Better peacekeepers, better protection? Troop quality of United Nations peace operations and violence against civilians

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Abstract

Why do similarly sized peacekeeping missions vary in their effectiveness to protect civilians in conflicts? We argue that peace operations with a large share of troops from countries with high-quality militaries are better able to deter violence from state and non-state actors, create buffer zones within conflict areas, can better reach remote locations, and have superior capabilities—including diplomatic pressure by troop contributing countries—to monitor the implementation of peace agreements. These operational advantages enable them to better protect civilians. Combining data from military expenditures of troop contributing countries together with monthly data on the composition of peace operations, we create a proxy indicator for the average troop quality of UN PKOs. Statistical evidence from an extended sample of conflicts in Africa and Asia between 1991 and 2010 supports our argument.

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Introduction

Do peacekeeping operations (PKOs) with well-trained troops and advanced military hardware better protect civilians from violence in armed conflicts than ill-equipped deployments? Or is it only troop size and mission diversity that shape a PKO's ability to reduce violence against civilians (Hultman, Kathman & Shannon, 2013; Bove & Ruggeri, 2016)? The cases of the Central African Republic (CAR) and Mali illustrate these questions well: Despite the presence of over 9,000 troops in the CAR in September 2015, the Multidimensional Integrated Stabilization Mission in the Central African Republic (MINUSCA) failed to contain the killing of 75 civilians in September of the same year. Amnesty International (AI) reports that—in addition to an extremely difficult political situation in the country—major gaps in training and equipment of the peacekeepers from, inter alia, the Democratic Republic of Congo (DRC), Mali, Vietnam, or Yemen, significantly contributed to the mission's failure. AI quotes a MINUSCA force staff observing that '[w]hen there's gunfire, we can only send the guys in armored vehicles. But several of these are currently out of service. […] This reduces our capacity of intervention.' (Amnesty International, 2016: 18).

Contrast this with the peacekeeping mission in Mali, the United Nations Multidimensional Integrated Stabilization Mission in Mali (MINUSMA) that was established in April 2013. Despite a mission strength that was significantly lower at the time that of MINUSCA (about 5,000 troops in August 2013) in a country about twice the size of the CAR, the UN operation successfully stabilized the situation in Mali and monitored the presidential elections in August 2013. MINUSMA was in a much better position to respond to threats against civilians than MINUSCA, in part due to the fact that the mission consisted, inter alia, of highly trained troops from the Netherlands, Denmark, Norway and Finland, and included sufficient transport

vehicles.[1] This logistical advantage—together with the diplomatic support that accompanied the troops from Western contributors—enabled MINUSMA to effectively stabilize the country and monitor the political situation around the 2013 elections.

Previous studies largely agree that UN peacekeeping has a positive effect on the reduction of violence both in terms of battle-deaths and civilian killings. Findings differ, however, in the explanations offered for a reduction of casualties, pointing towards mission size (Fortna, 2008; Hultman, Kathman & Shannon, 2013), type of mission (Doyle & Sambanis, 2006; Hultman, 2010) or mission diversity (Bove & Ruggeri, 2016). We agree with existing research that troop size and mission diversity can reduce violence against civilians. We argue, however, that mission-level variation in a United Nations (UN) mission's troop quality has an independent and empirically observable negative effect on one-sided violence. Focusing on troop quality in this way helps to explain the question of mismatch between the numbers of troops deployed and mission effectiveness.

Higher quality peacekeepers are better equipped and trained to create buffer zones between combatants and to monitor ceasefires and peace agreements. Together, deterrence, buffer zones and better monitoring, including better intelligence and diplomatic pressure, raise the costs of one-sided violence and lower its benefits to both rebels and governments, ultimately reducing rebels' and government's targeting of civilians. Using military expenditure data from troop contributing countries (TCCs) to UN peace operations, we find empirical support for this argument in an extended sample of 70 intrastate conflict episodes in Africa and Asia between 1991 and 2010.

A number of factors could make this relationship between troop quality and the protection of civilians a spurious one. Former colonial powers, such as France or the United Kingdom

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[1] Both countries also saw a military intervention by France – Operation Serval in Mali, Operation Sangaris in the CAR – which contributed to a stabilisation of the countries alongside the peacekeeping missions.

(UK), systematically deploy troops to UN missions in their former colonies. Other countries with more advanced military equipment, such as Germany or Japan, have traditionally been risk-averse when it comes to UN peace operations. Other variables that could bias our results are the general political willingness of contributing countries to deploy troops, as well as training and organization of forces (Tellis et al., 2000: 143ff.; Brooks & Stanley, 2007). To tackle these problems, we subject our main analysis to a series of robustness checks. We re-estimate our models on a matched sample, with conflict-fixed and mission-specific fixed effects, as well as on a sample that includes only conflicts where peacekeepers were present. In addition, we estimate a wide range of model specifications that explicitly control for potential alternative explanations. Our results are robust to these empirical approaches, suggesting that they are not driven by selection effects.

Our contribution to the debate on the protection of civilians by peacekeeping troops is twofold. First, we complement existing scholarship on peacekeeping operations by offering a more fine-grained explanation of their violence-reducing effect and to the question why large or diverse peacekeeping troops are not always able to prevent violence against civilians. Second, we introduce systematic quantitative evidence into a critical policy debate. Peacekeeping officials, think tanks and many troop contributing countries have long demanded that countries with more advanced military technology increase their contributions to UN peace operations (Smith & Boutellis, 2013; United Nations, 2015). We show that such an increase not only in numbers, but also improvements in mission quality have a substantively significant effect in reducing violence against civilians.

Peacekeeping troop quality and violence against civilians

Peacekeeping troop quality is defined as the technical and personal capability of a peace operation as well as its political support to fulfil its mandate independently of troop size. The

technical and personal capability of a peacekeeping mission is largely a function of the quality of the troops from the troop contributing countries, including training as well as equipment such as weapons, battle dress, but also larger equipment such as helicopters, transport planes, or military vehicles (Daniel, 2008).[2] Political support refers to the TCC's diplomatic bilateral and multilateral engagement in the mission country alongside the deployment of its troops, which is dependent on a number of factors such as security threats towards the TCC, risk averseness or membership in the UN Security Council (Tellis et al., 2000, Ch. 7; Brooks & Stanley, 2007).[3] Consequently, holding all other factors constant, the better the quality of each troop contributing country's military deployment to a UN peacekeeping mission, the better will be the overall troop quality of the peacekeeping mission. The overall troop quality of the mission is crucial for the prevention of one-sided violence against civilians.[4]

One-sided violence is defined as 'the use of armed force by the government of a state or by a formally organized group against civilians' (Eck & Hultman, 2007: 235). Violence against civilians is a strategy by conflict parties to gain a better bargaining position (Eck & Hultman, 2007) or to enforce collaboration and punish defection (Valentino, Huth & Balch-Lindsay, 2004). One of the most important tasks of UN peacekeeping missions—besides the monitoring of ceasefires and implementation of peace agreement—is the protection of civilians and the reduction of one-sided violence (Holt, Taylor & Kelly, 2009). We argue that peacekeeping missions with higher overall troop quality are better able to protect civilians because they are better equipped, both logistically and politically, to thwart the use of one-sided violence by conflict parties. Specifically, high-quality troops are better able to deter

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[2] We focus on contingent-owned equipment (COE), i.e. equipment and soldiers from the TCC. This type of equipment exists alongside mission-owned equipment. To ensure that our troop quality measure does not reflect mission expenditures, we also control for PKO expenditure in our robustness checks.

[3] It does not include the mission's mandate, which we discuss separately below.

