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# Shifting targets: the effect of peacekeeping on postwar violence

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## Abstract
Existing research shows that peace after civil wars is more stable with peacekeepers present. Yet, violence persists in many postwar contexts, and although postwar violence is often strategic and closely linked to the faultlines of the preceding war, we know little about the impact of peacekeepers on such violence. What we know, moreover, focuses on the former combatants, while this study shows that the majority of deaths in postwar violence are inflicted by other armed actors. This is a challenge for peacekeepers who – for mandate or capacity reasons – usually focus on the warring parties. I argue that the impact of peacekeepers on postwar violence hinges on the extent to which they fill a public security gap after war, since responsibility for violence not covered by a mission's mandate lies with the often dysfunctional security agencies of the state. To test this I use a novel spatial approach to generate data that captures the manifold manifestations of violence across different postwar contexts. I find that only UN police – with their broader effect on public security – mitigate postwar violence generally. UN troops have some impact on civilian targeting by former combatants but no such effect could be identified for violence by other armed actors. The findings highlight the importance of peacekeeping police at a time when the modus operandi and capacity of UN police have been questioned, but also the importance of accounting for a multitude of violent actors when analysing the impact of international interventions more generally.

## Keywords
Peacekeeping, postwar violence, conflict zones, armed actors, political violence, violence attribution

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Peace is often tenuous after civil wars. There is a risk that peace agreements break down and societies relapse into war. On a positive note, research shows that peacekeepers substantially reduce this risk (Fortna, 2008; Gilligan and Sergenti, 2008; Hultman et al., 2016). Even if peace prevails, however, the violence of war often transforms into other forms of collective violence in the postwar period. In Colombia, rival groups are battling to take over territory and the illicit businesses that the FARC vacated (International Crisis Group, 2017). After the Kosovo war, elements of the former Kosovo Liberation Army (KLA) orchestrated deadly violence to expel Serbs from Albanian-majority areas (Boyle, 2010). And in the Democratic Republic of the Congo (DRC), thousands perished after 2001 when militia groups fought over local power and natural resources while the main combatants – some of whom were allied to these militias – were under the auspices of a ceasefire (Autesserre, 2009).

Although these forms of organized violence after war are often strategic and closely linked to the faultlines and purposes of the preceding war – occasionally resembling a continuation of war by other means – we know little about the ability of peacekeepers to mitigate or stop them. This study asks whether and what type of peacekeeping influences the extent of postwar violence.

Two studies have addressed this question for one form of postwar violence: the intentional targeting of civilians (Kathman and Wood, 2016; Phayal and Prins, 2019), but like most studies of peacekeeping effectiveness during war, their focus is on violence by the former warring parties. In postwar periods the nature of violence, however, often changes (e.g. Darby, 2001; Boyle, 2014; Sisk, 2009; Höglund, 2008). There are shifts in both the forms of violence used, and the actors involved in wielding it. The new data I present in this study bolster this claim: The majority of deaths from postwar violence are not inflicted by the warring parties, but by new or seemingly new actors – actors not 'officially' involved in the terminated war. This is a challenge for peacekeepers who – for mandate or capacity reasons – usually focus on the warring parties, while the responsibility for violence not covered by a mission's mandate lies with the security agencies of the state, most notably the police. In many postwar contexts, these agencies are partial, weak, or dissolved altogether. To mitigate postwar violence, peacekeepers thus need to fill a public security gap (Call and Stanley, 2001). This highlights the role of UN police who are deployed to supplement, support, train, reform, and even recreate the local security forces. I, therefore, expect that UN missions with more police may mitigate postwar violence, whereas I do not expect such an effect for armed peacekeeping troops.

To test this argument I present a new global dataset of postwar violence, the dearth of which has hampered the comparative study of postwar violence that captures its manifold manifestations across different postwar contexts (see also Suhrke, 2013). The data cover organized collective violence such as civilian targeting by governments, rebels, and other armed groups; fighting between different armed groups; as well as violence between ethnic, political, and other identity-based groups. The key challenge when studying such a broad range of violent acts and actors is how to attribute – or link – this violence to particular wars (or postwar periods), particularly in countries with multiple conflicts. In this study I discuss this challenge in more detail and present a spatial solution that links violent acts to conflicts by their location. To that end, I have created conflict zone shape files using data on battle locations from the UCDP Georeferenced Event Dataset (GED) (Croicu and Sundberg, 2017; Sundberg and Melander, 2013).

Using these data in a global sample of postwar episodes between 1991 and 2016, I find that UN missions with more police substantially lower the levels of postwar violence. Armed troops do not have this effect. While UN troops do constrain the warring parties from targeting civilians in the postwar period – thus replicating previous findings (Kathman and Wood, 2016) – no such effect could be identified for violence by other armed actors. These findings demonstrate the importance of police in UN peacekeeping operations. As Call and Barnett (1999: 50) write, peacekeeping faces two security challenges: The first is to ensure the security of the combatants from each other. The second is to ensure the security of the rest of the population. While much research on peacekeeping effectiveness has focused on the former, this study contributes to recent research showing that police are particularly effective in addressing this latter challenge (Di Salvatore, 2019; Johansson and Hultman, 2019).

The contribution of this study, however, goes beyond its findings on peacekeeping effectiveness. The empirical approach, namely associating different acts of violence to each other via their spatial proximity (see also Findley and Young, 2012), demonstrates a fruitful way for scholars to integrate research on various forms of violence and study the relationships and shifts between them both during and after conflict. While this study focuses on the postwar period – when the emergence of new actors and the splintering or transformation of old ones may be most pronounced – the participation in violence of actors outside the traditional 'rebels vs state' framework is not just a postwar phenomenon. The article thus speaks to those who have called for a more integrated study of political violence (e.g. Tarrow, 2007; Staniland, 2017).

## Postwar violence: continuity and change

I define postwar violence as collective violence after fighting between the main warring parties has largely stopped.[1] It is a key tenet of the literature on violence during peace processes (e.g. Darby, 2001; Sisk, 2009; Boyle, 2014) that postwar violence is not simply justifiable 'noise' as societies emerge from the chaos of war. Instead, it often serves a strategic purpose: Some actors use it to derail the peace, others to transform the balance of power and resources within the new postwar environment (Boyle, 2014: 29). Civil wars rarely end because the combatants have resolved their incompatibilities. They end because one side wins, or because external pressures force the parties at the bargaining table, where they compromise on a peace that for neither side aligns with the most desirable outcome of the war (Werner and Yuen, 2005). Moreover, many individuals and groups fear losing the riches, power or status they gained during the war. From this perspective, many acts of postwar violence are strategies to pursue the multiple functions of violence into the postwar period, and to reach 'objectives that the civil war left unfinished' (Knudsen and Nasser, 2012: 127).

The Kosovo case mentioned in the introduction is an example: When a NATO intervention ended the war, Kosovo's final status was to be negotiated with Serbia at a later stage. Knowing that the presence of many Serbs in Albanian-majority areas would strengthen Belgrade's bargaining position against the independence of Kosovo, former KLA units employed large-scale violence to expel Serbs from those areas (Boyle, 2010). In Lebanon, the assassination of religious and political leaders after the end of the war in

1990 continued the struggle for power between various militias. Turned into political parties after a power-sharing agreement, they continued to compete for power – through the ballot box, guns and bombs (Knudsen and Nasser, 2012). And in post-Dayton Bosnia, the ethnic groups involved in the war continued to pursue their wartime objective of creating ethnically homogenous territories through ethnically motivated killings and attacks on returning refugees (Berdal et al., 2012).

