) to have been officially terminated until long after casualties have dropped below the 25-casualty-per-year threshold. As shown in Table 1, often, cease-fire agreements are signed during this gap between the end of significant casualties by the UCDP dataset and official pronouncements that the war has come to a close. Considering these criteria, I identified 50 cease-fire agreements signed and written in English during civil war peace process from 1989 to 2008.

4.1. Dependent variable

To investigate the conditions under which a civil war cease-fire agreement will be implemented successfully without violation, I define the starting date of an agreement in two ways. For one, like most previous civil war studies, I adopt the signing date as the starting date of an agreement. However, this could be problematic if signatories believe that the agreement takes effect on a date other than the signing date. This represents an important class of agreements since, empirically in the data used for this study, 24 out of 50 agreements have starting dates that differ from the date the agreement was signed. Specifically, many agreements specify that the agreement will take effect on a specific date, known as the entry into force (EIF) date, and some specify that the agreement enters into force as soon as it is signed, while others note a specific future date or time span after signing[3] or

[3]For example, Somalia's Agreement between the Transitional Federal Government of Somalia (TFG) and the Alliance for the Re-Liberation of Somalia (ARS) signed on 9 June 2008 specifies that 'The cessation of armed confrontation shall come into force thirty (30) days from the signing of this agreement throughout the national territory.' In this case, the signing date is 9 June 2008 while the EIF date is 9 July 2008.

a certain event[4] as the effective date of EIF. For cases that specify the signing date as the effective date and those that do not specify at all, I code the signing date as the EIF date.

After recording the agreement start date, I coded whether an agreement's terms were violated and, if so, the timing of the violation. To define a violation, I systematically searched the text of actual agreements to determine how the signatories themselves define the cease fire and what constitutes a violation. Through this process, it became clear that the activities that constitute violations can sometimes vary widely across agreements. However, the most common defining feature of a violation is the recurrence of armed military activity by signatories. Therefore, any definition of cease-fire agreement violation should include the occurrence of armed military activity.

Once it was determined whether a violation occurred, the timing of the violation was recorded as the first date that the signatory or signatories' armed personnel were involved in violence. This information was collected using news articles available through the Lexis-Nexis News Search Engine as the primary source and the UCDP Peace Agreement Database as the secondary source.[5]

4.2. Independent variables

4.2.1 Flexibility-enhancing

Cease-fire agreements have various provisions for influencing flexibility. I code for the presence of five types of flexibility-enhancing provisions. First, some cease-fire agreements stipulate amendment provisions so that the signatories can change the terms of the original agreement in order to cope with the new challenges that make sticking to the original terms a heavy burden. Amendment provisions appear in agreements in two ways: some are issue-specific and others apply to the agreement as a whole. As an example of the former, Croatia's Cease-fire Agreement on the lines of separation signed on 29 March 1994 states, 'The lines of separation shall be as drawn on maps by UNPROFOR and accepted by the parties. After separation is completed, these lines may be amended on the ground as proposed by UNPROFOR and accepted by the party concerned. Such a proposal may be based on suggestions from either of the parties.' And as an example of the latter, Sudan's Humanitarian Ceasefire Agreement on the Conflict in Darfur signed on 8 April 2004 states, 'This Agreement can be amended by agreement of the parties with the consent of the Cease-fire Commission.' With these considerations in mind, I constructed two versions of a binary variable: amendment provisions with respect to specific issues and amendment provisions for the agreement as a whole. There were nine agreements with amendment provisions on specific issues; 13 agreements with amendment provisions for the agreement as a whole.