[4] 'Holding all other factors constant' implies the assumption that TCCs are also *willing* to use higher quality troop commitments in PKOs. Empirically, there are exceptions to this. We therefore relax this assumption and address the question of whether some kind of TCCs (e.g. risk-averse or strategically-motivated TCCs) systematically commit and deploy troops to certain kinds of operations in the robustness checks section.

violence from state and non-state actors, to create buffer zones between combatants, and to monitor ceasefires and peace agreements which raise the costs of one-sided violence and lower its benefits to both rebels and governments.
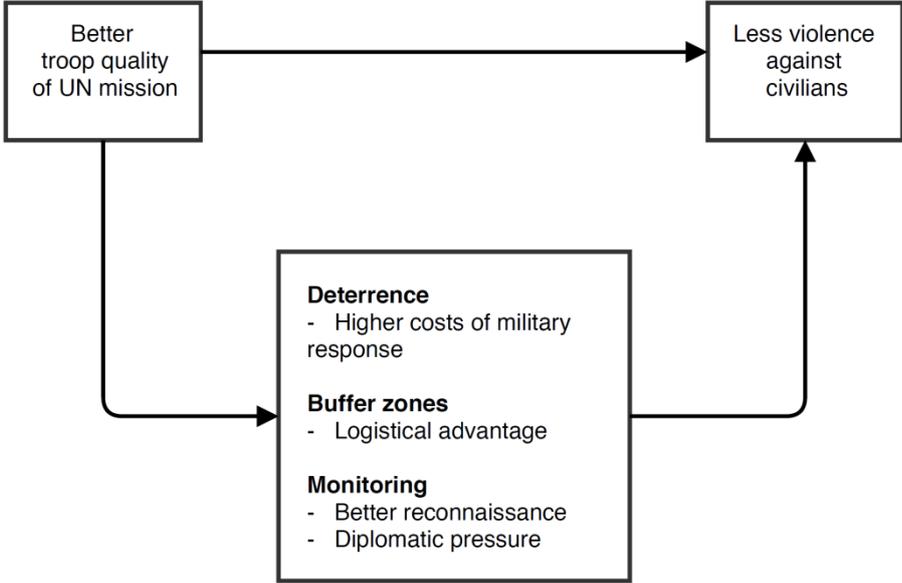
First, high-quality peacekeeping missions can better deter conflict actors that target civilians than lower-quality missions. If we consider violence against civilians as a strategic action by conflict actors, better equipped peacekeeping missions are potentially more dangerous to conflict parties as they function as costly response to the targeting civilians by conflict parties. Thus, the potential of high-quality peacekeeping missions to deter attacks increases combatants' costs of civilian victimization to a greater extent than the responses of peacekeeping missions of lower quality. In February 2014 a review of the African Union/United Nations Hybrid operation in Darfur (UNAMID), stated for instance that 'shortfalls in the operational capabilities of the Mission's military and police components [are a] key challenge to effective mandate implementation, particularly with respect to the protection of civilians and ensuring unhindered humanitarian access. Those shortfalls seriously constrain the force's mobility, effectiveness and ability to *deter attacks*' (United Nations, 2014: 8, emphasis added).

Second, high-quality missions are in a better position to create buffer zones between rebel groups and between combatants and civilians, because they are better able to respond to the logistical challenges posed by conflict-affected territories. Conflict countries typically suffer from enormous destruction of infrastructure. The main challenge peacekeepers face in such an environment is the logistics of moving troops in remote regions of the country. In the absence of functioning streets and railways, better military equipment such as transport helicopters or planes is essential to deploy peacekeepers even in remote areas (Ruggeri, Dorussen & Gizelis, 2016). Without such equipment, even nominally large peacekeeping operations will have

difficulties in creating effective buffer zones between conflict parties that prevents them from targeting civilians. The UN Mission in South Sudan (UNMISS) is a case in point: Despite a mission size of over 12,000 armed peacekeepers, more than 100 civilians and four UN peacekeepers were killed in attacks between December 2013 and July 2016 (Patinkin, 2017). The lack of civilian protection was caused by the force being too stretched to patrol a country the size of France; hence it was not able to deploy to a rebel stronghold in the Northeast, where a government offensive had displaced thousands of civilians (ibid.).

Third, high-quality peacekeeping troops are also in a better position to monitor conflict parties' behavior. This is especially important when the tasks of the peacekeeping mission include the oversight of a ceasefire or peace agreement, when the process of keeping the peace and political negotiations towards a long-lasting solution go hand in hand. Better equipment, such as planes, satellite imagery, and radio equipment allows more effective reconnaissance, especially of remote areas, and thus better monitoring of combatants. The failure of the United Nations Angola Verification Mission III (UNAVEM III) in Angola highlights this point. Nicholas Howen, the mission's Human Rights Unit director, stated: 'UNAVEM [III]'s intelligence was poor. Communication flows between the provinces and its […] headquarters in Luanda were sporadic; liaison between different departments […] itself was often fragmented due to factionalism among staff and overly bureaucratic procedures' (cited in Guyot & Vines, 2015: 336). These operational difficulties contributed to the inability of the mission to uphold the ceasefire and both parties resumed violent campaigns against civilians shortly after.

Figure 1. The relationship between peacekeeping troop quality and violence against civilians

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┌──────────────┐                              ┌──────────────┐
│   Better     │─────────────────────────────▶│ Less violence│
│ troop quality│                              │   against    │
│ of UN mission│                              │   civilians  │
└──────────────┘                              └──────────────┘
        │                                            ▲
        │        ┌────────────────────────────┐     │
        │        │ Deterrence                 │     │
        │        │ -  Higher costs of military│     │
        │        │    response                │     │
        │        │                            │     │
        └───────▶│ Buffer zones               │─────┘
                 │ -  Logistical advantage    │
                 │                            │
                 │ Monitoring                 │
                 │ -  Better reconnaissance   │
                 │ -  Diplomatic pressure     │
                 └────────────────────────────┘
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Another channel through which high-quality troops increase a mission's ability to monitor (and influence) combatant behavior is increased diplomatic pressure. A peacekeeping mission is often accompanied by a number of mediation and negotiation strategies that aim to increase the diplomatic pressure on the conflict parties. Troops in PKOs with higher quality equipment typically come from countries with higher military expenditures. Those countries which are willing and in the logistical position to deploy troops to a given conflict are often more willing to engage diplomatically to end the conflict (Camiña & Porteiro, 2009). For instance, the Lomé peace agreement between the government of Sierra Leone and the rebel group RUF was signed after international pressure and sanctions by the UN and other actors such as the regional organisation ECOWAS (see UN Security Council resolution 1132). While it did not bring a final end to the violence, it paved the way for the settlement of the armed conflict.

H: A UN peacekeeping operation with higher troop quality reduces the level of violence against civilians.

Figure 1 summarizes our argument and the causal mechanisms.

Data and research design

To test our hypothesis, we quantitatively investigate the impact of peacekeeping troop quality on incidences and magnitude of one-sided violence. Starting point for our analysis are all intrastate conflicts between 1991 and 2010 captured by version 1.9 of the Uppsala Conflict Data Program Georeferenced Event Dataset (UCDP GED) (Sundberg & Melander, 2013). To be included in the GED dataset, a conflict must have exceeded 25 battle-related deaths per year.[5] The GED project further provides data on all three of the UCDP's categories of organized violence: state-based armed conflict, non-state conflict, and one-sided violence. The last category allows us to combine information on armed conflict with incidences of one-sided violence by the government, rebel groups, or both. Also, 'the theoretical processes associated with victimization may continue after the cessation of hostilities' (Hultman, Kathman & Shannon, 2013: 882). Thus, we add two years of observations to the end of each conflict episode. These selection criteria result in the inclusion of 70 conflict episodes in our dataset, 39 in Africa and 32 in Asia (see Appendix A). From this base sample we create a dataset with conflict-months as unit of observation.[6]

Dependent variable: one-sided violence (OSV)

We measure the protection of civilians as the monthly count of one-sided violence as reported by the GED dataset. In our mechanism analysis below, we distinguish between OSV conducted by rebels and OSV perpetrated by the government.