These continuities in the *purposes* of violence should not obscure the fact that the *manifestations* of this violence are at times very different from the battlefield engagements of the war. There are shifts in the forms of violence, and the actors involved. Scholars of violence during peace processes (e.g. Darby, 2001; Sisk, 2009; Höglund, 2008) emphasize that such shifts are an inherent characteristic of societies in the transition from war to peace. Violence on the battlefield and beyond often escalates with the start of a peace process as all parties try to make territorial or political gains ahead of an agreement that will consolidate the status quo. Over the course of the process, however, battlefield engagements are increasingly delegitimized (Sisk, 2009: 77). This opportunity structure changes the strategic calculus of actors with a continued incentive for violence away from battle towards violent tactics that are less easily detected, associated with a breach of a ceasefire agreement, and/or sanctioned by actors with a stake in the peace.

## Shift in the forms of violence

The former warring parties (rebels and government) may still perpetrate violence in the postwar period. But rather than fighting each other, they target each other's civilian supporters. Alternatively, rebel groups turn against each other or engage in fighting with other armed groups. While the intentional targeting of civilians and fighting between armed groups may follow somewhat different logics, the two often go hand in hand. Wood and Kathman (2015) show that violence against civilians increases during periods in which non-state armed groups fight one another. Violence between rebel groups is seen as a competition over the distribution of resources and political leverage that can be gained from the war (Fjelde and Nilsson, 2012; Nygård and Weintraub, 2015). In the postwar context, former allies may fight each other to challenge the distribution of the 'spoils of peace', such as political positions or economic profits that may be available through postconflict patronage networks (Berdal, 2012: 320). Attacking civilians associated with the opposition is an alternative tactic to establish or maintain control over territory, intimidate one another's supporters, secure electoral advantage or influence the outcome of the peace process (Berdal, 2012: 320; Kathman and Wood, 2016). Höglund (2005) traces this shift during the peace process in Sri Lanka from 2002 to 2005, where a ceasefire was largely respected at the same time as the LTTE and government found new ways to pursue their war aims. The LTTE in particular was fighting a break-away faction and assassinated members of other Tamil opposition groups to strengthen its position in negotiations with the government – all while trying not to be associated with ceasefire violations (2005: 166–168).

## Shift in the actors of violence

Postwar periods also often see a rise of new actors – actors not formally part of the conflict between the rebels and the state. This includes new rebel groups or factions, foreign

armed groups, pro-government militia, political parties, or ethnic groups and other groups organized along identity lines. Regarding the latter, for instance, civil wars often arise out of communal conflicts, and when they end, violence shifts back to the community level as the combatants are bound by their ceasefire agreements (Brosché and Elfversson, 2012). While some of these 'new actors' may not be new at all, that is, they may have already been active during the war, their role can change in the postwar period. They are frequently not covered by ceasefire or peace agreements, which can be exploited by the warring parties by delegating, instigating, coordinating or supporting violence by other groups (Boyle, 2014: 38). This 'logic of delegation' is well known in the literature on pro-government militias, which argues that governments delegate violence to non-state groups to avoid accountability (Carey and Mitchell, 2017), but rebels likewise exploit violence by other groups to mask the strategic nature of their own campaigns (Berdal, 2012: 321). Much violence in the DRC after 2002 followed this logic. As part of a peace deal, Uganda – who had supported the rebels during the war –withdrew from Congolese territory and officially remained out of it (Autesserre, 2009). However, to continue pursuing its wartime profits from gold and other resources in the areas it had occupied (Human Rights Watch, 2005), it supported ethnic militias in the Northeast who perpetrated massive violence against civilians. While the conflict between these ethnic militias has its own roots that precede the Congo wars, it escalated drastically when former warring parties exploited it for their own political and economic aims.

Of course not all postwar violence is strategic, and not all is strategic in the sense that it is orchestrated by former conflict actors.[2] This is why this paper focuses on lethal forms of collective violence by organized groups. While the opportunity structure of the postwar environment may push some actors towards more unorganized violence that at least appears spontaneous or is not clearly attributable to an organized armed group, or towards non-lethal violence that may go undetected or unpunished (see Jarman, 2004), the strategic link to the just terminated war is clearest for collective violence by organized armed groups. This link, in turn, matters for our evaluation of peacekeeping effectiveness. Missions are given increasingly comprehensive mandates and there is a tendency to expect more and more of peace operations (Gizelis and Benson, 2019). But the core goal of all postwar missions is to keep a tenuous peace. Even this narrow goal is arguably not achieved if the war continues by other means, namely through the types of violent activities of highly organized armed, political or identity-based groups that I study in this paper.

## Peacekeeping and the changing nature of violence

The previous section has illustrated a certain continuity in the purposes of violence in war and peace, but argued that there are shifts in the predominant forms of violence, and the actors involved. This matters for peacekeeping. Postwar missions are deployed, first and foremost, to keep the peace between the former combatants. Research shows that they do this effectively (Fortna, 2008; Gilligan and Sergenti, 2008; Hultman et al., 2016). What we know little about, however, is how peacekeepers deal with shifting belligerent tactics and the fact that the majority of deaths from postwar violence are not inflicted by the warring parties at all, but by other actors.[3] Drawing on recent research on peacekeeping effectiveness I argue that peacekeeping police may be better suited to address these challenges than their military counterparts.

Military troops are most effective in mitigating the types of violence that are closest to the core mission task of keeping the combatants in check. UN troops – and not police or observers – are the personnel type associated with fewer battlefield deaths when deployed to ongoing wars (Hultman et al., 2014), and with a more stable peace after war (Hultman et al., 2016). Troops have also been shown to reduce targeting of civilians both during war, and after (Hultman et al., 2013; Kathman and Wood, 2016). Protecting civilians from atrocities has become a core obligation of peacekeepers, and more than 98% of military and police personnel currently deployed in UN missions have a protection mandate (United Nations, 2015: 24). However, peacekeeping troops protect civilians more reliably from rebel groups than from government forces (Fjelde et al., 2019). Effective protection through increasing the military costs of this strategy for perpetrators requires troops to be where civilians are under threat, and governments can de facto deny peacekeepers access to areas where it targets civilians (Fjelde et al., 2019). Peacekeepers are, thus, more likely to deploy to and protect civilians in areas that have seen high-profile military clashes between the belligerents (Phayal and Prins, 2019). In areas away from these frontlines, peacekeepers respond primarily to rebel-perpetrated atrocities.

UN troops appear to be generally less effective for violence that is further away – in nature, actors or location – from the frontlines, or violence that is not perpetrated by the warring parties, though the evidence base here is more limited. Two studies on combatant-perpetrated sexual violence during war come to diverging conclusions on whether UN troops reduce this nonlethal form of violence (Kirschner and Miller, 2019) or not (Johansson and Hultman, 2019).[4] Finally, troops may even have a counterproductive effect on violence by actors other than the combatants. Di Salvatore (2019)'s analysis of peacekeeping and criminal violence shows that larger numbers of UN troops are associated with more criminal violence. This, in turn, is an unintended effect of their stabilizing effect on war violence: As peacekeeping troops successfully address violence by political actors, they create a minimum level of operational security for organized crime to flourish.

