Shifting Targets: The Effect of Peacekeeping on Postwar Violence

APPENDIX

Last updated: 9 December 2019

Author: Corinne Bara (corinne.bara@pcr.uu.se),

Department of Peace and Conflict Research, Uppsala University

Contents

Conflict Zones

The general rules applied to create the conflict zone shapefiles are given in the article. This section offers more detailed information on special cases and individual decisions taken in the process. Note that the conflict zones were created in QGIS, an open source Geographic Information System software.

As indicated in the paper, I excluded a maximum of 10% of events that were far away from the main cluster(s) of battle activity. For conflicts with fewer than 10 events, no events were therefore excluded.

For episodes with only a single event or multiple events all at the same location, a rectangle of 0.5 decimal degrees wide and long is created around the event point, with the point in the center. This renders a conflict zone of roughly 55x55km.

For episodes with two single but distinct event locations, or for episodes in which all event locations form almost a line (which makes the creation of a polygon difficult), the procedure is similar. Rectangles as described above are created around each point, and then a convex hull is drawn around these rectangles to form a single conflict zone. In a few cases (Table A1), I have split a conflict zone in two distinct zones if the distance between two clusters of events was very large.

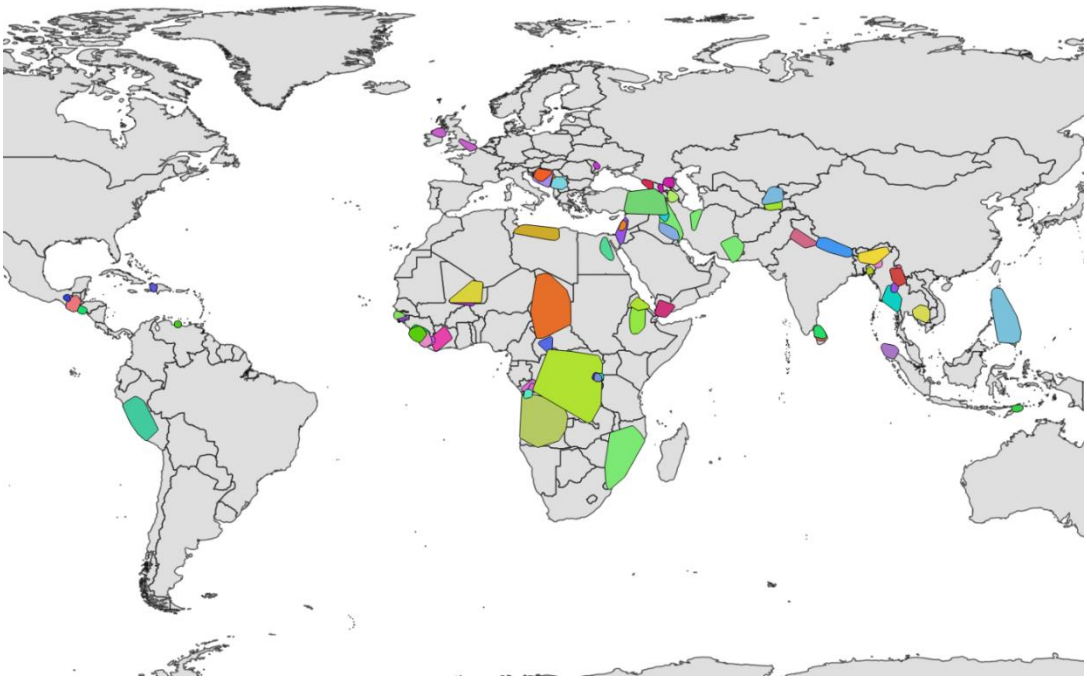
A buffer of 0.5 decimal degrees (ca. 55km) was added to the conflict zone polygons at the end. While this may appear like defeating the purpose of excluding outliers at the fringes of the conflict zones, this is not the case: The purpose of the exclusion procedure is to exclude points far away from the main cluster of events. Adding a small buffer should usually not include them back in, and if it does, this is no problem (as the event was then, after all, not so far away from the main cluster). The purpose of the buffer is to make sure that in areas with high event densities that are likewise close to the outer border of the conflict zone, we take into account that postwar violence may not stop right at that outside border but be distributed a bit further away, too.

Table A1 lists all the episodes in which I had to slightly deviate, for some reason, from the general procedures outlined in the paper and in the comments above. Figure A1 offers a world map overlaid with all conflict zone polygons.

Table A1. Deviations from general conflict zones procedure.

UCDP conflict ID	Episode number	Location	Comment
315	21501	UK (Northern Ireland)	Split conflict zone (one in Northern Ireland, one on mainland UK)
338	33801	Iran	Split conflict zone in three
352	35201	Sri Lanka	Extreme case. One single point is so far away (Germany) that no other points can be excluded, because this point drives the outlier calculation procedure. I thus first excluded the extreme event manually, and then ran the regular procedure.
382	38201	Sierra Leone	Extreme outlier. Same procedure as Sri Lanka
391	39101	Egypt	Extreme outlier. Same procedure as Sri Lanka
13640	1364001	Chad	Points almost in a line: Rectangle procedure
13645	1364501	Yemen	Points almost in a line: Rectangle procedure

Figure A1. Conflict zone polygons



Matching

For the matching in Models 3a and 3b in the paper, I first create a new dataset in which post-treatment observations are dropped. Post-treatment observations are the months of a postwar period in which a peace operation that had been there previously (during the postwar period) has already left. I do not use these as potential control cases. I do, however, use months without peacekeeping *before* a mission arrives (pre-treatment observations) as possible control cases.