Second, some agreements specify possible further discussions that will ostensibly provide signatories with the opportunity to alter the agreement in order to better deal with new challenges after it is reached. A significant number of the cease-fire agreements specify that further talks should take place on the details of and pending points from the current cease-fire agreement. For example, the El Salvadoran government and FMLN signed an agreement that took effect on 1 February 1992 that called for follow-up talks to discuss the details of the agreement, stating, 'A further meeting between the parties has been scheduled for 5 January 1992 to negotiate the timetable for implementing the agreements and the procedure for ending the military structure of the FMLN and reintegrating its

[4]For example, instead of stating a specific date, Tajikistan's Agreement on a Temporary Ceasefire and the Cessation of Other Hostile Acts on the Tajik–Afghan Border and within the Country for the Duration of the Talks that was signed on 17 September 1994 specifies that 'This Agreement…shall enter into force as soon as United Nations Observers are deployed in Tajikistan.' The UN Peacekeeping Operations webpage says, 'during October, 15 military observers arrived in Tajikistan and were deployed in Dushanbe, Garm, Kurgan-Tyube and Pyanj. The ceasefire came into effect from 0800 hours local time on 20 October 1994, following a public announcement by the Head of the United Nations office in Dushanbe.' Therefore, the EIF date was coded as 20 October 1994 while the signing date was coded as 17 September 1994.

[5]Reports whose only sources of information on armed military activity are unilateral claims by one side are not coded as violations. To be recorded as a violation, reports of returning to fighting must be confirmed by non-combatant actors or clarified by more than one of the parties engaged in the conflict.

members, within a framework of full legality, into the civil, political and institutional life of the country.' Other times, cease-fire agreements suggest talks to deal with new issues will follow. This happens because many cease-fire agreements are reached as a way to create an environment conducive to negotiating over some of the underlying issues of the conflict (e.g., political, economic, or territorial issues) in order to reach a permanent peace. For example, Liberia's Agreement on Ceasefire and Cessation of Hostilities between the Government of the Republic of Liberia (GOL), Liberians United for Reconciliation and Democracy (LURD), and the Movement for Democracy in Liberia (MODEL) that took effect on 17 June 2003 mentions various issues that should be discussed at the future bargaining table by stating, 'The signing of this agreement shall be followed immediately by the engagement of the GOL, LURD and MODEL with all other Liberian political parties and stakeholders in dialogue, to seek, within a period of thirty (30) days, a comprehensive peace agreement.' The peace agreement specifies that a future talk will cover issues like human rights, socio-economic reforms, formation of a transitional government, and elections. Cease-fire agreements were coded for whether they contained text explicitly specifying follow-up talks for the issues discussed in the current agreement (further talk) or a new round of talks about additional issues not discussed in the current round (future negotiation). There were 18 agreements with planned further talks and 23 agreements with planned future negotiations.

Third, some agreements specify whether the specific agreement is legally binding. For example, Ceasefire Agreement between the Transitional Government of Burundi and the Conseil National pour la Défense de la Démocratie-Forces pour la Défense de la Démocratie that was signed on 16 November 2003 states, 'The Transitional Government of the Republic of Burundi and the National Council for the Defence of Democracy-Forces for the Defence of Democracy (CNDD-FDD), hereinafter referred to as "the Parties"…solemnly declare ourselves to be bound by the provisions of the following Global Ceasefire Agreement.' When a civil war ends with any kind of negotiated settlement, the very act of signing an agreement with state actors means that a non-state actor is a subject of international law. However, it remains unclear whether an agreement between state and non-state actors is legally binding (Bell, 2008). By making a legally binding agreement, signatories assure that they are locked into 'a set of future relationships capable of implementing the peace agreement' (Bell, 2008: 161). Therefore, one can expect that when an agreement does not have a provision specifying that it is legally binding, the agreement will be more flexible and less likely to constrain groups' behavior during the implementation process. There were nine agreements with binding statements.

To capture the reinforcing nature of provisions, I created a count variable of flexibility-enhancing provisions that ranges from 0 (no flexibility: no provisions for amendment provision on a specific issue, no amendment provisions for the agreement as a whole, no provisions for planned further talks, no provisions for planned future negotiations, and provisions for a binding statement) to 5 (highest flexibility: amendment provisions for specific issues, amendment provisions for the agreement as a whole, provisions for planned further talks, provisions for planned future negotiations, and no provisions for a binding statement). There were two agreements with five flexibility provisions, five with four flexibility measures, 11 with three provisions, 14 with two provisions, and 13 with only one of the flexibility provisions present.