Drawing on this research, I do not expect peacekeeping troops to reduce the overall levels of postwar violence. While they may mitigate *some* effects of the former belligerents shifting violent tactics (by targeting civilians, for instance), I do not expect military troops to be effective in dealing with a proliferation and diversification of armed actors in the postwar period. For reasons of mandate and capacities, the priority is to contain clashes between the belligerents and protecting civilians 'within capabilities and areas of deployment' (Fjelde et al., 2019: 104). Violent activities by non-signatory groups (groups that have not signed the ceasefire or peace agreement that called for a mission in the first place) are not prioritized, even if these groups may have political motives.[5] Even in missions with expanded mandates that cover violent activities of irregular armed groups, the use of force is usually restricted to actors who 'actively seek to undermine the peace process or pose a threat to the civilian population' (UN DPKO, 2008: 34). It is unlikely that this will be interpreted as applying to violence in which the strategic intent of continuing the war by other means is not clear. In practice, UN troops have been reluctant to engage irregular actors beyond the 'priority target', i.e. the actor(s) that triggered a mission in the first instance (Tull, 2018). This may also be a result of deployment location. Even missions with large numbers of armed troops

cannot cover all areas of a country. Irregular armed groups in particular may operate in areas away from the main combat areas. So even if a mandate permits military troops to use force to protect civilians from such groups, this can only happen to the extent that they are actually deployed to those areas.

This illustrates an important point about dealing with multiple forms of violence in postwar contexts: The responsibility for any violence not covered by a peace operation's mandate, or for which missions do not have sufficient capacity, lies with the public security agencies of the state. In postwar contexts, however, these agencies are often weak, dysfunctional, involved in the violence themselves (Call and Stanley, 2001), or dissolved and recreated from scratch as part of the peace deal or a victory that ousts the prewar security elites. The success of peacekeepers in dealing with complex postwar environments thus hinges on the extent to which they are able to fill the ensuing public security gap. This highlights the potential role of UN police.

While armed troops usually make up the bulk of a peace operation, UN missions deploy increasing numbers of police. While UN police do not seem effective in mitigating the severity of battlefield violence or preventing the recurrence of war (Hultman et al., 2014, 2016), research has associated UN police, just as armed troops, with fewer deaths from combatant violence against civilians during and after war (Hultman et al., 2013; Kathman and Wood, 2016).[6] Hultman et al. (2013) offer an explanation that is in line with the reasoning on strategic shifts in violence outlined earlier: As opportunities on the battlefield reduce (either by a peacekeeping intervention or by the end of war itself), actors are tempted to shift operations to the rear bases, where they victimize opponents. By patrolling such areas behind the frontlines and protecting civilians directly, UN police can increase the costs of these alternative strategies of war as well (2013: 6), and perhaps in areas where military troops are not present. Importantly, however, recent research shows that UN police can be comparatively more effective for the types of violence that military troops have difficulty containing. Both Kirschner and Miller (2019) and Johansson and Hultman (2019) find that UN police reduce conflict-related sexual violence, and Di Salvatore (2019) finds that *only* UN police can reduce criminal violence.

Drawing on this research, I expect missions with more UN police to reduce the overall levels of postwar violence. But why should unarmed or lightly armed police officers be effective in dealing with violence that heavily armed troops are not able to address or even exacerbate? After all, the number of UN police is fairly low in most missions. The answer, as the studies earlier suggest, lies in the fact that UN police provide security *and* build the capacity and legitimacy of the host state's own security forces. That is, they have a direct and an indirect impact on levels of postwar violence.

UN police have a direct impact by providing interim security and law enforcement especially in the early phases of the postwar period and if functioning state institutions are absent (Call and Stanley, 2001: 157–160). While it is rare that UN police have an executive mandate to enforce the law, they still provide direct operational support to host-state police in the protection of civilians, the planning and conducting of operations, investigations of incidents, patrolling or community-oriented policing (UN DPKO/DFS, 2014). Johansson and Hultman (2019) for instance attribute the success of UN police in lowering sexual violence to the close interaction with government in activities such as

joint patrolling. By working together, UN police are able to monitor the behaviour of government in response to challenges to their authority, thus perhaps lowering the risk of abuses. In this way, UN policing also contributes to increasing the legitimacy of host-state police. Blair (2019) shows for Liberia that citizens' exposure to UN police made them more likely to rely on state rather than informal authorities to resolve incidents. He argues that through joint patrols and other activities, UN personnel create opportunities for positive interactions between communities and host-state police and, thus, generate support for state authority. That citizens turn to state authorities to resolve disputes, in turn, is crucial for preventing their further escalation.

Besides this more direct impact, UN police have an indirect and longer-term effect on postwar violence. UN police usually engage in the creation, training or reform of the local security agents (mainly the police) to improve public security in the medium and long run. These tasks fall under a broader set of activities that have come to be known as Security Sector Reform (SSR) and aim to ensure that all parts of a state's security sector perform their duties effectively and with respect to human rights and the rule of law. The UN is only one of many actors that can initiate or lead SSR programs, and SSR activities also take place in conflicts without a peacekeeping mission, or in countries where just a political UN mission is deployed (Scherrer, 2007). But if a peacekeeping mission is deployed, UN police are almost definitely involved in SSR activities.

If new police forces are created, UN police are involved in vetting, recruiting and selecting candidates, and training them in police academies and in the field (Hansen, 2002: 22). When working with existing police forces, UN police advise and mentor, and provide specialized training for instance in riot control, community-based policing or human rights. Through these training and reform activities, UN police have a 'force multiplier effect' (Hultman et al., 2013: 6) as they strengthen the state's ability to deal with postwar violence and especially violence that peacekeepers do not have the mandate or capacity to deal with. This is particularly important in postwar contexts that witness a transformation of violence towards non-warring parties including newly emerging armed groups, or only loosely organized violent actors. This indirect effect via capacity building also explains how police can have an impact on violence – and even violence by heavily armed organized groups – despite their often low numbers (United Nations, 2016: 12).

There are a few additional reasons why UN police may be better situated than their military counterparts to address violence by new or non-signatory armed actors. First, police officers are trained to use primarily non-lethal force, which should make them less reluctant (for fear of political repercussions) to take action even in situations in which either the identity or the strategic aims of an armed group are not clear. Second, police components of UN peace operations are often not strongly guided by mandates, i.e. there is little information on how the police ought to stabilize a postwar country (Rausch, 2002). According to Holt et al. (2009: 127), this is due to political concerns, as 'precise language within mandates could be seen, depending on the wording, as encroaching upon host state sovereignty'. As Hansen (2002: 41) argues, this can – ironically – be a good thing, as police operations benefit from some flexibility, so that they can adjust more quickly to changing realities, such as newly emerging armed actors.