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[5] We exclude Rwanda from our sample since the Rwandan genocide in 1994 constitutes a substantial outlier in the count of civilian deaths as a result of one-sided violence.
[6] The dataset design follows Hultman et al. (2013), but on an updated sample built from the UCDP GED version 1.9. GED 1.9 includes both a longer period of observation (until 2010, as opposed 2008) and a greater geographical scope (Africa plus South East Asia, as opposed to sub-Saharan Africa).

Measuring UN peacekeeping troop quality

We consider a TCC's overall military capabilities as a rough proxy for the quality of its deployed peacekeeping troops. Specifically, we use a TCC's annual military expenditures in constant 2011 US dollars divided by the number of armed personnel (short: *spending per capita*) to operationalize its military capabilities.[7] Similar to most of the existing literature, we use data from the Stockholm Institute of Peace Research (SIPRI) (SIPRI, 2014) on countries' military expenditures.[8]

We are aware of the fact that military spending per capita is an imperfect measure of a country's military capabilities. Military spending is an *input* measure and captures a country's willingness to spend money on its military. Military capabilities, on the other hand, are *output* measures. They denote a country's ability to translate its spending into military outcomes. Unfortunately, research lacks more detailed data on military capabilities and the translation of a country's input measures into actual military outcomes. Thus, expenditures are widely used in the literature as a proxy for military capabilities (Fearon & Laitin, 2003). This shortcoming is particularly pronounced when it comes to capturing training quality. Yet we are optimistic that our measure also captures training quality, at least to some extent. First, our primary measure is calculated as expenditures per number of armed personnel. This normalization by army size takes into account that oversized militaries, for instanced due to conscription, might also imply higher overall spending. Second, our measure is calculated in constant 2011 USD. This accounts for purchasing power differences across countries, including differences in

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[7] We also use total annual military spending (not normalized by military personnel) as well as spending as a share of GDP as a robustness check for the troop quality measure, which does not substantively alter our results. See Appendix D.2.

[8] SIPRI (2015: n.p.) defines military expenditures as 'all current and capital expenditure on the armed forces, including peace keeping forces; defence ministries and other government agencies engaged in defence projects; paramilitary forces when judged to be trained, equipped and available for military operations; and military space activities.'

salaries (which could reflect training quality), albeit imperfectly.[9] Finally, higher spending on equipment implies more complex machinery which, in turn, also requires better training to operate. Simply put, high expenditures on drones or surveillance equipment also require additional training. Thus, we are confident that our measure captures the training aspect of a country's military capability as well.

We believe that a country's military spending positively correlates with the quality of a country's troop contributions to a peacekeeping operation. Many Western countries that have high per-capita military spending rate compared to other militaries are also often very risk-averse when it comes to their military deployments (Schörnig & Lembcke, 2006). Consequently, we can assume that these countries' military spending translates to better equipment as well as diplomatic engagement with the mission country in order to protect their soldiers in the field – and thus also their soldiers in peacekeeping operations. Even if countries systematically select the least-trained units and the worst maintained equipment as their contributions to peacekeeping operations higher levels of spending should also be associated with a better equipment of such contributions, on average. To corroborate this proposition, we exploit a dataset by the Center on International Cooperation (CIC) (see Appendix E for details). The dataset contains information of the number of pieces of specialized equipment in UN peace operations, such as combat vehicles or transport helicopters for June 2014 (the only month for which data is available). We correlate our troop quality measure that is based on TCC military spending (see below) with the CICs recorded number of equipment pieces across different equipment types. We report a positive relationship between higher values of our mission-specific troop quality data and more

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[9] Imperfection in measuring salary disparities across countries not only stems from differences in purchasing power parity (for which we account), but also from different relative costs between personnel and equipment (for which we cannot account). To put differently, the costs of machinery typically remain relatively constant (fighter jets are expensive everywhere) whereas it less expensive to train fighter pilots in some countries than others. There is a lack of data on relative personnel vs equipment spending, however, so that we cannot directly measure this.

specialized pieces of equipment per mission. Despite this positive correlation, a number of factors can systematically influence which troops a country deploys to a mission (or if it deploys them at all). These factors include risk-aversion, strategic interests in the mission country, a culture of restrictive rules of engagement and others. We address these concerns in the robustness check section, particularly in Table D.1 in the Appendix.

To translate individual TCC military spending into a measure of a peacekeeping operation's troop quality, we exploit information about the size of its troop contributions to each mission. The United Nations Department of Peacekeeping Operations provides detailed data on these contributions on a monthly basis (Kathman, 2013; Perry & Smith, 2013). We use this information to construct a time-varying weighted average of military equipment for each mission-month for all missions in the data set, using the following formula:

$$\overline{Troop\ quality_{mt}} = \frac{\sum_{i=1}^{n} spending\ per\ capita_i * troops_i}{\sum_{i=1}^{n} troops_m} \qquad (1)$$

where for mission *m* in month *t* an average *equipment* is calculated for the sum of *spending* multiplied by *troops* for each country *i*, divided by the sum of all *troops* of the mission under consideration. Countries with less military spending will decrease the average measure of *Troop Quality* of the overall mission. By construction, countries with a higher number of troops will be given a higher weight in the quality measure than countries with a low number of troops. The following example illustrates the construction of our troop quality measure. Table I shows the troop deployments to the United Nations Operation in Cote d'Ivoire (UNOCI) in April 2004 together with each country's military expenditure per soldier in constant 2011 US dollars (in millions). The unweighted average of military spending for this mission-month is US$ 20.81m. We can see, however, that France has the highest military spending per soldier of all troop contributing countries for UNOCI in this month. The weighted mean of this mission-month, which explicitly takes into account this enormous

contribution of troops by France, is US$ 28.53m—considerably higher than the simple mission mean of military spending per soldier.

Table I. Troop contributions to UNOCI in April 2004

Contributor	Troops	Annual spending per soldier (in USD millions)
France	182	185.83
Brazil	1	35.65
Russian Federation	2	30.56
Uruguay	1	27.57
Kenya	4	19.10
China	2	16.93
Romania	2	14.48
India	4	13.05
Ghana	260	11.33
Benin	260	10.00
Morocco	49	9.65
Paraguay	1	8.42
Senegal	237	8.06
Burkina Faso	1	7.88
Jordan	4	7.73
Nigeria	3	7.24
Pakistan	6	6.66
Togo	231	5.22
Niger	312	4.60
Bangladesh	9	3.78
Gambia	1	3.38
Sum:	1572	
Weighted average:		28.53

Calculations based on IPI data on troop contributions (Perry and Smith 2013) and SIPRI data on military expenditures (SIPRI 2014).
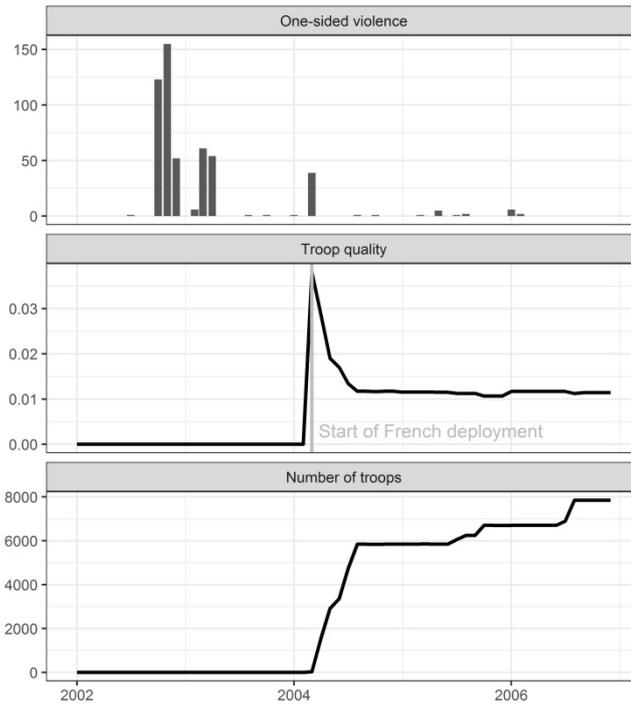
Figure 2. Troop quality, one-sided violence and troop strength of UNOCI