I then match peacekeeping months to non-peacekeeping months using coarsened exact matching (CEM) (Iacus et al., 2012). The covariates used for matching are the control variables of the main model: The (logged) cumulative count of deaths from all GED types of violence in the conflict zone over the course of the conflict, the (logged) average deaths from all GED types of violence in the conflict zone in the two years before termination, a binary of whether the conflict ended in a ceasefire or peace agreement, and a binary of whether multiple rebel groups were involved in the conflict. To coarsen these variables, I use the default binning algorithm implemented in the *cem* command in Stata (Blackwell et al., 2010). The code for the entire matching procedure is provided in the supplementary materials.

Table A2 offers balance statistics for the matching on the UN troops binary (an observation is a treatment observation if it has at least 3000 troops present, and it is compared to observations that have no troops present). The Multivariate L1 Distance, which measures imbalance with respect to the full joint distribution of the variables (Blackwell et al., 2010) reduces from 0.97 to 0.34. The absolute L1 value is not so valuable per se but serves to compare different matching solutions. Given that L1 values range from 0 (complete balance) to 1 (extreme imbalance), however, a value of 0.97 clearly indicates that before matching, the sample was very imbalanced. Table A2 offers additional balance statistics (univariate L1 distance and difference in mean between treated and untreated observations) for the individual variables. It shows, for instance, that the only imbalance that remains after matching (though it is reduced) is for the two conflict intensity variables.

Table A2. Balance before and after matching on UN troops.

Variable	Status	L1 distance	Difference in Means
Conflict deaths cumulative (ln)	Unmatched	0.39	0.10
	Matched	0.22	-0.16
Conflict deaths last 2 years (ln)	Unmatched	0.46	0.60
	Matched	0.32	-0.04
Outcome: Agreement	Unmatched	0.16	0.16
	Matched	0.00	0.00
Factionalism	Unmatched	0.14	0.14
	Matched	0.00	0.00

Table A3 offers balance statistics for the matching on the UN police binary (an observation is a treatment observation if it has at least 400 police present, and it is compared to observations that have no police present). The Multivariate L1 Distance reduces from 0.98 to 0.50, and univariate imbalance remains primarily with regard to conflict intensity in the 2 years before termination.

Table A3. Balance before and after matching on UN police.

Variable	Status	L1 distance	Difference in Means
Conflict deaths cumulative (ln)	Unmatched	0.42	0.26
	Matched	0.04	0.08
Conflict deaths last 2 years (ln)	Unmatched	0.57	0.85
	Matched	0.29	-0.19
Outcome: Agreement	Unmatched	0.12	0.12
	Matched	0.00	0.00
Factionalism	Unmatched	0.12	0.12
	Matched	0.00	0.00

In both matching procedures (troops and police) we lose a substantial amount of treatment observations for which no suitable control cases could be found. For the troop match, we lose 264 out of 611 treated observations, for the police match 373 out of 611. As look at the variable distributions of all treated observations versus the treated observations still in the sample after matching shows, as was to be expected, that we lose some of the most extreme cases in terms of how violent the conflict was over its course or in the last two years. On a positive note, however, the means of these intensity variables before and after losing the extremes are not equally effected.

Matching on Pre-Deployment Violence Levels

In the matching procedure described above, I match postwar observations on characteristics we observe at the moment of conflict termination. This includes violence: How many people died over the course of conflict *until it ended*? How many people died on average in the last two years *before it ended*? Many peacekeeping missions, however, deploy before the conflict ends, sometimes years before that. This raises the possibility of post-treatment bias: While deployed, these ongoing missions could have already affected violence levels before the postwar period started, thus distorting our results. This applies in particular to violence in the last two years: If a peacekeeping mission was already active during the ongoing civil war, the last two conflict years may have been less violent than they would have otherwise been. And indeed, for peacekeeping cases, the mean logged count of deaths from violence in the two years before deployment is higher (4.22) than the mean logged count of deaths from violence in the two years before conflict termination (3.97). While matching, we would pair such postwar observations after ongoing peacekeeping with similarly "easy" (low-violence) cases and thus perhaps underestimate the impact of peacekeeping. This could explain why I do not find a violence-mitigating effect of UN troops in this study.

To account for this, I test an alternative matching strategy in which I match peacekeeping cases on the basis of violence levels observed in the two years before deployment (rather than conflict termination), even if that was while the conflict was still ongoing. Control cases without peacekeeping have of course no deployment date. For these cases, the average violence levels in the 2 years before the current month of observation are used as comparison. Now we are comparing postwar peacekeeping cases where a mission had already entered in the middle of an active civil war to really hard control cases, namely cases that are currently (or better: in the last two years) as violent as the active conflict was when the peacekeeping mission entered.

I coarsen the violence variable into decile-bins. Apart from that, the matching strategy is as described in the previous section.[1] For the match on troops, the Multivariate L1 Distance reduces from 0.87 to 0.52, and we lose no treated (peacekeeping) cases, resulting in a sample of 1242 observations. For the match on police, the Multivariate L1 Distance reduces from 0.87 to 0.47 and again we lose no treated cases, resulting in a sample of 1116 observations.