To sum up, while there is no strong case for assuming that armed troops in UN peace operations reduce the overall extent of postwar violence, there is reason to expect that

UN police may – via their direct activities but especially via their impact on the local security forces – mitigate the manifold forms of violence by a multitude of actors in postwar societies.

> **Hypothesis 1**: *Higher numbers of UN troops committed to a postwar situation are not associated with lower levels of organized collective postwar violence.*

> **Hypothesis 2**: *Higher numbers of UN police committed to a postwar situation are associated with lower levels of organized collective postwar violence.*

## Research design

To test these hypotheses, I create a global monthly dataset of peacekeeping and postwar violence between 1991 and 2016. Postwar periods are observed for five years, or less if conflict resumes. The list of conflict episodes is taken from the UCDP/PRIO Armed Conflict Dataset v17.2 (Allansson et al., 2017; Gleditsch et al., 2002). Two rules apply: First, I include only postwar periods of relatively severe conflicts (on average 100 battle deaths in a year or at least 1000 battle deaths over their course). This criterion ensures that there is sufficient information on the location of battles to define the conflict zones. Second, for a conflict to be considered over, it has to have a clear ending (a victory or settlement) *or* a full postwar period of five years in which there was no conflict recurrence. This distinguishes more genuine postwar periods from the many situations in which years with few battle deaths constitute mere breaks in fighting. This yields a sample of 71 postwar periods.

### Postwar violence

The dependent variable, level of postwar violence, is defined as a count of deaths from organized collective violence in what used to be the conflict zone of the civil war.[7] Information on violence is taken from the UCDP GED data (Croicu and Sundberg, 2017; Sundberg and Melander, 2013). Specifically, I use information on non-state violence and one-sided violence against civilians (OSV). This covers a broad spectrum of organized violence common in postwar societies, including OSV by the state, rebels, militias, as well as foreign governments and armed groups; fighting between all these different armed groups, and communal or identity-based violence between ethnic groups or supporters of political parties. While other GEDs could be used to get an even fuller picture of postwar violence (Raleigh et al., 2010; Salehyan et al., 2012), they do not have global coverage. Also, combining datasets from different sources requires identifying events that are covered by several datasets to avoid double-counting events.[8]

But how do we know that a particular violent event *is* postwar violence, i.e. has something to do with a just terminated war? Many forms of collective violence that are common in postwar societies also take place in countries that do not have a civil war, or they take place in war countries but are unrelated to a particular conflict against the government. An act of killing civilians in India's troubled Northeast, for instance, is unlikely related to all the multiple conflicts there. Moreover, not all collective violence in

India – one need just think of Hindu–Muslim riots – are part of an armed conflict in which a rebel group fights the state. A country-level approach that attributes all collective violence in a postwar country to one particular postwar situation is accordingly problematic, especially in large countries or in countries with multiple conflicts.

UCDP does not code events of non-state violence or civilian targeting in relation to state-based conflicts (civil conflicts/civil wars), i.e. there is no information in the data on whether an event is linked to a particular war, and if yes, to which one. With linked I mean that this violence would have been much less likely if there had been no civil war and is thus 'war-related violence'. This data challenge partially explains the focus on combatant-perpetrated violence in previous peacekeeping research. If a rebel group targets civilians, it is logical to assume that this violence is war-related. Moreover, it is easy to attribute it to a particular war, as most rebel groups fight only in one conflict within a country. For all actors that were not formally a warring party (such as militias or communal groups), this does not work. Importantly, it also does *not* work for the government side of civil conflict dyads. The government is a warring party in all civil conflicts coded by UCDP, hence if a government targets civilians, there is no way to know from the data in which conflict context this violence happened, if it was war-related at all. Previous research has circumvented this problem by counting all government OSV in a country for each conflict (e.g. Hultman et al., 2013; Haass and Ansorg, 2018), or by dividing deaths from government OSV equally between conflicts (Kathman and Wood, 2016). Either way, government OSV is overcounted and/or misattributed.

I propose a spatial solution to this attribution problem and attribute acts of postwar violence to the preceding conflict by their location.[9] This approach is similar to Findley and Young (2012) who study the relationship between civil war and terrorism, where terrorism is defined as war-related if it occurred in the civil war zone prior to, during or after a war. This spatial identification of postwar violence permits me to study relevant instances of postwar violence even in countries with several conflicts, such as India or Ethiopia, and to exclude instances of violence that are likely unrelated to any particular conflict because of their spatial (and temporal) distance to the war.[10]

To delimit the conflict zone, I draw a polygon around the area in which war violence (GED violence between rebels and the state) took place while the conflict was active. Conflict zone polygons have previously been created for GED by Croicu and Sundberg (2012), but are not updated, i.e. cover only conflicts from 1989 to 2010 in Africa. While I follow the authors' procedure to an extent,[11] I deviate from their decision to exclude only extreme outliers – such as the IRA killing a British soldier in Germany – by excluding a larger share of events away from the bulk of fighting. Specifically, I exclude a maximum of 10% of events whose average distance to all other events is more than 2 standard deviations larger than the mean distance of all events to each other. The reason is that the outer boundaries of conflict zones often overlap, which means that certain acts of violence would be considered postwar violence of several conflicts at once, or that violence taking place during one active conflict could at the same time be identified as postwar violence of another conflict. Such overlap cannot be completely avoided, but by excluding more distant events it is limited to cases in which the bulk of fighting within different conflicts really did take place in a similar area.[12]

**Figure 1.** Chittagong Hill Tracts rebellion in Bangladesh – conflict zone (dark grey area) and violent postwar events.

The polygons are then used as a filter to select acts of postwar violence deemed linked to a particular conflict episode. Figure 1 illustrates this for the Chittagong Hill Tracts rebellion in Bangladesh (1975–1991). The dark grey area is the conflict zone, while the black dots are events of collective violence during the postwar period (1992–1996). Only events located in the dark grey area are selected as instances of postwar violence for that conflict. All other events taking place at the same time in Bangladesh, and all violence in Northeast India or Burma, are not considered.[13] A qualitative check shows that this procedure distinguishes between war-related and non-war-related violence very well in this case. Only three events that are clearly related to the conflict fall outside the zone at the southern end of the Chittagong Hill Tracts (false negatives), and with a few exceptions (false positives), events within the zone are clearly related to the just terminated conflict. The spatial approach will never fully avoid misattributions, but as Figure 1 shows it is clearly superior to counting all violence in a country as postwar violence of a territorially localized conflict.