Figure 2 illustrates the relationship of this PKO troop quality measure with troop size, as well as civilian killings. The upper panel of Figure 2 displays the count of civilian killings in Cote d'Ivoire. The second panel visualizes the development of UNOCI's troop quality over time; the third panel shows the number of troops deployed to the country. Figure 2 indicates that France's early contribution to UNOCI led to a spike in the mission's equipment measure right after deployment. But the measure slowly declines as more troops from countries with lower military spending per soldier deploy troops to UNOCI, which pull the measure downwards. The plot also shows two other features of the data: First, it indicates that variation in troop quality is distinct from the actual number of troops on the ground. Indeed, the spike of UNOCI's troop quality is in March 2004 when only very few soldiers were actually deployed. The nominal mission strength increases while the troop quality declines. Second, one-sided

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violence seems indeed to be negatively correlated with an increase in troop quality, but also with an increase in troop strength. Visually, however, the effect is difficult to precisely disentangle from the effect of troop size. To separate out the effect of troop quality as opposed to troop strength we therefore turn to multivariate methods.

Model and control variables

Our empirical approach follows Hultman, Kathman, and Shannon's (2013) and Bove and Ruggeri's (2016) econometric strategy in estimating a model of the following form:

$$OSV_{it} = \eta(.) = Troop\ Quality_{it-1} + X_{it} + OSV_{it-1} + \varepsilon_{it} \quad (2)$$

where the subscripts i and t refer to conflict id and month, respectively. OSV_{it} denotes the total number of civilians killed in conflict-month it. $quality_{it-1}$ is a measure of our troop quality proxy lagged by one month and OSV_{it-1} is a lagged dependent dummy variable which takes 1 if in the previous month one-sided violence occurred and 0 if not in order to control for time dependency. X_{it} is a matrix of peacekeeping mission-specific and conflict-specific control variables (see below). ε_{it} is the month specific error term. $\eta(.)$ refers to a negative binomial link function, since our dependent variable is a count variable. The negative binomial link function is a reasonable choice as it controls for heterogeneity and contagion in the data and allows our data to be highly overdispersed with variance not equal to the mean (Fox, 2008: 391ff).

We follow Hultman, Kathman and Shannon (2013) and Bove and Ruggeri (2015) by including the following PKO-specific and conflict-specific control variables in our basic specification: the lagged monthly number of troops, police, and military observers deployed

to the conflict-month.[10] Although Kathman (2013) provides comprehensive data on troop contributions to UN peacekeeping missions, we construct this measure using data from the International Peace Institute (Perry & Smith, 2013) which hand-coded the same data as Kathman (2013). IPI's independent coding of the same data source of UN troop contributions (i.e. monthly reports by the UN Department of Peacekeeping Operations) serves as an additional robustness check to Hultman et al.'s original approach, which employs Kathman's data.

We include the same conflict-specific control variables as Hultman et al. (2013). A measure of *all battle-related deaths* as captured by the UCDP GED data set (Sundberg & Melander, 2013). *Incompatibility* is a dummy variable, indicating whether a conflict was fought over territory or government and is taken from UCDP (Themnér & Wallensteen, 2012). Finally we include *conflict duration* (measured in months since conflict onset) and *population* (logged), which is a country's population size in a given year (World Bank, 2015). The *battle-deaths* and the *duration* covariates explicitly allow us to control for potential adverse selection effects if troop contributing countries systematically shy away from particularly violent or long conflicts.

Results and analysis

We report the main results of our quantitative analysis of the effect of a PKO's troop quality on one-sided violence against civilians in Table II. Model 1 represents a baseline model, assessing the effect of troop quality on one-sided violence while controlling for a host of mission-specific and conflict-specific covariates. The negative and statistically significant coefficient for the troop quality variable in Model 1 indicates that as the average spending per

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[10] By construction, troop size and our measure of troop quality are correlated, since they can only occur together. This raises concerns about multicollinearity. However, correlation between both measures is weak (0.38) and variance inflation diagnostics for the models in Table II do not reveal any collinearity problems.

soldier of troop contributing countries increases, the level of civilian killings declines. Note that the negative relationship between troop quality and one-sided violence persists even though we explicitly control for the number of troops on the ground. This supports our argument that troop quality has an effect that is independent of troop size.

The coefficient for numbers of UN troops is negative and statistically significant across all models, which confirms the findings of previous research with our geographically extended dataset: an increased number of troops reduces violence against civilians. We do not find, however, the same negative and statistically significant effect of UN police on the reduction of one-sided violence as Hultman et al. Our findings for UN observers are similarly inconclusive across models. Our data suggest that only military troop, as well as a troop quality can lead to a reduction of violence against civilians.

Table II. Effect of peacekeeping troop quality on violence against civilians

	Base model (1)	Matching (2)	Fixed effects (3)	FE + Cubic time trend (4)	FE + PKO only (5)
Troop quality$_{(t-1)}$	-0.02***	-0.01***	-0.01†	-0.01*	-0.01**
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
UN troops$_{(t-1)}$	-0.26***	-0.22***	-0.06**	-0.06**	-0.09***
	(0.06)	(0.05)	(0.02)	(0.02)	(0.02)
UN police$_{(t-1)}$	0.32***	0.41*	0.28**	0.44***	0.24*
	(0.08)	(0.17)	(0.09)	(0.09)	(0.11)
UN observers$_{(t-1)}$	0.01***	0.004*	-0.0003	-0.0004	0.001
	(0.00)	(0.002)	(0.0004)	(0.0004)	(0.0006)
Government conflict	0.38				
	(0.56)				
Population	-0.61*	-0.01	0.22***	0.23***	0.38***
	(0.24)	(0.23)	(0.02)	(0.02)	(0.12)
All battle deaths$_{(t-1)}$	3.03***	0.0018*	0.31***	0.31***	0.15
	(0.74)	(0.00)	(0.08)	(0.08)	(0.15)
All OSV dummy$_{(t-1)}$	2.56***	1.77**	1.82***	1.78***	0.85***
	(0.28)	(0.55)	(0.05)	(0.05)	(0.14)
Conflict duration	-0.04	-0.01***	-0.01*	-0.07*	-0.03*
	(0.03)	(0.003)	(0.004)	(0.03)	(0.01)
Conflict duration^2				0.02***	
				(0.005)	
Conflict duration^3				-0.0008***	
				(0.0002)	
Constant	11.02*	3.33	-6.65***	-6.75***	-8.04***
	(4.83)	(3.47)	(0.42)	(0.43)	(1.86)
Alpha	2.40***	2.10***	-	-	-
	(0.18)	(0.33)			
No. of observations	7,934	1,382	5,413	5,413	637
No. of conflict episodes	70	17	47	47	21
Wald χ^2	499	176	1,506	1546	119
Log-Likelihood	-13,354	-3,481	-11,286	-11,261	-1,221