4.2.2 Fear reducing1 (Enforcement) and information flow (Nonenforcement)

As a way to resolve disputes, cease-fire agreements sometimes specify a role for the international community. Systematically examining cease-fire agreements led to the identification of roughly two broad roles for the international community interested in resolving the commitment and information problems. Specifically, cease-fire agreements sometimes specify that a third party should enforce the cease-fire agreement whenever there is a violation. Third-party enforcement is expected to decrease the fear of being the victim and increase the cost to pay for the offending party (e.g., Walter, 2002). When such provisions exist, the variable *Enforcement* is coded as 1, and 0 otherwise. Other times, agreements specify a process or rationale for third-party operations, but limit their role to monitoring and verification in investigations of cease-fire violation claims to help groups gather better

information (e.g., Mattes and Savun, 2010). When this type of provision exists, the variable *Nonenforcement* is coded as 1, and 0 otherwise.

4.2.3 Fear reducing2 (Joint Committee)

To decrease security concerns, some civil war scholars have emphasized the importance and existence of power-sharing institutions (e.g., Hartzell and Hoddie, 2007). In a cease-fire agreement setting, warring groups sometimes commit to establishing joint committees or commissions that include bargaining participants, and whose consensus is required to make political and/or military decisions related to agreement implementation. When such provisions are present, the variable *Joint Committee* is coded as 1, and 0 otherwise.

In addition to this primary set of variables, I consider several additional factors to account for potential confounding explanations. Some scholars have discussed the lack of random selection in studies of agreement design (Downs *et al*., 1996; Stein, 2005). According to this school of thought, rational actors will carefully consider the intent to comply before they sit down at the bargaining table, thereby spending resources on agreement design only when they are truly willing to adhere to an agreement's terms once reached. Similar to the managerial school of international law (e.g., Chayes and Chayes, 1993), this argument predicts that the very act of signing an agreement is a strong indication that the actors are likely to comply. In other words, the factors that led to the agreement itself may be the same that affect its outcome. Therefore, to better understand the conditions under which a cease-fire agreement will be complied with, one should also consider the conditions that lead participants to sign in the first place. To test this selection effect, I also coded for the existence of armed non-signatories, the regime type at the time of agreement signing, the number of signatories, and type of war.

The looming presence of armed non-signatories is a common feature of many cease-fire agreement negotiations. In total, 64% of cease-fire agreements in the current study experienced armed non-signatories. These warring groups outside of an agreement are known to be major spoilers of the peace process (e.g., Stedman, 1997). Given that these spoilers are more likely to be a threat to the success of an agreement, some groups may want to sign the agreement only when every warring group signs. These types of favorable environments for signing might also affect the environment for implementation. I systematically searched the text of each cease-fire agreement, *Military Balance* (Institute for Strategic Studies), Keesing's World News Archive, the UCDP database, and the Lexis-Nexis News Search Engine and coded for the presence or absence of armed non-signatories (1 or 0, respectively).

Additionally, scholars have argued that domestic political factors influence leaders' behavior during the bargaining process (e.g., Goemans, 2000). For example, under more democratic regimes, leaders may face more intense pressure to end the war from citizens and opposition groups. Therefore, they are more likely to sign an agreement and they are more likely to try to comply with it once signed. Similarly, when democratic regimes are signatories to an agreement, it is expected to be more likely to be successfully implemented. Using the 'Revised Combined Polity Score' from Polity IV (http://www.systemicpeace.org/polity/polity4.htm), I coded regimes' levels of democracy at the time a cease-fire agreement was signed. The measure ranges from −10 (most autocratic) to 10 (most democratic).

Further, agreements can be unilateral, bilateral, or multilateral. Previous studies suggest that each warring group can be a veto player in the decision-making process (e.g., Cunningham, 2006). Following this logic, the number of signatories could reasonably affect implementation. The prediction is that agreements with more signatories will be more difficult to implement than agreements with fewer signatories. On the contrary, one could also expect that the level of cooperation required to reach a multilateral deal will be more difficult to break and therefore multilateral signed agreements will be more likely to be implemented. To examine this, agreements were coded for the number of signatories who are internal to the civil war. This measure ranges from 1 to 22.