As Figure 2 shows, the approach also renders a fuller picture of postwar violence compared to focusing only on violence by the former warring parties.[14] I define warring party violence as violent events in which at least one of the combatants (rebel group or state army) take part. Violence by other actors covers violent events in which *none* of the participants were (former) warring parties. When societies move from war to peace, there is a striking shift in the actors responsible for most war-related violence. During war, violence by actors not formally involved in the armed conflict against the state accounts for only 20% of war-related collective violence. In the postwar period, this
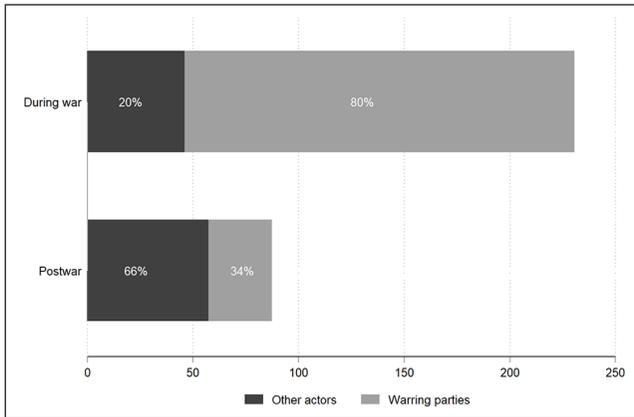
**Figure 2.** Average annual deaths from war-related violence by perpetrator category.

changes to 66%. This massive shift shows even in absolute numbers: Non-warring parties perpetrate more violence in postwar periods even when taking into account that ongoing wars are so much more deadly on average, causing 58 annual deaths on average compared to 46 for ongoing wars.[15] A focus on the former combatant parties as perpetrators of postwar violence thus sheds only a limited light on the phenomenon. Moreover, the extent to which the former warring parties are still responsible for violence in these postwar periods varies widely across postwar contexts. This implies that not only do we miss a lot of postwar violence if we focus on the former combatants only, but we get a biased selection of postwar violence that does not capture the security situation equally well in all countries.

## Independent variable and controls

The independent variable in this study is the monthly number of armed peacekeeping troops and police, respectively, in UN missions. Data are from the International Peace Institute Peacekeeping Database (2019).[16] The original data are on the country level, hence if there are several conflicts in a country, I include missions only for the conflict(s) for which they had a mandate.[17] I also include the number of UN observers as the third common personnel category, though I make no hypothesis about their effectiveness. All peacekeeping variables are measured in thousands and lagged one month to ensure temporal order. Some of the postwar contexts with most troops include Bosnia, the DR Congo, Sierra Leone, Liberia and Croatia. High numbers of police were deployed, for instance, to Kosovo, Bosnia, Haiti, Liberia and East Timor.

I further include a number of control variables that I assume to influence both the severity of postwar violence and the likelihood of peacekeeping deployment. One is the (logged) cumulative count of deaths from all GED types of violence in the conflict zone over the course of the conflict. The other focuses on the severity of the conflict directly before it ended and records the (logged) average deaths from all GED types of

violence in the conflict zone in the two years before termination. While there are notable exceptions of brutal civil wars followed by comparatively little postwar violence (Suhrke, 2013), I expect the levels of violence during the war to be a good predictor of the levels of postwar violence (see also Boyle, 2014). Also, more deadly wars increase the likelihood that a peace operation is deployed (Gilligan and Sergenti, 2008; but see Fortna, 2008).

I also include a binary that records whether the conflict ended in a ceasefire or peace agreement. These are the contexts in which peacekeeping missions are most likely, and they may see most violence because the warring parties retain some ability to continue fighting (Kathman and Wood, 2016). Other actors, moreover, may use violence to derail the implementation of such agreements and/or to force their way into negotiations. Data are from the UCDP Conflict Termination Dataset (Kreutz, 2010), extended to 2015. In addition, a dummy variable records whether multiple rebel groups were involved in the conflict. Fragmentation increases the risk of recurrence (Rudloff and Findley, 2016), and I expect a similar relationship on the risk of postwar violence. The presence of several groups fighting the state over the same incompatibility suggests disagreements within the opposition and will likely increase postwar competition over the distribution of the 'spoils of peace'. Information for this variable is from the UCDP Armed Conflict Dataset v17.1 (Allansson et al., 2017; Gleditsch et al., 2002). Last, I include a cubic polynomial of time (number of months) since conflict termination to account for temporal dynamics in the prevalence of postwar violence (Carter and Signorino, 2010).

## Addressing non-random treatment assignment

Control variables can only partially address the challenge of selection bias in the study of peacekeeping effectiveness. Peacekeeping missions are not deployed at random, but have been shown go to more difficult cases (Gilligan and Sergenti, 2008; Fortna, 2008). If we see more violence in conflicts with more peacekeepers we may thus mistakenly conclude that peacekeeping leads to more violence, when the relationship is in fact the other way around. I thus employ additional modelling strategies that address this threat to inference in one or another way.

First, I estimate the main model with a sample restricted to observations with a peacekeeping presence. This ensures that the results do not stem from unobserved factors that distinguish conflicts with a UN mission from conflicts that do not receive peacekeeping (e.g. Hultman et al., 2014: 749). The effects we estimate in this model are, therefore, purely due to the different strength of troops and police between conflicts and over time. What that model assumes, however, is that the most important difference is between conflicts that receive a mission and those that do not. It ignores that among peacekeeping cases, those conflicts to which many troops have been deployed differ systematically from conflicts with many police. If UN troops were deployed to cases with high levels of violence and UN police to areas where violence is expected to be low or has already subsided, we could mistake this as a violence-exacerbating effect of UN troops and a violence-mitigating effect of UN police. Though a look at the cases suggests that this is unlikely (we usually see high numbers of UN police in large, multidimensional missions with many troops), a more systematic strategy is needed to alleviate this concern.

As a second strategy I, thus, restrict the analysis to two different matched samples. Matching preprocesses the data by pairing peacekeeping observations with suitable control cases and discarding observations that do not compare well on factors of interest. Because the aim is to address the differential likelihood that a mission gets a lot of troops versus a lot of police, I create two matched samples. To assess the effectiveness of UN troops I match cases with a substantial amount of troops (3000) with cases that had no troops. To assess the effectiveness of UN police, I match cases with a substantial amount of police (400) to cases with no police.[18] To create the matches I use the control variables introduced earlier and employ coarsened exact matching (CEM) (Iacus et al., 2012).[19] CEM coarsens each variable into bins, then only matches cases that have all variables in the same bins. For the analysis, the uncoarsened values are used. This double strategy – matching on and controlling for the same confounders – improves inference: If covariate imbalance between cases with and without peacekeepers remains after the first step, the second step eliminates some of the remaining bias (Ho et al., 2007). And imbalance does remain, although matching is a substantial improvement. For the match on UN troops, the multivariate covariate imbalance drops from a staggering 0.97 to 0.34, though we lose many peacekeeping observations.[20] For the match on UN police, imbalance drops from 0.98 to 0.50 at a loss of even more peacekeeping cases. To peacekeeping scholars, these numbers will not come as a surprise. It is for a reason that the international community intervenes in some cases but not others, and finding comparable cases is thus hard. Besides that, however, matching has an additional drawback, namely that it can only eliminate bias from observed confounders, that is, factors that we have included in the matching procedure. This leaves the possibility that the estimated effects of UN troops or police are influenced by differences between conflicts that we have not measured.

As a third strategy, I, therefore, estimate a model with conflict-level fixed effects on the full sample of observations. This eliminates at least the unobserved but time-constant differences between conflicts that could influence deployment levels and postwar violence, and evaluates whether changes in postwar violence over time correlate with changing troop and police numbers. These changing troop and police numbers could of course again be influenced by expectations of violence at different times, and thus partially endogenous. At the same time, there is no model that completely eliminates bias stemming from non-random treatment assignment, and if the results of the analysis hold across all of the models mentioned earlier we can be more confident that the estimated effects are not just a result of selection bias.