† p<0.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors clustered on conflict. Government conflict drops out of Models 2 to 5 due to collinearity.
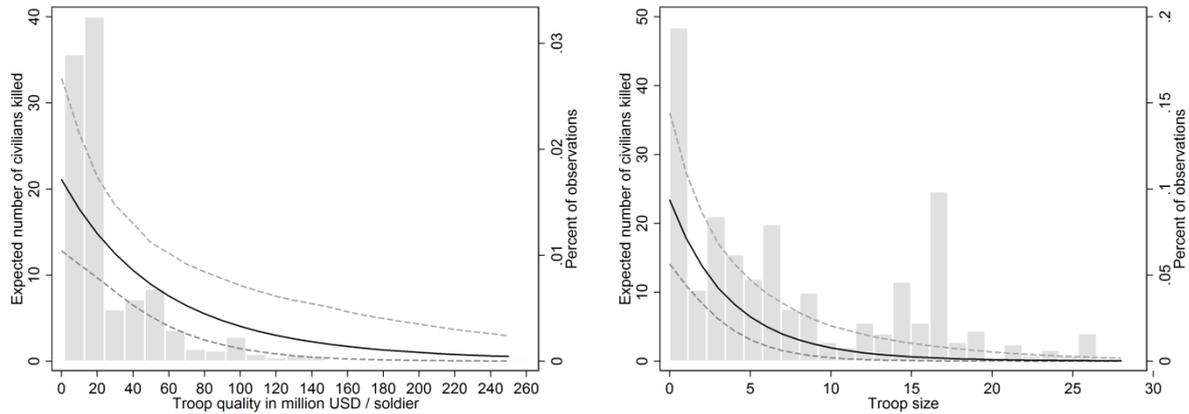
Figure 3. Substantive effects of troop quality and troop size on civilian killings
Left panel simulates an increase in troop quality measured as the weighted average expenditure measure per soldier in steps of USD 20 million. Right panel simulates an increase of troop size in steps of 5,000 soldiers. Dashed lines represent 95% confidence intervals.

Troop quality is not only a statistically significant predictor of reduced one-sided violence, but has also a substantively meaningful effect. Figure 3 plots the substantive effects of troop quality and troop size on one-sided violence against civilians. In the left panel of Figure 3, we simulate the effect of an increase by steps of USD 20 million per soldier on one-sided violence.[11] If we move from zero to an average TCC spending per capita of 115 million USD the number of civilians killed is reduced from 21 to about 4. This corresponds to an over 90% reduction in civilian deaths as our troop quality measure increases from zero to 115.

The right panel of Figure 3 simulates an increase in the number of peacekeeping troops. This allows us to compare the effect of troop quality and troop size on one-sided violence. We see that troop size has a slightly larger effect on the reduction of civilian victimization than troop quality.

We can use our information about military expenditures of troop contributing countries to simulate the effects of a troop quality change in a given conflict-month. Consider, for

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[11] We held all quantitative variables at their mean and set government conflict and lagged OSV dummy variables to 1. Both simulations in Figure 3 are based on Model 1 in Table II and were conducted in Stata 13 using Clarify (King, Tomz & Wittenberg, 2000).

instance, a hypothetical mission similar to the MONUC mission in the DR Congo in June 2002. Our troop quality measure for MONUC in the DR Congo in June 2002 is around USD 22.2 million weighted average military spending per soldier of all TCCs. In the simulated results of Figure 3, a troop quality measure of USD 22.2 million corresponds to about 15 killed civilians per month. In June 2002 both the United Kingdom and France contributed five soldiers to MONUC. The bulk of the deployment was shouldered by troops from Uruguay, Mozambique, Senegal, and Ghana with relatively low military spending per soldier, thus contributing to the rather low troop quality measure of MONUC in that particular month. If we hypothetically add 1,000 troops by France and the UK respectively to that month and keep the deployments of all other troop contributing countries as they were, our troop quality measure almost quadruples to over USD 88.26 million. This corresponds to about five civilians killed, i.e. a drop of 10 civilian victims in comparison to the actual situation in the DRC in June 2002. This suggests that a substantial commitment by troop contributing countries with a better equipped military can substantively reduce the killing of civilians.

The main problem to the robustness of our results from Model 1 stems from the non-random selection of the UN missions to which countries with better equipped military deploy their troops. We therefore follow Hultman et al. (2013) and Gilligan and Sergenti (2008) and reanalyze Model 1 on a matched sample and as well as with models that include both conflict-fixed effects and mission-specific fixed effects.

Matching allows us to create a dataset which includes only similar observations, but which differ with respect to whether a peace operation was deployed or not. Datasets pre-processed in such a way resemble fully-blocked experiments which compare a treatment (= deployment

to peace operations) and a control (= no deployment to peace operations) group which are sufficiently similar but differ in their treatment status (Ho et al., 2006).[12]

We include the covariates from Model 1 in Table II in our matching approach to explicitly model the selection of deployment to peace operations dependent on these covariates.[13] Since we include measures of conflict intensity such as battle-deaths and a lagged dummy of whether one-sided violence occurred or not, we can systematically control for selection on conflict intensity variables. Diagnostic statistics reveal that the matching procedure increases balance between control and treatment groups (see Appendix E).

Model 2 in Table II reports the results from an optimal matching without replacement, using the Mahalanobis distance between observations. The negative and statistically significant coefficient for troop quality in Model 2 provides further evidence for our hypothesis: if we compare conflict-months that are similar except for the fact that a peace operation was deployed or not, those months with higher quality missions deployed in the previous month see significant reduction in violence against civilians.[14] The results from the matching analysis increase our confidence in the causal interpretation of the violence-reducing effect of higher-quality peace operations.

A second source of potential bias comes from time-invariant variables, both on the level of the conflict and on the level of individual mission, which might drive the systematic

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[12] This treatment choice reflects a trade-off. Matching is designed for binary treatments. However, the continuous nature of the troop quality variable makes it difficult to dichotomize it as a binary treatment without substantial loss of information. Thus, PKO presence—while controlling for troop size within operations to tease out the independent effect of troop quality—most reasonably reflects such a binary treatment while only imperfectly captures troop quality treatment. In an alternative approach, we dichotomize troop quality with varying cutoffs at lower, middle, and upper quartiles of the troop quality variable. Using the resulting dummy as treatment variable, our results do not substantively change, except when we use a 75% cutoff. This could indicate that increasing troop quality is most effective at lower levels, consistent with Figure 3. See Appendix E for details.

[13] The *Government Conflict* dummy drops out due to collinearity.

[14] We include the mission-specific covariates such as number of troops, police, and observers as well as the other covariates in Model 2 to account for any residual variance that remains after the matching procedure (Ho et al., 2006). We nevertheless re-estimate Model 2 without covariates. The results do not substantively change (see Appendix E).

deployment of troops to some peacekeeping operations and some conflicts but not to others, such as in the case of France and its former colonies. To account for such (and all other sources of) time-invariant heterogeneity, we reanalyze Model 1 using conflict-level fixed effects. The results are reported in Model 3 of Table II. Additionally in Model 4, we add a cubic time trend to the fixed effect specification of Model 4. The coefficient for troop quality becomes smaller, but remains negative and statistically significant at conventional levels. In Model 5 of Table I we restrict our sample to observations in which a peacekeeping operation was deployed. This allows us to compare only conflict-months in which a peacekeeping operation was deployed, controlling for any factors that might drive the systematic selection of peace operations. Further, we include mission-level fixed effects in Model 5 of Table II, since time-invariant unobserved heterogeneity across missions might systematically affect the deployment of troops to a mission. Even after accounting for mission-level fixed effects in the PKO-only sample, the effect of troop quality remains negative and statistically significant. We are therefore confident that our results are not biased due to adverse selection on observable or time-invariant unobservable factors.