Table A4 shows that the main results (Models 3a and 3b in Table 1 of the article) are robust to this pre-deployment matching strategy. Troops do not have a statistically significant effect on the levels of postwar violence, but unlike in the pre-*termination* matched sample, the violence-exacerbating effect of UN troops is also not significant here (in line with other robustness tests reported in the article). Police have, as in the main results reported in the paper, a strong violence-mitigating effect on postwar violence.

[1] As in the main matching procedure, I also match on whether the conflict ended in an agreement and on whether there were multiple rebel groups, but I leave the cumulative count of deaths out of this matching procedure. This is because we "cut off" or stop counting deaths at the moment of deployment for peacekeeping cases, which may introduce bias because we do not do the same for non-peacekeeping conflicts.

Table A4. Effect of UN peacekeeping on postwar violence, matched on pre-deployment violence.

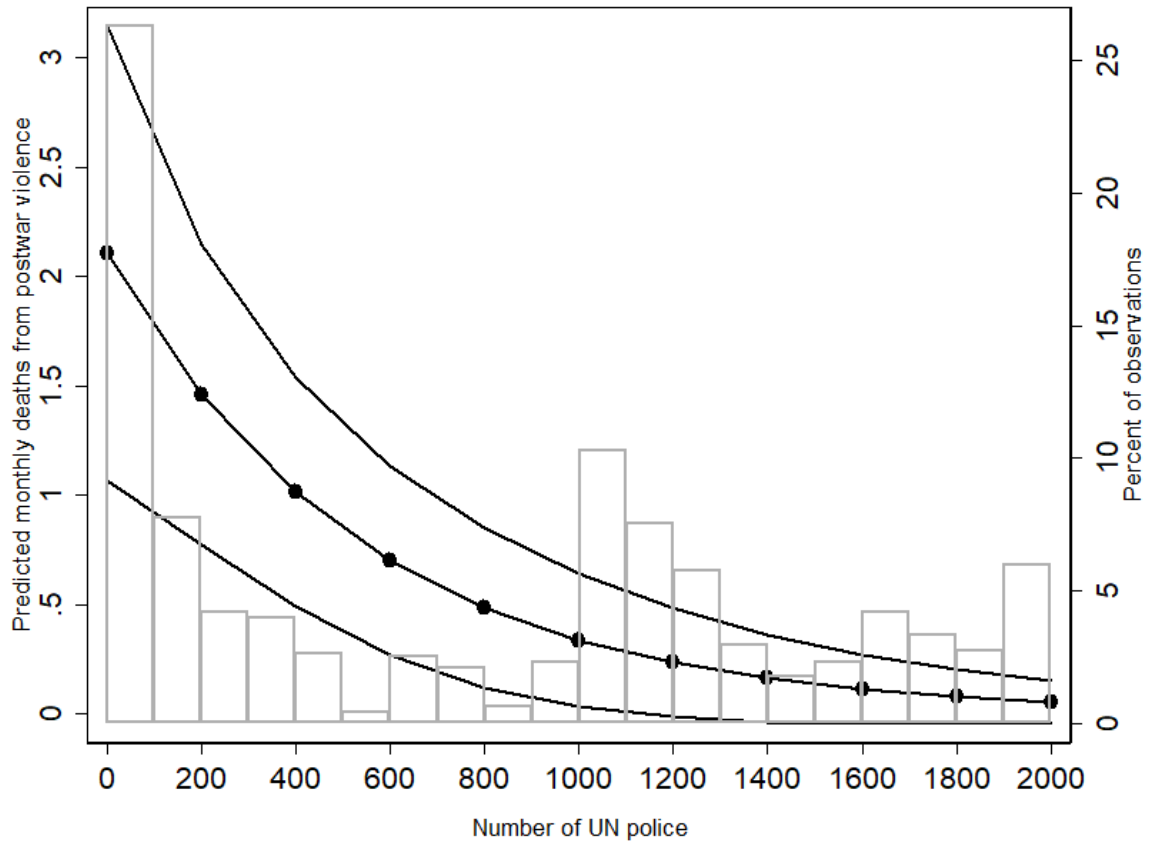
	(1) **All violence,** **matched on troops**	**(2)** **All violence,** **matched on police**
UN troops	0.049	0.241
	(0.051)	(0.237)
UN police	-2.426***	-2.962***
	(0.556)	(0.846)
UN observers	10.752***	6.197
	(2.827)	(4.802)
Pre-deployment 2-year average deaths (ln)	-0.806**	-0.985***
	(0.370)	(0.371)
Conflict ended in CF or PA	-2.082***	-0.279
	(0.765)	(1.036)
Factionalism	1.698***	0.992
	(0.649)	(0.753)
Time since termination	0.093	-0.137
	(0.111)	(0.109)
Time since termination(^{2})	-0.003	0.006
	(0.005)	(0.004)
Time since termination(^{3})	0.000	-0.000
	(0.000)	(0.000)
Constant	3.944**	4.576***
	(1.628)	(1.548)
Alpha	2.974***	2.928***
	(0.326)	(0.329)
Number of observations	1242	1116
Number of postwar periods	*47*	*51*

Note: Standard errors clustered on conflict (postwar episode) in parentheses. *** p<=0.01 ** p<=0.05 *p<=0.1

Substantive Impact of UN Police

Figure A2 shows the substantive strength of the UN police impact on postwar violence by plotting the predicted counts of postwar violence for different sizes of UN police deployments.[2] 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%.