## Results

I have argued that only UN operations with greater numbers of police are able to reduce postwar violence, but not UN troops, primarily because of the shift towards violence by actors other than the main warring parties. Table 1 reports the results of a negative binomial regression to test these hypotheses. Model 1 is the main model with control variables. As expected, UN police are associated with a strong reduction in the levels of postwar violence, while the effect for UN troops actually goes in the opposite direction and is significant. As discussed earlier, this could be due to troop and police levels being endogenous to violence (or the expectation thereof). The remaining models in Table 1

**Table 1.** Effect of peacekeeping on postwar violence, 1991–2016.

| | (1)<br>With<br>controls | (2)<br>Peacekeeping<br>operation only | (3a)<br>Matched<br>on troops | (3b)<br>Matched<br>on police | (4)<br>Fixed<br>effects |
|---|---|---|---|---|---|
| UN troops | 0.113* | 0.055 | 0.581*** | 0.152[a] | 0.009 |
| | (0.067) | (0.077) | (0.173) | (0.137) | (0.012) |
| UN police | −1.830*** | −2.181*** | −0.019[a] | −3.444*** | −0.987*** |
| | (0.470) | (0.821) | (1.253) | (0.681) | (0.146) |
| UN observers | 5.208** | 5.316 | −2.129 | −1.535 | 2.427*** |
| | (2.308) | (3.572) | (4.849) | (3.402) | (0.385) |
| Conflict deaths | −0.035 | 0.022 | −0.623*** | −0.052 | −0.035 |
| cumulative (ln) | (0.047) | (0.104) | (0.179) | (0.125) | (0.026) |
| Conflict deaths last | 0.001 | −0.005 | 0.014** | 0.004 | 0.001 |
| two years (ln) | (0.002) | (0.004) | (0.006) | (0.004) | (0.001) |
| Conflict ended in | −0.000 | 0.000* | −0.000* | −0.000 | −0.000 |
| ceasefire or peace | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| agreement | | | | | |
| Factionalism | −0.127 | −0.316 | 0.249 | 0.440 | |
| | (0.221) | (0.365) | (0.864) | (0.556) | |
| Time since | 0.501*** | 1.450** | −0.159 | 3.916*** | |
| termination | (0.181) | (0.619) | (2.738) | (1.127) | |
| Time since | 0.237 | −2.431* | −5.150** | −7.632*** | |
| termination($^2$) | (0.497) | (1.419) | (2.384) | (2.480) | |
| Time since | 0.173 | 1.183* | −4.579 | 0.220 | |
| termination($^3$) | (0.500) | (0.699) | (5.084) | (0.829) | |
| Constant | −0.299 | −0.134 | 5.001 | −14.057*** | −2.224*** |
| | (1.167) | (3.217) | (10.131) | (4.792) | (0.173) |
| Alpha | 3.326*** | 2.872*** | 2.247*** | 1.766*** | |
| | (0.229) | (0.411) | (0.289) | (0.321) | |
| *Number of observations* | 3761 | 1217 | 696 | 528 | 2398 |
| *Number of postwar* | 71 | 28 | 15 | 12 | 47 |
| *periods* | | | | | |

**Note**: Standard errors clustered on conflict (postwar episode) in parentheses. ***$p \leqslant 0.01$; **$p \leqslant 0.05$; *$p \leqslant 0.1$.
[a]These estimates should not be interpreted, as the samples were matched on the respective other personnel type.

address this concern: Model 2 restricts the analysis to cases with peacekeeping, Models 3a and 3b to samples matched on UN troops and police, respectively, and Model 4 uses conflict-level fixed effects. The violence-mitigating effect of UN police is statistically significant across all models, which lends strong support to Hypothesis 2. It is also a strong effect: With as few as 400 police, the predicted level of postwar violence drops to half of what it is when no police are deployed. If 1000 police are deployed, the level of postwar violence reduces by 84%.[21]

Troops are never significantly associated with lower levels of postwar violence, as expected in Hypothesis 1. In fact, the direction of the effect points to violence

exacerbation in all models, but is not significant in the two models that more strongly (Model 2) or exclusively (Model 4) estimate an over-time (rather than between-case) association between troop levels and violence. We should accordingly be wary of interpreting this effect as causal, as no model can completely remedy the problem of selection bias. At the same time, it is not entirely implausible that troops could have this unintended effect: By their proven ability to prevent the former combatants from clashing again (Hultman et al., 2016), they may exacerbate the incentives of the former combatants to use alternative violent strategies in the postwar period. This speaks to an emerging discussion in peacekeeping research (Di Salvatore, 2019) that unintended consequences need not always stem from flawed peacekeeping, as is the case when we think of negative peacekeeping externalities such as sexual exploitation or the spread of communicable diseases. Instead, unintended consequences in the form of shifts in violence may ensue exactly *because* peacekeeping is effective and thus alters the strategic environment in which actors operate.

Table 2 disentangles postwar violence into violence in which the former warring parties are involved, and violence perpetrated by new or other actors. The first outcome, thus, corresponds with the theorized shift in the forms of violence, while the second outcome corresponds with the theorized shift in the actors of violence. Based on previous research I have argued that troops may at least mitigate the extent of former belligerents shifting violent tactics in the postwar period, but struggle with the strengthening of other armed actors during the same time. Police, on the other hand, should be able to deal with violence by both groups of actors. The latter is supported. UN police are associated with a reduction in overall violence by the warring parties (Model 5) and other actors (Model 6), and less violence against civilians by both actor categories (Models 7 and 8).

Troops, on the other hand, have no significant effect on overall violence by the warring parties (this is mostly fighting with other armed groups), and are associated with a significant increase in violence by other actors. They are, however, associated with fewer deaths from combatants targeting civilians (Model 7). This replicates previous findings by Kathman and Wood (2016) for Africa in a global sample of postwar situations and with information updated until 2016. When other actors target civilians, however (Model 8), troops are again associated with an increase in violence. In combination, these results demonstrate that this study's move away from a narrow focus on the warring parties to a broader set of violent actors explains the divergence from previous research that has associated UN troops with violence-mitigating effects across the board (e.g. Hultman et al., 2013, 2014, 2016; Kathman and Wood, 2016; Haass and Ansorg, 2018), and not different methodological choices, for example.[22]

To sum up, the empirical analyses strongly support Hypothesis 2 that larger numbers of UN police mitigate postwar violence. In terms of UN troops, there are some indications that they may be associated with an increase in postwar violence, but this result is not fully robust. The only conclusion the data thus support is that there is no evidence that troops reduce postwar violence, which is exactly in line with Hypothesis 1.[23]

The finding that UN police significantly reduce the overall levels of postwar violence, on the other hand, is robust to a host of alternative sample and model specifications.[24] First, it holds when using a count of violent events rather than the number of deaths from violent events in the dependent variable as an alternative operationalization of the