Some have argued that the type of war groups are fighting can affect the likelihood of a negotiated settlement. Specifically, scholars suggest that wars over government issues are more likely to threaten the existence of incumbent elites than wars over territorial issues, and thus wars over government

issues will make it less likely for groups to be willing to end war through negotiated settlement (e.g., Stedman, 1997). To test the effect of type of war on implementation failure, I coded a measure as 1 for war over government issues and 0 otherwise.

In addition, there is a strong possibility that factors that lead warring groups to include provisions with flexibility might also lead to a relatively short duration of time until failure of agreement implementation. In other words, when groups believe that they are compelled to sign half-hearted agreements, implementation of those agreements is more likely to fail. I argue that the timing of agreements provides warring groups with a means to determine whether they want to include flexibility provisions. As Table 1 shows, cease-fire agreements between groups are reached at various points: (1) during active war, (2) at the time fighting ceases, and (3) after fighting has ceased. And the point at which an agreement is reached has implications for its duration. For instance, when groups sign an agreement after war has ended (by definition, when a conflict falls below 25 casualties in a year), the likelihood that war will resume is expected to be lower than, say, when warring groups are actively engaged in war, or have just ceased fighting. Therefore, one can expect that when an agreement is reached after a war has ended, it is less likely to be interrupted. The timing of agreements is coded 1 if an agreement is signed after a war ends, and 0 otherwise. According to the dataset, when agreements are signed either during fighting or at the time fighting ceases (i.e., *Timing: After War* = 0), 37 agreements have at least one flexibility provision. In contrast, when agreements are signed after war ends (i.e., *Timing: After War* = 1), only eight agreements have at least one flexibility provision. This seems to suggest that the timing of agreements is correlated with the inclusion of flexibility provisions. While I included as controls factors that affect both the signing of agreements and the inclusion of flexibility provisions in those agreements, there is still a possibility that unobserved factors affect agreement signing and that the inclusion of flexibility provisions affects agreement failure. This therefore warrants caution in interpreting any empirical relationship between the inclusion of flexibility provisions and agreement failure as causal.

5. Empirical analysis

A duration model is employed to test the above-stated empirical implication about the implementation of cease-fire agreements. Specifically, I use a Cox model because I do not have strong theoretical expectations to assume a specific functional form for the baseline hazard. However, using the Cox model introduces at least two concerns. First, there is the issue of tied survival times.[6] The estimation procedure for the Cox model uses ordered survival times, and an examination of the Civil War Cease-Fire Agreement Dataset shows that multiple observations experience the event of interest (a violation) after the same elapsed time. This problem is known to prevent researchers from parsing out the composition of the risk set at failure times (Box-Steffensmeier and Jones, 2004). To handle tied cease-fire agreement violation, the Efron method is used.

Second, before deciding on the final form of the fitted statistical model, I consider violations of the proportional hazards (PH) assumption of the Cox model. The PH assumption implies that whenever a change in a variable occurs, the effect of this change on the hazard rate of event occurrence (here, an agreement violation) is proportional over time. To test whether the effects of the covariates adhere to the PH assumption, I used a residual diagnostic that assesses the correlation of scaled Schoenfeld residuals and some function of the time scale (Grambsch and Therneau, 1994). Prior to the formal test, I also examined the graphical distribution of the covariate-specific residuals plotted against untransformed time and noted the presence of possible outlier survival times. As suggested by Park and

[6]The smallest unit of time on which information is available about the timing of cease-fire agreement violations is the day. According to the news reports used to code violations, eight cases experienced a violation on the same day that the agreement took effect. For these cases, the duration of the agreement would be zero, thus leading them to be dropped from the analysis. To account for these cases, which may be crucial for understanding the dynamics of agreement implementation, I coded an alternative measure of the duration of agreements by adding 1 day to the raw measure, thus retaining the cases that would otherwise experience no duration.