Figure A2. Substantive effect of differing sizes of UN police deployments (95% CI).



[2] The predictions are made using Model 1 (the main model in the paper) for an average postwar context, i.e., the number of troops and all control variables are held at their means. The graph shows predicted counts for a maximum of 2000 police, as fewer than 10% of observations in my data see more numerous police deployments than that.

Interaction Between Troops and Police

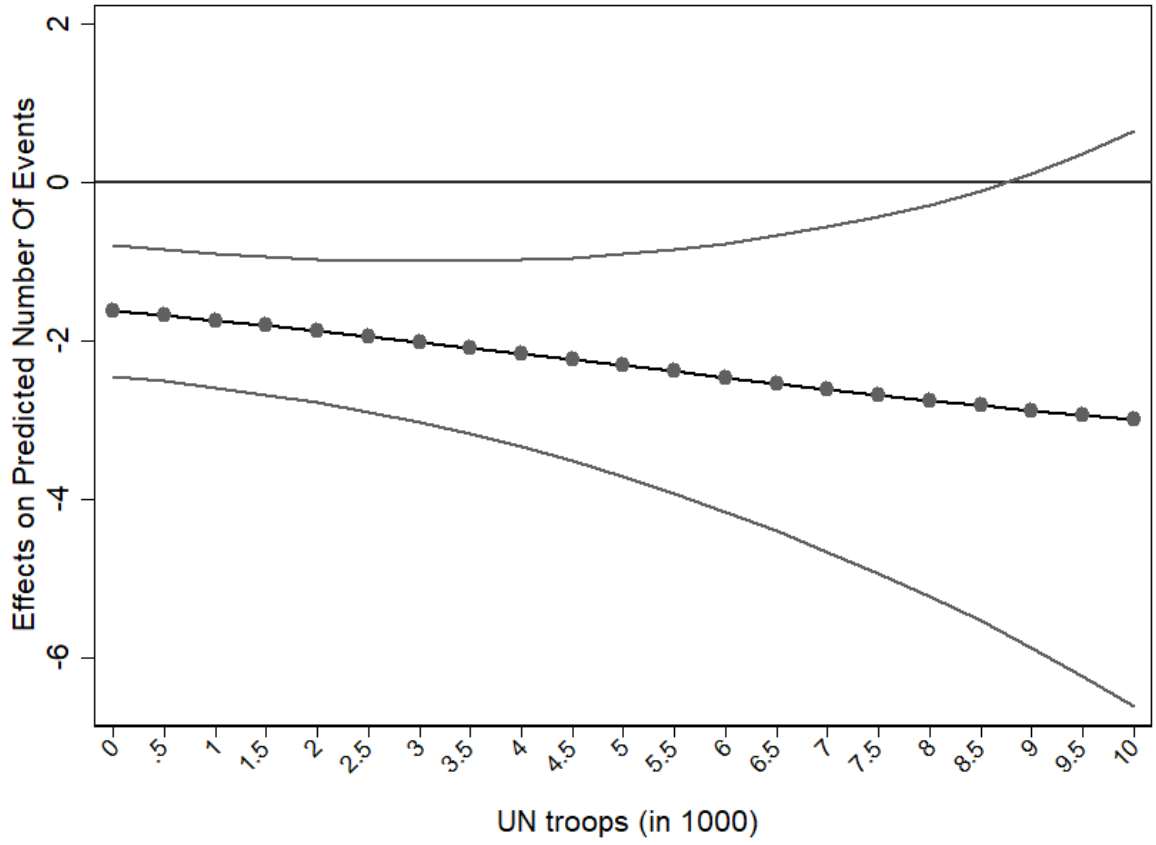
Table A5 presents evidence that troops, although they have no violence-mitigating "net" effect on postwar violence, make the police more effective. For this analysis I have turned the number of UN police into a binary that records whether a substantial number of police were deployed (1000), and tested the impact of that binary at different troop levels (by interacting police with troops, see Table A5). As Figure A3 shows, the violence-mitigating effect of 1000 police increases with more troops, namely by 62% as a median number of troops (7000) are deployed. At troop levels higher than that, the effect soon becomes insignificant for lack of sufficient observations. The results hold when using 400 police as a cut-off for the binary.

Table A5. Estimates for interaction of UN police (binary) with UN troops, 1991-2016.