**Table 2.** Effect of peacekeeping on postwar violence by actor, 1991–2016.

| | (5) Warring party violence (all) | (6) Other actor violence (all) | (7) Warring party OSV | (8) Other actor OSV |
|---|---|---|---|---|
| UN troops | 0.056 | 0.147*** | −0.107* | 0.147*** |
| | (0.143) | (0.054) | (0.060) | (0.057) |
| UN police | −1.654** | −2.901*** | −1.638*** | −4.837** |
| | (0.690) | (0.688) | (0.435) | (1.952) |
| UN observers | 5.541* | 7.483*** | 9.467*** | 10.822*** |
| | (2.833) | (2.899) | (2.696) | (2.861) |
| Conflict deaths cumulative (ln) | −0.574** | 0.138 | −0.639** | 0.137 |
| | (0.255) | (0.259) | (0.265) | (0.241) |
| Conflict deaths last two years (ln) | 0.832*** | 0.370* | 0.803*** | 0.135 |
| | (0.227) | (0.220) | (0.234) | (0.237) |
| Conflict ended in ceasefire or peace agreement | −0.116 | −0.274 | 0.039 | 0.110 |
| | (0.594) | (0.575) | (0.622) | (0.690) |
| Factionalism | 1.069* | −0.720 | 1.111* | −0.835 |
| | (0.557) | (0.801) | (0.629) | (0.743) |
| Time since termination | −0.056 | 0.011 | −0.032 | −0.059 |
| | (0.067) | (0.054) | (0.065) | (0.100) |
| Time since termination($^2$) | −0.000 | 0.002 | −0.001 | 0.004 |
| | (0.002) | (0.003) | (0.002) | (0.005) |
| Time since termination($^3$) | 0.000 | −0.000 | 0.000 | −0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Constant | 1.564 | −3.120* | 1.765 | −3.107* |
| | (1.406) | (1.623) | (1.443) | (1.682) |
| Alpha | 3.779*** | 3.848*** | 3.880*** | 3.927*** |
| | (0.269) | (0.343) | (0.292) | (0.463) |
| *Number of observations* | *3761* | *3761* | *3761* | *3761* |
| *Number of postwar periods* | *71* | *71* | *71* | *71* |

Note: Standard errors clustered on conflict (postwar episode) in parentheses. ***$p \leqslant 0.01$; ** $p \leqslant 0.05$; *$p \leqslant 0.1$.
OSV: one-sided violence against civilians.

intensity of violence. It also holds when using the natural log of police numbers, the idea being that a change of 100 police makes less difference when many are deployed, compared to when this change is from 0 to 100. Importantly, it holds when replacing the number of UN observers with a dummy that records whether observers were present or not to address multicollinearity. The numbers of troops and observers in UN missions tend to be highly correlated, and peacekeeping estimates thus often sensitive to the inclusion or specification of the observer variable. The results are also robust to limiting the postwar period to the first two years after conflict termination, as Kathman and Wood (2016) had done, or just to the first year.[25] The results hold when including the lagged dependent variable, as the best predictor for violence in a month may be violence in the previous month.

For a similar rationale, I have run the models with a cubic polynomial of time since the last violent event (of any type used in these data). This ensures that police are not simply associated with lower violence levels because they are deployed when violence has already subsided a long while back. The results are further robust to the inclusion of additional control variables. These include the size of the conflict zone (assuming more violence in larger areas); regional dummies from the UCDP/PRIO conflict data; whether conflict ended in a victory (Boyle (2014) has associated victories with higher levels of postwar violence); how long the conflict lasted (a measure that in Kathman and Wood (2016) has an ambiguous effect), or a dummy for whether a peace operation was present while the conflict was ongoing. An additional concern given the low number of postwar situations in the sample is that cases with extreme values on the dependent or independent variable could drive the results. A cross-validation test in which the model is run 71 times, each time with a different postwar episode left out, shows that this is not the case.

Finally, while most previous research on peacekeeping effectiveness focuses on UN missions, other organizations are increasingly important peacekeeping actors. The European Union in particular has numerous police missions. While the number of police in these missions is usually low, they are strongly involved in the type of capacity-building that I theorize to be at the heart of the effect of UN police. To ensure that these non-UN deployments are not what drive or bias the results, I take annual information on non-UN deployment numbers from the Stockholm International Peace Research Institute (2019) and turn them into monthly data using linear interpolation. Both when estimating the effect of UN and non-UN troops and police together, and when controlling for non-UN troops and police alongside UN troops and police, the findings remain: Troops do not mitigate postwar violence, while police do, and the latter effect is driven primarily by UN police.[26]

I have argued that UN police have this effect through a direct and an indirect pathway. Given that executive policing mandates and similar independent roles for UN police are the exception, the latter should be more prevalent. However, strengthening and building local and national police capacities take time and will produce change mainly in the medium and long run. Moreover, police deployments tend to reach maximum numbers a bit later in the postwar periods than troops.[27] Figure 3, thus, plots the conditional marginal effect of UN police over time. To facilitate interpretation, I have transformed the number of UN police into a binary variable that takes on the value of 1 if a many police (more than 1000) are deployed.[28] Clearly, the effect of UN police increases over time. The confidence interval is large, suggesting that the effectiveness of police varies strongly between missions, but the average effect illustrated in Figure 3 lends credence to the causal mechanism of police effectiveness outlined in this study.

## Conclusion

This study demonstrates the importance of police in UN peace operations. While armed troops seem to struggle with shifts in the nature of violence after war, deploying even a modest number of police may mitigate organized collective violence during that time. This does not mean that we should send fewer troops to postwar situations. Postwar periods are on average massively less violent than the preceding wars. Keeping the
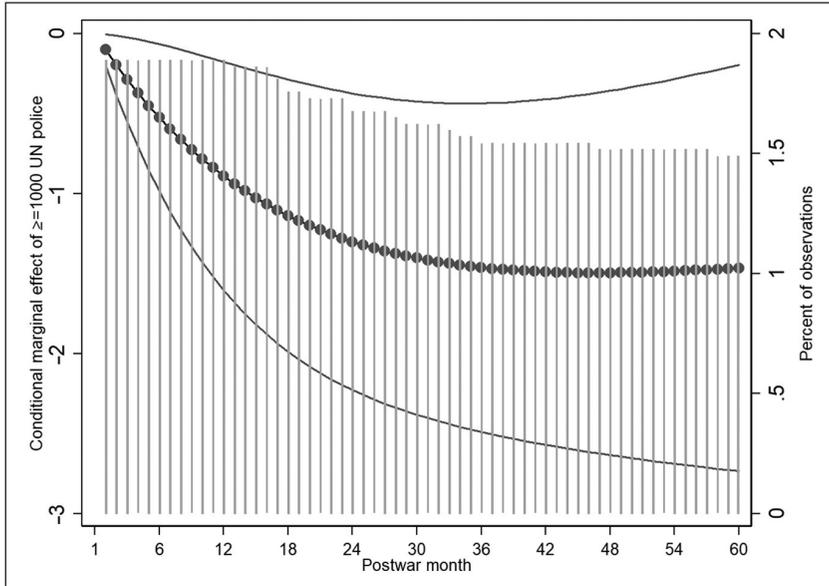
**Figure 3.** Impact of UN police on overall deaths from postwar violence over time (95% confidence interval).

peace, thus, has to remain the first priority of peacekeeping, and this requires armed troops (Hultman et al., 2016). The conclusion that should be drawn from these results is that more police are needed *alongside* troops. But unlike troops who are dispensable during peacetime, police are always needed at home (Call and Barnett, 1999). This deployment gap is a problem that policymakers ought to address, and the results here hopefully contribute towards motivating such action.