Table 2. Cox models of cease-fire agreement violation

Variables	Model 1	Model 2
Flexibility		0.28*
		(0.15)
Enforcement	−1.51*	−1.21
	(0.82)	(0.83)
Joint Committee	0.25	0.06
	(0.34)	(0.37)
Nonenforcement	−0.22	−0.16
	(0.33)	(0.34)
Armed Nonsignatories	−0.16	−0.24
	(0.35)	(0.35)
Agreement Government	−0.07*	−0.06
	(0.04)	(0.04)
No. of Signatories	0.07	0.07
	(0.05)	(0.05)
War Type	−0.41	−0.12
	(0.46)	(0.48)
Timing: After War	−1.26***	−0.96*
	(0.49)	(0.51)
Number of observations	50	50
LR χ^2	18.89**	22.09***
Log-likelihood	−128.43	−126.83

*$P < 0.1$; **$P < 0.05$; ***$P < 0.01$.

Hendry (2015), in the presence of outliers, using untransformed time or the natural log of time in tests of PH violations may work relatively poorly, while using the rank of survival times or 1 minus the Kaplan–Meier (KM) transformation works relatively well. It was determined that outlier survival times do in fact exist in the dataset, and therefore I used the KM transformation of the time scale to perform the tests for violations of the PH assumption. The tests indicate that no covariates violate the PH assumption, and therefore no corrective measures are required.

Table 2 shows the results of the statistical analyses that use the signing date of agreements as their starting dates, while Table 3 shows the results of the analyses that adopt the EIF date as their starting dates. The first thing to notice is that the effects of flexibility-enhancing provisions (*Flexibility*) remain similar across the specifications in which they are included. Namely, the coefficient estimates are positive and statistically significant, indicating that an increase in the number of flexibility-enhancing provisions leads to an expected greater likelihood of agreement violation. Specifically, the estimates of *Flexibility* are 0.28 ($P < 0.1$) in Table 2 and 0.38 ($P < 0.01$) in Table 3, indicating that the addition of one flexibility-enhancing provision in a cease-fire agreement increases the hazard rate of implementation interruption by 32 and 46%, respectively.

Table 4 presents increases in the estimated hazard rates based on increases in the number of flexibility-enhancing provisions. Overall, the picture painted is that when flexibility-enhancing provisions are specifically written into agreements, they tend to be relatively fragile.

Moving on to the effect of the fear-reducing provisions, the results are somewhat weak. The estimates for *Joint Committee* fail to achieve statistical significance across specifications. This means that promises by the parties to establish joint committees or commissions to make decisions together had no impact on preventing warring groups from going back to fighting. The estimated effects of *Enforcement*, on the contrary, are non-zero, but only conditionally. Specifically, the estimates for *Enforcement* are negative, indicating the existence of a third-party enforcement provision leads signed agreements to be less likely to be violated. However, the estimates are only statistically significant when flexibility-enhancing provisions are not included (model 1 in Tables 2 and 3).[7] Specifically, a change

[7]Alternative specifications of the relationship using logistic regression are presented in the online Appendix. Using all control variables in a logistic regression, the estimate for *Enforcement* is negative and statistically significant.

Table 3. Cox models of cease-fire agreement violation

Variables	Model 1	Model 2
Flexibility		0.38***
		(0.14)
Enforcement	−1.58*	−1.33
	(0.84)	(0.84)
Joint Committee	0.09	−0.06
	(0.35)	(0.36)
Nonenforcement	−0.31	−0.10
	(0.34)	(0.35)
Armed Nonsignatories	−0.39	−0.45
	(0.37)	(0.37)
Agreement Government	−0.08**	−0.08**
	(0.04)	(0.04)
No. of Signatories	0.08*	0.08*
	(0.05)	(0.05)
War Type	−0.45	−0.21
	(0.44)	(0.45)
Timing: After War	−1.25***	−1.17**
	(0.46)	(0.47)
Number of observations	50	50
LR χ^2	21.51***	28.50***
Log-likelihood	−127.12	−123.62

*$P < 0.1$; **$P < 0.05$; ***$P < 0.01$.