	(1) >=1000 police	**(2) >=400 police**
UN troops	0.081	0.095
	(0.054)	(0.059)
UN police binary (1000)	-3.765**	
	(1.471)	
UN police binary (400)		-3.266**
		(1.592)
Police binary x Troops	0.214	0.161
	(0.206)	(0.227)
UN observers	4.967**	4.950**
	(2.473)	(2.458)
Conflict deaths cumulative (ln)	-0.084	-0.121
	(0.215)	(0.221)
Conflict deaths last 2 years (ln)	0.466***	0.504***
	(0.177)	(0.180)
Conflict ended in CF or PA	0.272	0.233
	(0.484)	(0.491)
Factionalism	0.297	0.177
	(0.499)	(0.507)
Time since termination	-0.055	-0.016
	(0.048)	(0.048)
Time since termination(^{2})	0.002	0.001
	(0.002)	(0.002)
Time since termination(^{3})	-0.000	-0.000
	(0.000)	(0.000)
Constant	-0.429	-0.445
	(1.146)	(1.178)
Alpha	3.345***	3.346***
	(0.229)	(0.229)
Number of observations	3761	3761
Number of postwar periods	*71*	*71*

Note: Standard errors clustered on conflict (postwar episode) in parentheses. *** p<=0.01 ** p<=0.05 *p<=0.1

Figure A3. Conditional marginal effects of UN police binary (1000 police) with 95% CIs



UN and Non-UN Peacekeeping

Table A6 reports the results for UN troops and police after controlling for the number of non-UN troops and police deployed to postwar contexts. It also reports the results for UN and non-UN troops and police together, respectively. Data on non-UN troops and police are from SIPRI, the Stockholm International Peace Research Institute (2019). SIPRI offers information on peacekeeping personnel deployments in their annual SIPRI Yearbooks from 1993 onwards. From 2000 onwards, this data is available online in the SIPRI Multilateral Peace Operations Database.[3]

[3] https://www.sipri.org/databases/pko. The database has been offline for a while. SIPRI has kindly provided the author with the data files.

SIPRI offers personnel numbers for troops, police, and observers annually. For ongoing missions, this information is recorded at the end of the calendar year. For the year in which a mission ends, SIPRI offers the numbers in the last month of deployment. In order to arrive at approximate monthly numbers, I linearly interpolate between these known data points. More information on this data collection on non-UN peacekeeping is available from the author on request.

A descriptive analysis of the data shows that the mean number of troops deployed in UN missions to the postwar contexts studied in the article is slightly lower (5211) than the mean number deployed in non-UN missions (6858). This number, however, is drive by some very large non-UN deployments (IFOR/SFOR Bosnia). In term of police, the relationship is the other way around: While the UN has deployed on average 771 police with a maximum of 4731 in Kosovo, non-UN deployments see an average of 11 police, with a maximum of 367 in Mali.

When either the UN or another organization has deployed troops in a postwar month in my sample, it was only UN troops in 49% of observations, only non-UN troops in 29%, and both UN and non-UN troops together in 22%. In postwar months during which any organization has deployed police, it was 80% only UN police, 3% only non-UN police, and 17% both.

Looking at the effects in Table A6 we see that the violence-mitigating effect of UN police on overall violence (Model 1), and violence by the warring parties (Model 3) and other actors (Model 5), respectively, remains when controlling for non-UN peacekeeping. The effect for non-UN police goes in the same direction, but is not significant except in Model 3. This is likely due to the low number of non-UN police. When testing UN and non-UN police together, they reduce overall postwar violence (Model 4) and also when split between warring parties (Model 5) and other actors (Model 6), but this is not surprising, as UN police will drive this result.

Table A6. Effect of UN and non-UN peacekeeping on postwar violence, 1991-2016.

	(1) All violence	(2) All violence	(3) Warring parties	(4) Warring parties	(5) Other actors	(6) Other actors
UN troops	0.196**		0.246**		0.281	
	(0.094)		(0.111)		(0.208)	
UN police	-1.851***		-2.670***		-3.025*	
	(0.718)		(0.884)		(1.613)	
Non-UN troops	-0.038		0.059		-0.024	
	(0.063)		(0.083)		(0.103)	
Non-UN police	-4.824		-10.910***		-8.060	
	(4.627)		(3.946)		(15.985)	
All troops		0.065		0.103		0.106
		(0.065)		(0.132)		(0.071)
All police		-1.617***		-1.963**		-2.776***
		(0.500)		(0.854)		(0.779)
UN observers	0.963	1.806*	1.694***	2.232***	0.751	1.999
	(0.655)	(0.998)	(0.556)	(0.818)	(1.162)	(1.294)
Conflict deaths cumulative (ln)	0.070	0.046	-0.443**	-0.413*	0.371*	0.342
	(0.198)	(0.224)	(0.225)	(0.251)	(0.205)	(0.241)
Conflict deaths last 2 years (ln)	0.487***	0.415**	0.670***	0.621**	0.552***	0.531**
	(0.173)	(0.167)	(0.240)	(0.276)	(0.212)	(0.224)
Conflict ended in CF or PA	0.190	0.002	-0.699	-0.759	0.230	-0.012
	(0.488)	(0.516)	(0.560)	(0.630)	(0.644)	(0.662)
Factionalism	0.212	-0.047	1.251**	1.011*	-0.838	-1.298
	(0.502)	(0.534)	(0.518)	(0.517)	(0.832)	(0.892)
Time since termination	0.012	0.020	0.023	0.024	0.071	0.089
	(0.065)	(0.072)	(0.060)	(0.059)	(0.087)	(0.087)
Time since termination(²)	-0.000	-0.001	-0.003	-0.003	-0.001	-0.003
	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
Time since termination(³)	-0.000	0.000	0.000	0.000*	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-1.993*	-1.115	0.376	0.517	-5.476***	-4.593***
	(1.097)	(1.248)	(1.304)	(1.222)	(1.347)	(1.517)
Alpha	3.362***	3.420***	3.758***	3.793***	3.909***	3.993***
	(0.228)	(0.231)	(0.277)	(0.275)	(0.343)	(0.350)
Number of observations	3650	3650	3650	3650	3650	3650
Number of postwar periods	*71*	*71*	*71*	*71*	*71*	*71*