None of our control variables has a consistent and robust effect on the intensity of violence against civilians across all model specifications. The coefficient for population is negative in Model 1, but positive and statistically significant in the fixed effects specifications, indicating that a counterintuitive negative effect of population size in Model 1 is largely driven by between-country variation in population. The positive coefficient for population in the fixed effects models is in line with expectations: in highly populated countries, chances for civilian targeting increase. The lagged dependent variable is positive and significant, indicating serial correlation of civilian targeting over time, which is in line with the findings of Hultman et al (2013).

Probing causal mechanisms

Having established a general relationship between better troop quality and reduced targeting of civilians, we turn to an analysis of the underlying causal mechanisms proposed in our theory. Exploring causal pathways not only strengthens the credibility of our overall theory by testing additional observable implications, but allows us to interpret our results in a more fine-grained manner and to better inform the policy debate.[15]

Deterrence

The first causal mechanism we propose is that better equipped peacekeepers are better able to deter combatants. A peacekeeping mission with better offensive and defensive capabilities and more professionally trained personnel poses a greater threat to perpetrators of violence against civilians than missions without these features. Deterrence can manifest itself through swift defensive or offensive actions in reaction to attacks against civilians, signaling peacekeepers' future resolve in similar situations. The intervention brigade of the UN Peacekeeping mission in the DR Congo, for instance, was mandated as a direct reply to the ongoing violence against civilians and to break the persistent cycles of violence. But deterrence does not necessarily require prior civilian targeting. Peacekeepers can also deter civilian attacks by showing strength and presence—something that missions with newer equipment and more training should be better at than missions with less professional troops and frail machinery. In these ways, deterrence should prevent rebel and government forces from civilian targeting not only in the short-term, but also in the future.

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[15] We report here only marginal effects and coefficient plots and refer to our supplementary appendix (section C) for detailed model results.
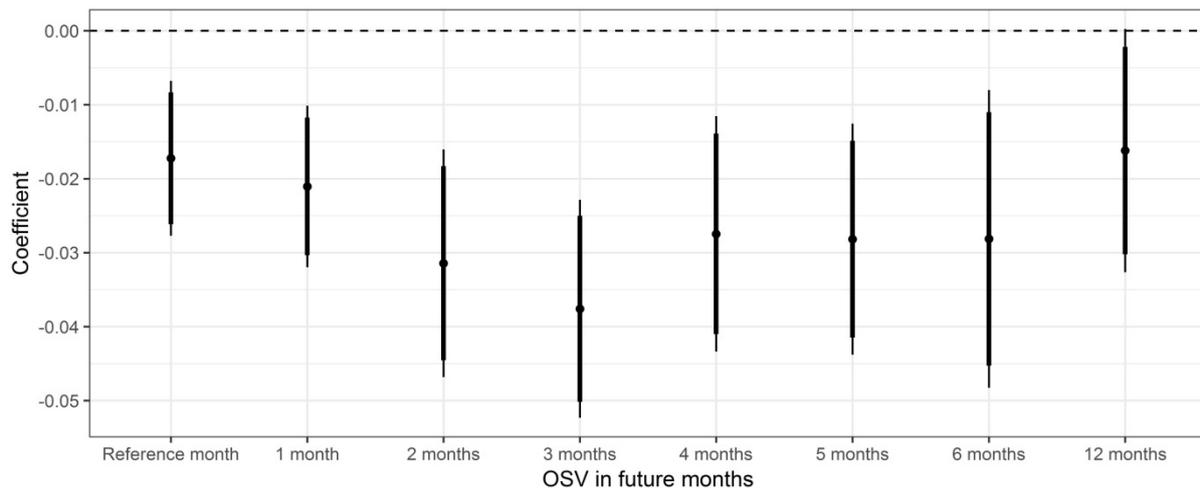
Figure 4. Probing mechanisms I: Deterrence and long-term effects of troop quality

Coefficient estimates with 90% (solid line) and 95% (thin line) confidence intervals. "Reference month" is the OSV count in the month used in Model 1, Table 2. "1 month", "2 month" etc. refer to 1, 2, etc. month leads of the OSV variable.

Empirically, deterrence thus implies a negative effect of troop quality on civilian targeting that persists over time. Our empirical strategy so far, however, has aimed to establish the relationship between higher score on our quality variable and levels of civilian targeting in the next month. From the resulting model setup (see Equation 2), we therefore cannot know whether the effect persists over time—it could be that the coefficients in Table II only represent a short-term dent in the level of one-sided violence in the next month while jumping back to conventional levels after that. To test for temporal persistence, we thus estimate Model 1 from Table II using varying leads of our dependent variable. The results are plotted in Figure 4. The 'Reference Month' coefficient represents the troop quality coefficient from Table II, whereas the other coefficients represent the relationship between troop quality and one-sided violence in future months. Consistent with our expectations, Figure 4 shows that the effect of troop quality persists over time. The association even becomes stronger for future levels of violence against civilians in the medium-term up until one year after the reference month. We take this as evidence that troop quality indeed shapes one-sided violence through a peacekeeping missions' ability to deter attacks.
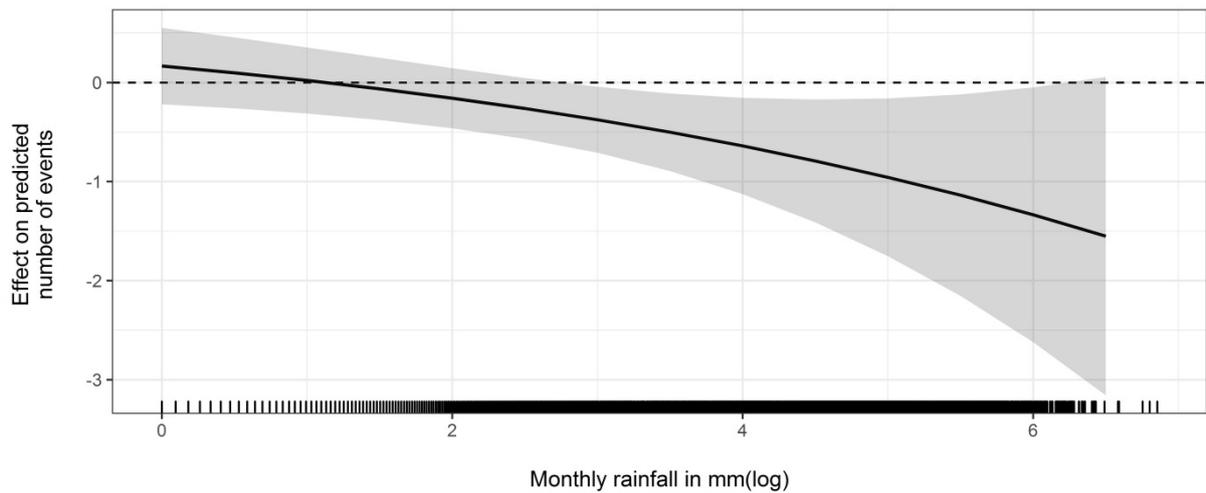
24

Figure 5. Probing mechanisms II: logistical advantage and rainfall

Plot shows the conditional marginal effect of a one unit increase in our troop quality measure (=1 million USD) at varying monthly rainfall levels. The shaded area represents a 95% confidence band. The rug plot on the bottom of the x-axis shows the distribution of the rainfall variable.

Logistics

The second causal mechanism we put forward concerns the role of logistical advantage of high-quality peacekeeping troops. The UN often stresses the challenges posed by environment and weather conditions, such as rain seasons (Government Accountability Office, 2008: 52).

This implies that the effect of high-quality troops should be particularly visible in environmentally harsh environments. We use a country's monthly rainfall levels to approximate severe weather conditions.[16] We add an interaction term between monthly rainfall levels and our troop quality measure to Model 1 from Table II. We expect a negative sign of the interaction term which indicates a negative effect of troop quality on civilian

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[16] Another potential variable that approximates difficult environmental conditions is mountainous terrain (Fearon & Laitin, 2003). This variable is time-invariant, however, and strips our monthly observations from much of the within-group variation, making the direct effect less interpretable. Nevertheless, in the appendix, we use mountainous terrain as variable to control for a potential omitted variable bias resulting from better equipped troops being systematically sent to logistically more difficult (= more mountainous) countries.