Table 4. Hazard rate increase

Variables	From signing date (%) (Table 2)	From EIF date (%) (Table 3)
One Provision Increase	32	46
Two Provision Increase	74	112
Three Provision Increase	130	209
Four Provision Increase	203	351
Five Provision Increase	300	557

from the absence to the existence of provision of third-party enforcement is expected to decrease the hazard rate of implementation violation by 78% ($P < 0.01$) in model 1 of Table 2, and by 79% ($P < 0.1$) in model 1 of Table 3. Overall, the combined effects of *Enforcement* and *Joint Committee* are in line with Walter's (2002) argument that third-party enforcement is more effective than power-sharing institutions in helping actors overcome the commitment problem that acts as a constant impediment to successful bargaining. However, when flexibility-enhancing provisions are considered, both *Enforcement* and *Joint Committee* provisions are no longer effective in overcoming the commitment problem and preventing agreement violation.

Now considering factors related to information flow, the estimated effect of *Nonenforcement* is negative across the board, indicating that the existence of provisions allowing third parties to get involved for various missions other than enforcement decreases the likelihood of agreement violation. However, this estimated effect is not statistically significant.

When all provisions related to the information and commitment problems are considered (model 2 in Tables 2 and 3), some control variables also exhibit explanatory power. The estimate for *Timing: After War* is consistently negative and statistically significant. Specifically, when cease-fires are signed after civil war has ended, as opposed to being signed during the war, the hazard rate of agreement violation is expected to decrease by 62–69%. The estimate for *Agreement Government* is negative in every specification and statistically significant in all cases except for model 2 in Table 2. This indicates

Table 5. Summary of change in hazard rate

Variables	Change in hazard rate
Flexibility	46% increase
Enforcement	NS
Joint Committee	NS
Nonenforcement	NS
Armed Nonsignatories	NS
Agreement Government	8% decrease
No. of Signatories	8% increase
War Type	NS
Timing: After War	69% decrease

that as a country's Polity score at the time of signing an agreement increases by one, the hazard rate of agreement violation is expected to decrease by 6–8%. The estimate for *No. of Signatories* is positive and statistically significant only in Table 3, indicating that as the number of signatories increases by one, the hazard rate of agreement violation is expected to increase by about 8%. However, *Armed Nonsignatories* and *War Type* fail to achieve statistical significance in any model specification.[8] Table 5 presents a summary of percentage changes in the hazard rate based on model 2 in Table 3.

These results have at least two important implications for civil war studies. First, they suggest that the effects of fear-reducing provisions on agreement violation expected by the literature are not supported by the evidence presented here. Specifically, provisions about establishing joint committees or commissions do not affect agreement violations, with or without considering flexibility-enhancing provisions. Further, provisions to have third parties step in to enforce the terms of an agreement decrease the likelihood of violations when they are considered without flexibility-enhancing provisions. However, the promise of having third-party enforcement does not affect agreement failure when flexibility-enhancing provisions are considered. Looking at the data more closely, among agreements with provisions for establishing joint committees or commissions, 90% were violated, and half of those were violated within 11 days of coming into effect. Further, among agreements with provisions for third-party enforcement, 50% were violated, usually within about a month of coming into effect. It is not clear from the current data whether joint committees or commissions are eventually established or whether third-party enforcers were deployed before violations (i.e., whether the provisions actually mapped onto real action on the ground). Therefore, the results should be interpreted merely as the existence of provisions for reducing fear not being very effective in preventing agreement violation.

Second, previous studies of civil war cease-fire agreements have paid little attention to the starting dates of those agreements. Most studies in this area have adopted the signing date as the starting date of agreements, apparently without realizing that there is substantial variation in the match between the two. However, this study suggests that the results can be influenced substantially by how one defines the starting dates of agreements. Looking at the main independent variable, the results generally indicate that agreements attempting to enhance flexibility to cope with the commitment problem, as opposed to agreements without flexibility-enhancing mechanisms, are expected to increase substantially the likelihood of implementation failure. These results, in other words, strongly support the hypothesized relationship. Interestingly, the effect of flexibility-enhancing provisions is shown to have stronger statistical significance in Table 3. This means that when one uses the signing date as the starting date of an agreement (as most previous civil war studies do), the effects of flexibility-enhancing provisions are weaker than when one considers the EIF date as the starting date.