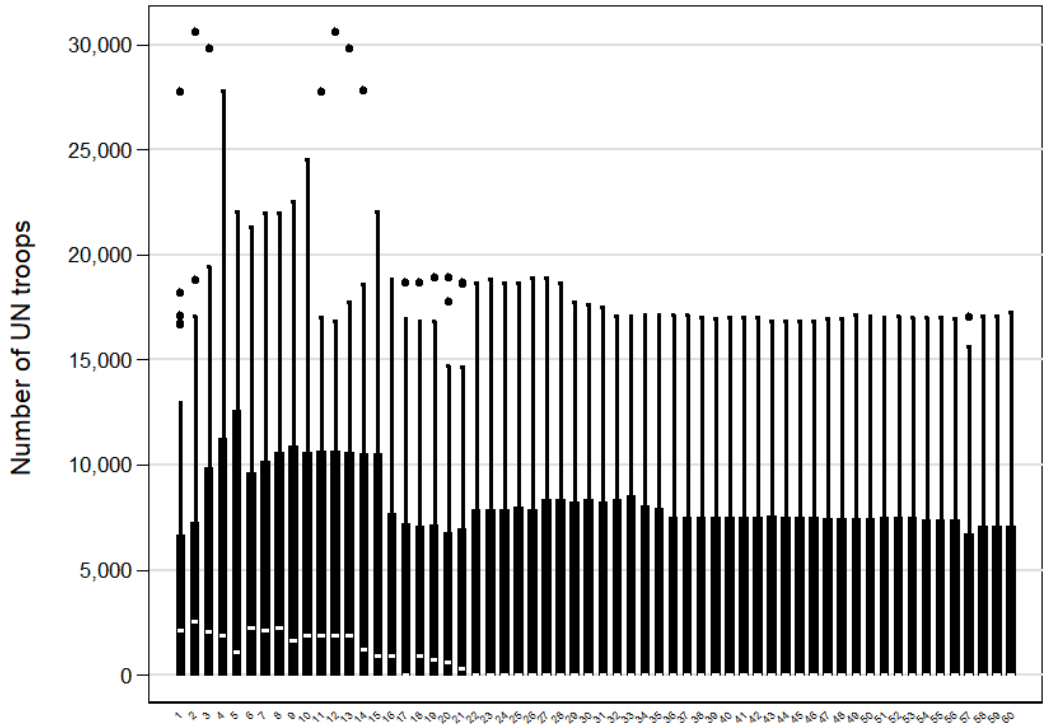
Note: Standard errors clustered on conflict (postwar episode) in parentheses. *** p<=0.01 ** p<=0.05 *p<=0.1

Police Impact Over Time

As was argued in the article, time likely plays a role in the analysis of UN police impact, as indirect violence-mitigating effects from capacity-building take time to unfold. This section analyses these temporal dynamics in more detail.

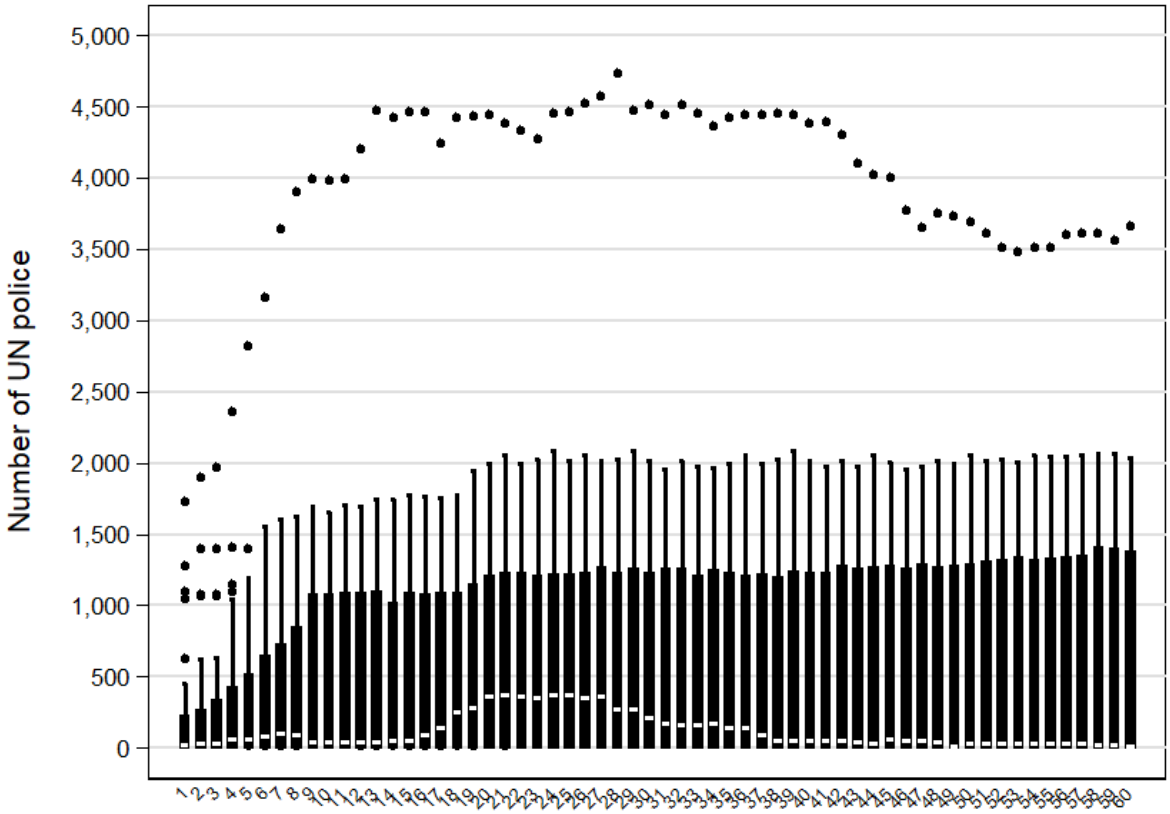
The first two figures (A4 and A5) illustrate the distribution in troop and police deployments over time in all postwar situations that ever, in the course of the postwar period, received peacekeeping. Figure A4 does that for troops. We see that the median number of troops in postwar peacekeeping drops from around 2500 in the first year to zero after two years. Also, the upper bound of the black bars representing the range within which 50% of observations lie drops after the first year, and outliers with very high troop numbers exist only for the first two years. After two years, troop deployments are fairly stable over time. In sum, the number of troops deployed to postwar situations is, as a whole, largest in the first two years of the postwar period.