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targeting in particularly rainy seasons.[17] Figure 5 plots the conditional marginal effect of troop quality at varying levels of rainfall. The relationship is negative as expected and is particularly strong in months characterized by heavy rainfall. We interpret this result as evidence that better equipped peacekeepers do indeed have a logistical advantage in environmentally harsh conditions when it comes to preventing attacks against civilians.

Monitoring

The third proposed mechanism states that high-quality peacekeepers are better able to monitor conflict parties' behavior, thus improving the mission's capability to respond to civilian emergencies. We identified two channels through which monitoring might work: First, high-quality peacekeepers have better means for communication through better access to more advanced communication equipment. Second, high quality peacekeepers enjoy diplomatic pressure by countries willing to deploy high-quality peacekeeping troops to a conflict.

An additional empirical implication regarding the first monitoring channel—communication—concerns the relationship between mission diversity and access to communication technology. Bove and Ruggeri (2016) have shown that mission diversity can play a beneficial role for reducing attacks against civilians. More diverse missions—measured through a troop fractionalization index that captures the range and diversity of countries that contribute troops to a peacekeeping force—better complement each other's capability and increase their ability to observe misconduct. At the same time, more diverse troops can also induce coordination problems due to the large number of different actors involved. We argue that better equipment strengthens the positive impact of diversity and reduces the costs of coordination problems induced by mission diversity. To test this argument, we introduce an interaction term between mission diversity taken from Bove and Ruggeri (2016) and our troop

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[17] Data on rainfall patterns is taken from Harris et al. (2014). We take the natural log and lag the rainfall variable by one month.

quality measure of Model 1 from Table II, expecting a negative relationship that indicates a conditionally negative effect of troop quality and higher levels of mission diversity.[18]
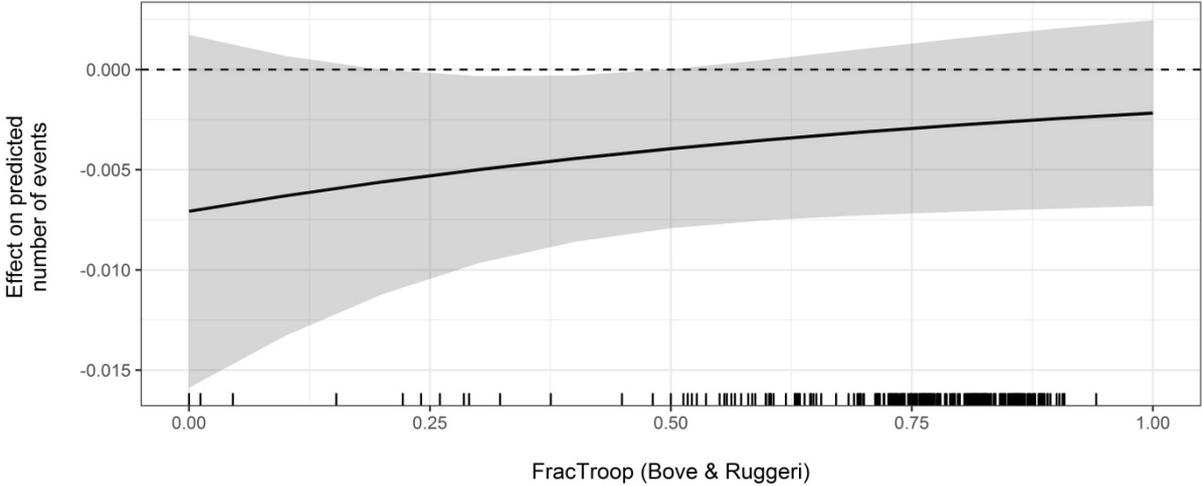

Figure 6. Probing mechanisms III: information and mission diversity

Plot depicts the conditional marginal effect of a one unit increase in our troop quality measure (=1 million USD) at varying levels of Bove and Ruggeri's (2015) troop fractionalization measure. The shaded area represents a 95% confidence band. The rug plot shows the distribution of the troop fractionalization measure

Figure 6 does not show the expected effect, however. There is a statistically significant marginal effect of troop quality at low levels of fractionalization (between 0.25 and 0.5 on the fractionalization index score). Nevertheless, the effect is substantively very small and statistically insignificant over most of the range of the fractionalization variable. We therefore cannot rule out a null effect, questioning our confidence in a strong interpretation of this channel of the monitoring mechanism.

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[18] The model without interaction between troop fractionalization and troop quality also serves as an additional robustness check whether our results are not driven by the omitted variable of troop fractionalization, which they are not.
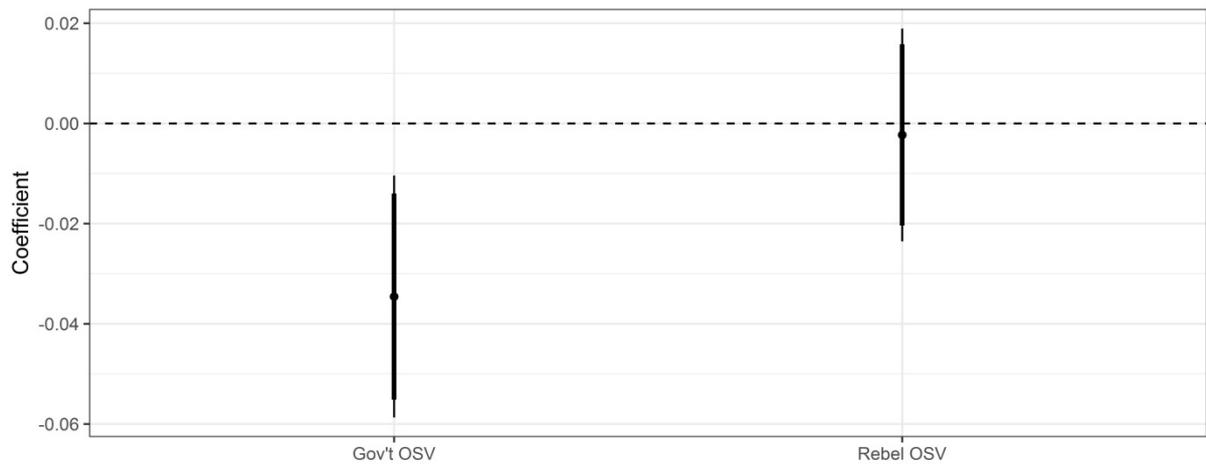
Figure 7. Probing mechanisms IV: diplomatic pressure and government vs rebel OSV

Coefficient estimates with 90% (solid line) and 95% (thin line) confidence intervals. "Gov't OS\" refers to one-sided violence perpetrated by the government only; "Rebel OSV" refers to one-sided violence perpetrated by the rebel side only

Another implication follows from the second channel, diplomatic pressure: If troop contributing countries accompany their troop deployment with diplomatic actions targeted at reducing civilian killings, this effect should be visible in different effects on the perpetrators of one-sided violence. Specifically, we expect that high-quality troops should be more clearly associated with a decrease in OSV by the government side than by the rebel side. Governments are much more amenable to diplomatic pressure: they rely on foreign aid and the diplomatic support by TCCs in international organizations, and are thus more vulnerable to political and economic pressure by donor states.[19] Furthermore, governments often prevent the international community to directly engage in diplomatic talks with rebel groups out of fear that this might legitimate insurgents. Also, governments are typically the direct

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[19] In section G of the Appendix, we provide evidence that troop quality is indeed positively associated with foreign aid.

counterpart when negotiating peacekeepers' rules of engagement, which often also reflects the top-down, nation-level approaches of most UN peace operations (Autesserre, 2010).