[8]In addition, I ran models only with the primary independent variables, that is, without the variables used to account for the selection effect. The results seem to be quite robust. The main difference is that enforcement is not statistically significant when we consider only the primary independent variables. Also, the significance levels of the flexibility variable in the models that only include the primary independent variables are generally higher than those in the original Tables 2 and 3.

6. Conclusion

The government of the Syrian Arab Republic and the Opposition signed a cease-fire agreement brokered by Turkey and Russia on 29 December 2016. As Syrian warring groups were actively engaged in war without much progress on negotiation, the agreement was essentially setting the context for negotiations on a political settlement in January of 2017. Having a provision that groups will negotiate in the future increases the chance that they can reevaluate the changing environment after an agreement has been signed, and thus better cope with it. However, this will also increase the chances that groups will want to be in a better position in future negotiations, and may thus violate the current cease-fire agreement to obtain the upper hand.

This paper examined a question of fundamental importance for conflict and peace: How might actors engaged in civil war construct cease-fire agreements that will lead to lasting peace? To address this question, scholars have recently begun analyzing the features of agreements that appear to be associated with conflict and peace, and using the lessons learned to make prescriptions for agreement design. Based on a rationalist approach, previous studies suggest that provisions to reduce the commitment and information problems lead to a higher likelihood of successful implementation of agreements. While these works have advanced our understanding of the relationship between agreement design and desirable outcomes, they have been limited in the extent to which they explore various mechanisms that affect the commitment problem. Specifically, civil war scholars have mainly focused on how groups should write agreements to reduce fear among the relevant parties, while international law and international political economy scholars have focused on the issue of whether agreements should be rigid or flexible to cope with the commitment problem, which arises due to new, unforeseen, challenges. The latter group of scholars in particular has shown that increasing flexibility to deal with the commitment problem might affect agreement implementation. The current paper represents the first attempt to examine both types of claims systematically within the same framework, for the first time exploring the applicability of flexibility-enhancing mechanisms for civil war studies.

The current analysis indicates that, in general, the prescriptions with respect to flexibility-enhancing provisions offer clear guidance, while those with respect to provisions intended to deal with fear and increase information flows are, at best, somewhat mixed. Specifically, and unlike the suggestions of many past studies from international law and international political economy, it seems that flexibility-enhancing provisions are generally detrimental to the implementation of civil war cease-fire agreements. This may be due to the fact that the main assumptions used by international law and international political economy scholars are not necessarily applicable to the civil war context. Specifically, researchers in these areas assume that signatories will engage in a long-term repeated interaction and argue that the existence of flexibility-enhancing provisions will allow for short-term deviations to tackle immediate challenges, thus leading to long-term benefits. In the specific context of civil war, however, one-time cheating could lead to the end of the current interaction, and groups cannot wait patiently to see a long-term benefit because the immediate stakes are too high. For civil war cease-fire agreements, therefore, the evidence presented here suggests that rigid agreements are more desirable than flexible agreements.

This paper has also touched upon the issue of the starting date of agreements. I contend that scholars often employ an operational starting date that does not match the substantive situation. This would not be a serious problem if the choice of operational definition were not consequential, but the empirical analyses presented here suggest that this choice carries considerable weight when examining the impact of agreement characteristics on violations. Specifically, previous studies have tended to adopt the signing date as the starting date of an agreement, but it turns out that many agreements specify a starting date for the terms that is different from the date of signing. And according to the dataset used for analysis, among the cease-fire agreements whose terms were eventually violated, 31% experience actions inconsistent with the terms of the agreement between the signing date and the EIF date. This potentially means that this time period is particularly susceptible to groups using military strategy to advance their positions before an agreement comes into effect. The implication

for past studies that simply use the signing date as the date that an agreement comes into force, even when many specify an exact EIF date that is different, is that a biased picture will likely emerge in which agreements are observed failing in greater quantities and at faster rates than the empirical reality when EIF dates are considered the conceptual starting points. At a minimum, scholars studying agreement design should be more cautious in defining the starting date and be aware of how consequential this choice is for their results.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/S1468109923000154 and https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/TBUUDK.

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