The findings on the positive impact of UN police on collective postwar violence – and other recent findings on a violence-mitigating effect of police peacekeepers (Di Salvatore, 2019; Johansson and Hultman, 2019) – come at a time when the current capacity of UN police to contribute to long-term peace has been questioned. Deployed police often lack training in specialized tasks, or come from countries that have themselves no tradition of human-rights-based policing (Greener, 2011). Another problem, identified in an external review of the UN Police Division (United Nations, 2016), is the gap between frequently rotating police personnel and the medium-term structural challenges they are expected to address. The review is skeptical: The current UNPOL operating model does not put UN police in a position to support a host state towards a functioning police service meeting even basic standards (United Nations, 2016: VII).

The findings of this paper do not conflict with this assessment, but they show that UN police are effective if we employ a more modest yardstick of success. That UN police help mitigate the worst forms of organized collective violence in the first five years of the postwar period is a crucial achievement. However, it says little about long-term outcomes or about the impact of police on violence not covered by the analysis here. Further

research may extend this analysis and study peacekeeping impact on less organized and less visible forms of violence in postwar countries. In this context, the role of civilian peacekeeping personnel deserves more attention. Just like police, they are also involved in capacity-building, institutional development and strengthening the rule of law, but research has only recently begun to study their impact.[29]

To prepare the way for such research, this paper has overcome a limitation that has hampered the quantitative study of postwar violence so far, namely a lack of data on the various forms of postwar violence we see in different contexts (Suhrke, 2013: 5). The spatial methodology used in this article can be used with any event data that are georeferenced. Novel approaches to integrate event data from multiple sources (e.g. Donnay et al., 2019) open up exciting possibilities for studying a much broader set of violent acts and violent actors that allow us to analyse how violence transforms in the course of a conflict and its aftermath.

This may also break up the dichotomy between war and peace that makes it necessary to study 'postwar' violence as a particular category of violence at all (Campbell et al., 2017; Davenport et al., 2018; Diehl, 2016). Some experts would hardly identify a number of cases analysed in this study as postwar periods. But this is the point of this paper: Quantitative peacekeeping research always studies these cases as postwar contexts – as successful examples of peace that is kept. Unfortunately, policymakers may do that too: As Autesserre (2009) shows for the DRC after 2003, it was exactly the postwar lens adopted by international peacebuilders that blinded them to the fact that the continuation of violence in the East was more than just local troubles, but a continuation of war by other means. While this study is limited to the postwar period as a time when shifts in the forms and actors of violence are most pronounced, combatants use a broad repertoire of violent tactics during war, and hardly any war is limited to violence by the main combatants. This likely poses challenges for peacekeepers already while wars are ongoing.

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## Notes

1.  *Interpersonal* violence (violent crime, domestic violence) often also persists or increases during this time. For a review, see Gartner and Kennedy (2018); for a quantitative study of how wartime violence influences postwar crime, see Deglow (2016).

2.  See Boyle (2014) for a distinction between strategic, instrumental and expressive postwar violence.
3.  The Research Design section offers descriptive statistics for this claim.
4.  Johansson and Hultman (2019) find that UN troops affect conflict-related sexual violence only when perpetrator groups have strong internal control.
5.  Mission reports can be quite explicit about this. See for instance S/2001/373 and S/2002/169 for MONUC in 2001/2002 (UNSG, 2001; UNSG, 2002).
6.  In Kathman and Wood (2016), this effect was significant only for rebel-perpetrated violence.
7.  These include deaths on the sides of the perpetrators as well as civilians caught in the crossfire or civilians intentionally targeted, depending on the type of violence.
8.  See Donnay et al. (2019) for a procedure to do that.
9.  There is of course the qualitative alternative – attributing events to particular conflicts based on case evidence (Boyle, 2014). This is labor-intensive, hence if such data are to be made public, replicable and continuously updated (the Boyle data end in 2007), it is only feasible within large data collection programmes with the necessary resources.
10. This assumes that postwar violence does not systematically shift to areas that were not battle-grounds during the war. A glance at the data suggests this assumption is by and large warranted, though systematic analyses of this are not available.
11. For instance by only considering events that are coded with decent location precision (at least level 3) and by the choice of a convex hull that is uniquely defined for each case.
12. More information on the creation of the conflict zones and the shapefiles are in the Appendix.
13. I drop events that take place outside the country border. Peacekeepers are usually deployed to a country with host-state consent and cannot just act on the sovereign territory of neighbouring states.
14. The charts exclude the year 1994 for Rwanda. The genocide was extreme in that it accounts for the vast majority of non-battle deaths in the entire dataset.
15. The graph shows only war-related violence (OSV, fighting between armed groups and identity-based groups), and excludes battle between the main combatants (war violence). If battle deaths are included, ongoing wars are of course even deadlier than postwar periods.
16. I inter-/extrapolated for a few missing observations and split the numbers for UNPROFOR between Croatia and Bosnia using information from yearbooks published by the Stockholm International Peace Research Institute (2019).
17. Subnational data on peacekeeping deployment (Fjelde et al., 2019; Ruggeri et al., 2016) could be used in connection with the conflict zone polygons to count only peacekeeping personnel that are actually in the conflict zone. However, the available datasets do not distinguish between troops and police. Moreover, for the indirect effect of UN police (capacity-building) I do not necessarily expect that they need to be deployed in the zone of fighting.
18. Three thousand troops and 400 police is roughly the median number if there is a UN presence at all.
19. In the Appendix I discuss concerns of post-treatment bias that stem from the fact that peace operations often deploy before conflict termination, and could have already influenced the violence levels we match on. I show that the results are robust to matching on the level of violence before peacekeeping deployment, rather than before conflict termination.
20. More detailed information on the characteristics of these 'lost' observations as well as the matching procedure more generally is in the Appendix.
21. In the Appendix I plot the predicted counts of violence for different sizes of police deployments.
22. All results for UN police hold when using conflict-level fixed effects, which I consider the toughest of the three different tests to address non-random treatment assignment. For UN troops, however, no effects are statistically significant in the fixed-effects models.

23. At least they do not have this effect independently. In a test presented in the Appendix I show that larger troop deployments can strengthen the violence-mitigating impact of UN police.
24. All tests run on Model 1 in Table 1. Code to replicate all tests is in the supplementary materials.
25. As descriptive statistics in the Appendix show, troops tend to be deployed in larger numbers in the first two years of the postwar period and then decrease, while police deployments tend to reach their maximum numbers slightly later. That this is not what influences the results is demonstrated in this and additional robustness tests for different segments of the postwar period (see Appendix).
26. More information on the non-UN peacekeeping data and the analysis is in the Appendix.
27. Figures for this claim are in the Appendix.
28. However, we see the same pattern when using the continuous UN police variable and instead of an interaction run the model separately for each of the five years of the postwar period, or when splitting the postwar period into an early period (first two years) and a later period (last three years). The results of these tests plus the coefficient table for Figure 3 are in the Appendix.
29. See Kirschner and Miller (2019), and a new project at Uppsala University (Otto, 2019).

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