Figure A4. Box plot of UN troop numbers in all postwar missions over time (postwar month).



For UN police, the relationship is the other way around. The median number of police deployed to postwar situations only really starts to go beyond 100 shortly before two years into the postwar period. In the first couple months, 50% of observations that have or will get a peacekeeping mission have fewer than 500 police deployed, but this number increases quickly over the course of the first postwar year. To sum up, all indicators in these box plots point to a somewhat later deployment of police as compared to troops on average, but also to a longer period during which high police numbers are and remain deployed.

Figure A5. Box plot of UN police numbers in all postwar missions over time (postwar month).



To make sure that these differential deployment patterns do not drive the results reported in the paper, I have run a number of additional tests that split the postwar period into an early postwar period (the first two years when troops are prominent) and a late postwar period (when police have usually reached their highest deployment numbers). Table A7 reports the results of this analysis.

Table A7. Effect of peacekeeping – early versus late postwar period, 1991-2016.

	(1) All actors, first 2 years	**(2) All actors, last 3 years**	**(3) Warring parties, first 2 yr**	**(4) Warring parties, last 3 yr**	**(5) Other actors, first 2 yr**	**(6) Other actors, last 3 yr**
UN troops	0.089*	0.247*	-0.112**	0.172	0.236***	0.193
	(0.053)	(0.141)	(0.044)	(0.161)	(0.054)	(0.193)
UN police	-1.323***	-2.881***	-0.876**	-2.801***	-2.195**	-2.973***

	(0.451)	(0.792)	(0.428)	(0.951)	(0.884)	(0.849)
UN observers	7.189***	0.266	7.720***	3.951	11.800***	0.607
	(2.597)	(2.725)	(1.887)	(3.571)	(2.836)	(4.046)
Conflict deaths cumulative (ln)	-0.011	-0.208	-0.441*	-1.022***	0.583**	0.153
	(0.248)	(0.273)	(0.243)	(0.318)	(0.240)	(0.288)
Conflict deaths last 2 years (ln)	0.377	0.570***	0.862***	0.917***	-0.430	0.593**
	(0.233)	(0.191)	(0.251)	(0.236)	(0.274)	(0.286)
Conflict ended in CF or PA	0.118	0.591	0.426	-0.207	-1.058	0.406
	(0.588)	(0.653)	(0.656)	(0.698)	(0.759)	(1.051)
Factionalism	0.165	0.361	0.918	2.021***	-1.472*	-0.430
	(0.544)	(0.727)	(0.586)	(0.783)	(0.811)	(1.355)
Time since termination	0.049	-0.451	0.196	-0.650	-0.175	-0.101
	(0.197)	(0.940)	(0.186)	(0.771)	(0.315)	(1.261)
Time since termination(^{2})	-0.017	0.009	-0.024	0.012	-0.004	0.001
	(0.021)	(0.023)	(0.022)	(0.019)	(0.026)	(0.032)
Time since termination(^{3})	0.001	-0.000	0.001	-0.000	0.001	-0.000
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Constant	-0.741	6.806	-0.372	13.449	-2.444*	-0.722
	(1.047)	(11.888)	(1.201)	(10.402)	(1.417)	(15.900)
Alpha	3.137***	3.449***	3.450***	4.012***	3.531***	3.894***
	(0.228)	(0.279)	(0.277)	(0.308)	(0.362)	(0.418)
Number of observations	*1655*	*2106*	*1655*	*2106*	*1655*	*2106*
Number of postwar periods	*71*	*63*	*71*	*63*	*71*	*63*

Note: Standard errors clustered on conflict (postwar episode) in parentheses. *** p<=0.01 ** p<=0.05 *p<=0.1

UN troops are associated with higher levels of postwar violence both in the early and late postwar period. When splitting this up into violence by the warring parties and other actors, an interesting pattern emerges: Troop effects are not significant in the late postwar period, but in the first two years, troops are associated with lower levels of violence perpetrated by the combatants, but higher levels of violence perpetrated by other actors. Again, we should be reluctant to interpret this effect as causal, but it would speak to the explanation in the paper that troops may exacerbate the shift towards other armed actors by effectively altering the strategic environment in which the former combatants have to operate. UN police are effective both in the early and late postwar period, but as expected (and plotted in Figure 4 the article), their effect gets stronger over time.