Splitting the OSV variable into rebel and government OSV, we find support for this expectation as reported by Figure 7. While there is a substantively stronger relationship between troop quality and OSV perpetrated by the government side, the effect becomes small and statistically insignificant for rebel violence only. This indicates support for the diplomatic pressure channel of our third causal mechanism.[20]

Alternative explanations

We have provided statistical evidence for the argument that better equipped and better trained peacekeepers from countries with higher military expenditures systematically reduce the level of one-sided violence against civilians and illustrated the causal channels through which this effect might run. There is a chance, however, that this pattern results from other factors that systematically drive both variation in peacekeeping troop quality and violence against civilians. We therefore conducted a series of robustness checks that we summarize here for brevity and present in more detail in the appendix.

One challenge to our proposed mechanisms is that they implicitly assume that TCCs with high quality militaries are *willing* to both contribute and deploy their troops within PKOs. To account for the presence or lack of such political will we conduct a series of robustness checks. First, we include a control for the mean cumulative number of casualties to which a

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[20] Figure 6 might seem to undermine our argument above: Logistical advantages and deterrence should also apply to OSV perpetrated by rebels—yet the small, insignificant coefficient for rebel OSV suggests that it does not. This interpretation is misleading, however. As we document in more detail in Figure C.1 in the Appendix, the seeming null finding for rebel OSV in Figure 6 masks different temporal dynamics of the effect of troop quality across OSV types. Figure C.1 shows that the effect of troop quality on Government OSV is visible immediately in the following months. The effect of troop quality on rebel OSV, however, manifests only after about 2 months. Thus, a mission's higher troop quality is more effective in limiting civilian victimization immediately, suggesting that diplomatic pressure is a causal channel. Yet the statistically significant—albeit smaller—coefficients for rebel OSV after 2 months onward (as well as our findings for the interaction with rainfall above) also provide evidence in favor for the logistical and deterrence channels.

mission's troop contributing countries were exposed since 1990. This measure should capture (at least to an extent) the risk-aversion of TCCs (in addition to the control variables for battlefield violence):[21] a TCC that has suffered from many fatalities in prior peace operations might be more restrictive in its rules of engagement due to prior 'bad experiences,' such as the United States after its participation in the disastrous UNOSOM II mission in Somalia in the early 1990s. Data for this measure is taken from the *Peacemakers at Risk* dataset (Bromley, 2018). Second, we include a cumulative count of mission-specific casualties. Not only the TCC-specific experience of casualties might change deployment rules, but also the fact that peacekeepers have died on the very mission to which countries have deployed their troops. Data for this variable also comes from the PAR dataset. Third, we include a count of the number of TCCs that represent one of the P5 countries in the UN Security Council (UNSC). If our measure reflects only the impact of the PKO participation by powerful countries, the independent effect of troop quality should disappear after including this variable. In an alternative specification, we use the count of both P5 and non-permanent UNSC members. Fourth, regional proximity might affect a TCC's propensity to deploy troops within a mission. Conflicts in countries that are closer to a TCC imply higher security concerns than conflicts in far-away countries—which might trigger troop deployment. To control for this, we include a count of how many TCCs in a mission come from the same UN regional bloc as the mission country. Finally, some countries have very specific national deployment rules that are often historically determined. Swedish troops in the Malian MINUSMA mission, for instance, have stricter deployment caveats due to evacuation concerns than their Chadian counterparts. Our mission- and conflict-fixed effects models in Table II account for this to some extent (since many of these restrictions are of historical nature and thus time-invariant). Nevertheless, we include a count variable that captures how many European countries (including Japan)

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[21] We are aware that this is only an imperfect indicator, as even risk-averse troops can be attacked.

participated in the mission, with the expectation that if our result (and troop quality measure) is entirely biased by these deployment restrictions it should disappear once we control for this explicitly. In all model specifications (see Table D.1 Appendix), the coefficient for the troop quality proxy remains negative and statistically significant. We are therefore confident that our results are not driven by a systematic bias in the political willingness of TCCs to commit and deploy their troops to a PKO.

Another possible alternative explanation is that it is not the peacekeepers' troop quality which leads to reduced OSV, but that both the presence of high-quality peacekeepers and lower levels of civilian killings are driven by a ceasefire between parties (Fortna, 2004). An additional potential source of bias is the type of mandate (Hultman, 2010). Two types of mandates might systematically affect our results: first, a mandate that allows for the use of military force (Chapter VII mandates) and/or a mandate that explicitly calls for the protection of civilians. Since TCCs with better equipped militaries might systematically shy away from deploying their troops to missions where soldiers might be put into harm's way, the type of mandate might be driving both a mission's average troop quality as well as the mission's ability to protect civilian from violence. Finally, deployment of better equipped troops to UN peace operations could also be affected by the presence of non-UN troops in the same country to a different mission that might or might not support the UN peacekeeping operation (Gaibulloev et al., 2015). Many Western countries choose to deploy their troops to peace operations—quite often, however, not under UN command, but either under their own command structure or in the context of an operation led by a regional organization, such as the European Union or NATO.

Controlling explicitly for these variables does not substantially change our results (see Appendix D.2). We report the results of a series of additional robustness checks in the

supplementary appendix. We test whether the effect of troop quality is robust to (1) an alternative measure of troop quality that only uses the weighted average of troop contributing countries' military expenditures instead of also weighting by military personnel, (2) the exclusion of Sudan as potentially distorting case, (3) adding a measure of total UN mission expenditures[22] (4) a model in which we include all variables presented in the previous section, including mission fixed effects, (5) varying time lengths of post-conflict periods and (6) excluding TCCs with less than 40 troops. Across these different specifications, our results remain largely stable: higher measures of troop quality are consistently associated with fewer civilian killings.

Conclusion

In this article, we demonstrate that troop quality of UN peace operations can reduce civilian victimization in internal conflicts. Well-trained troops with adequate equipment, high-quality intelligence, and diplomatic support from their troop donors are better able to deter violence from state and non-state actors, can reach remote and inaccessible locations, inflict higher costs to combatants who target civilians and have superior abilities in monitoring peace agreements than similar operations with troops that are less apt for peacekeeping tasks. These operational advantages translate into better capabilities to protect civilians.

Our findings complement existing scholarship on peacekeeping operations by offering a more fine-grained explanation of their violence-reducing effect and to the puzzle why large or diverse peacekeeping troops are not always able prevent violence against civilians. Our evidence suggests that troop quality is a critical ingredient for successful peace operations, in addition to troop size and diversity.

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[22] Since the UN compensates TCCs this variable ensures that our troop quality measure does not reflect UN mission expenditures.

Future research should invest in better measures of mission capacity. While our proxy indicator has sufficient face validity and does relatively well capture the broad tendency of a mission's operational capacity, it misses specific nuances. For instance, we lack precise, comparative and comprehensive information about training of forces, airlift capacity, vehicle quality, or intelligence capabilities, particularly when it comes to mission owned equipment. Another avenue for future research, particularly quantitative studies, is the decision-making process of TCCs to deploy specific types of troops to PKOs. Existing research has largely focused on why countries contribute troops to PKOs in the first place (Gaibulloev et al., 2015). Why countries deploy certain *kinds* of troops with varying rules of engagement is less understood, however. In this article, we treat this question only as a source of bias for the relationship between troop quality and civilian protection. Yet we believe that putting deployment strategies front and center as well as more direct measures of PKO's operational capabilities holds much potential to better understand peacekeeping performance.

Replication data

Replication scripts, data, and the online appendix can be found at http://www.prio.on/jpr/datasets. All analyses were conducted in R 3.4.1 and Stata 13.1.

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