Table A8 offers the corresponding coefficient estimates for the model run to produce Figure 4 in the article. To estimate the effect of UN police over time, the police binary was interacted with time measured in months since the postwar period started. Time in these models was simplified from cubic polynomials to simple linear time.

Table A8. UN police interacted with postwar time: Coefficient estimates.

	(1)
UN troops	0.087
	(0.058)
UN police binary*time	-0.047**
	(0.023)
UN observers	5.196**
	(2.453)
Conflict deaths cumulative (ln)	-0.071
	(0.219)
Conflict deaths last 2 years (ln)	0.495***
	(0.179)
Conflict ended in CF or PA	0.276
	(0.518)
Factionalism	0.031
	(0.521)
Time since termination	-0.006
	(0.008)
Constant	-0.949
	(1.202)
Alpha	3.365***
	(0.228)
Number of observations	*3761*
Number of postwar periods	*71*

Note: Standard errors clustered on conflict (postwar episode) in parentheses. *** p<=0.01 ** p<=0.05 *p<=0.1

Note that of the two constitutive terms of the interaction, UN police and postwar time, only time was included separately in the equation. While the recommendation is to always include constitutive terms except in very specific circumstances (Brambor et al., 2017), the constitutive term for UN police was left out of the equation here for two reasons: First, we cannot (logically) imagine that peacekeeping has an impact on postwar violence that is independent of time, i.e., when time is zero. Second, when including the constitutive term for UN police nevertheless against theoretical reasoning, the resulting graph for the conditional marginal effect of UN police is fundamentally different from what we expect based on the separate annual regressions reported in Table A9. In Table A9, the regression of the main Model 1 reported in the paper is run separately for each of the five years of the postwar period. We see that the effect of UN police becomes stronger over time, which is the pattern we get when estimating and plotting the conditional marginal effect of the UN police binary without the police constitutive term. To sum up, including

the constitutive term for UN police is not only theoretically counterintuitive, but also produces empirical results that are questionable in light of the results we get when we follow the easiest and most straightforward way to analyse the effect of something over time, using separate regressions.

Table A9. Impact of peacekeeping on postwar violence over time

	First year	**Second year**	**Third year**	**Fourth year**	**Fifth year**
UN troops	0.115*	-0.117	0.424	0.049	0.472**
	(0.060)	(0.125)	(0.344)	(0.109)	(0.224)
UN police	-1.024**	-1.426**	-4.048**	-1.737***	-3.425***
	(0.512)	(0.592)	(1.694)	(0.548)	(1.165)
UN observers	9.752***	7.912***	-4.487	10.344**	-4.228
	(3.104)	(3.008)	(5.802)	(4.521)	(4.717)
Conflict deaths cumulative (ln)	0.036	0.010	-0.011	-0.187	-0.098
	(0.299)	(0.343)	(0.465)	(0.276)	(0.295)
Conflict deaths last 2 years (ln)	0.124	0.455*	0.440	0.174	0.517**
	(0.400)	(0.245)	(0.411)	(0.269)	(0.201)
Conflict ended in CF or PA	-0.620	0.500	0.090	0.568	1.212
	(0.861)	(0.904)	(0.890)	(0.674)	(0.769)
Factionalism	-0.134	-0.343	0.277	-0.128	0.746
	(0.638)	(0.776)	(1.160)	(0.764)	(0.866)
Time since termination	-0.068	0.130**	-0.050	0.136**	-0.074
	(0.057)	(0.051)	(0.070)	(0.054)	(0.077)
Constant	0.259	-4.093*	0.815	-6.185**	2.036
	(1.258)	(2.099)	(2.626)	(2.494)	(4.992)
Alpha	3.059***	3.118***	3.310***	3.198***	3.621***
	(0.249)	(0.274)	(0.298)	(0.332)	(0.328)
Number of observations	852	803	731	694	681
Number of postwar periods	*71*	*70*	*63*	*58*	*57*

Note: Standard errors clustered on conflict (postwar episode) in parentheses. *** p<=0.01 ** p<=0.05 *p<=0.1

References (Appendix)

Blackwell M, Iacus SM, King G, et al. (2010) *cem: Coarsened Exact Matching in Stata*. Available at: https://gking.harvard.edu/files/gking/files/cem-stata.pdf.

Brambor T, Clark WR and Golder M (2017) Understanding Interaction Models: Improving Empirical Analyses. *Political Analysis* 14(1): 63-82.

Iacus SM, King G and Porro G (2012) Causal Inference without Balance Checking: Coarsened Exact Matching. *Political Analysis* 20(1): 1-24.

Stockholm International Peace Research Institute (2019) *SIPRI Multilateral Peace Operations Database*. Available at: https://www.sipri.org/databases/pko.