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Help is close at hand? Proximity and the effectiveness of peacekeepers

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Abstract

How do the national origins of peacekeepers influence peacekeeping operations' success? We argue that peacekeeping operations better protect civilians when a higher percentage of peacekeepers come from geographically proximate countries. These peacekeepers have been exposed to similar societal and cultural norms and are more invested in preventing conflict diffusion. Peacekeepers from proximate countries can better collect and analyze intelligence, are more effective at separating combatants, and are therefore more successful at protecting civilians. In making this argument, we also challenge the theory that diversity in a peacekeeping operation matters. We find support for both our mechanisms and show that the importance of diversity may have been overstated. Where a peacekeeping operation is present in civil conflicts, if a quarter of its personnel come from proximate countries, then all things being equal, it would completely prevent civilians dying. The results show policymakers the importance of recruiting peacekeepers from countries near to conflicts.

Keywords

Peacekeeping, United Nations, Africa, international organizations, civilian protection, contiguity

How do the national origins of peacekeepers influence peacekeeping operations' (PKOs) success? The determinants of PKO success have important policy implications, and there is a growing body of quantitative scholarship devoted to PKO effectiveness. Scholars have largely focused on how PKOs as homogeneous entities shape PKO success (e.g. Beardsley, 2011; Fortna, 2004), and have only recently begun to examine how PKO composition matters (e.g. Beardsley and Gleditsch, 2015; Karim and Beardsley, 2013; Kathman and Wood, 2011). For example, Hultman et al. showed that more troops and police can reduce civilian deaths (2013), and increased troops can minimize battlefield deaths (2014).

We argue that peacekeepers from geographically proximate countries help PKOs better protect civilians.[1] Their proximity means these peacekeepers have had exposure to and understand similar societal and cultural norms, and may also be more invested in preventing conflict diffusion. Thus, peacekeepers from proximate countries can better collect and analyze intelligence, more effectively separate combatants, and overall better protect civilians.

However, recent analysis on peacekeeping effectiveness has highlighted the role of the national diversity of PKOs. Bove and Ruggeri (2015) argue that a PKO's diversity, in terms of how many countries contribute peacekeepers, helps protect civilians. However, theoretically we suspect that diversity is less important than other issues. If a PKO has forces from a range of countries but there are no personnel who understand the cultural and societal norms of the host country, then this will likely inhibit peacekeepers' abilities to alleviate tensions, manage disputes, and collect local intelligence.

Our argument provides an alternative perspective to Bove and Ruggeri's (2015) notion that diversity matters. However, to be clear, we do *not* argue that diversity is unhelpful and that similar PKOs are more effective. Rather, both diverse and similar PKOs *could* be correlated with effectiveness. For example, the Democratic Republic of the Congo (DRC) is geographically vast and has nine borders. If peacekeepers from geographically proximate countries

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can aid effectiveness, then the United Nations Organization Stabilization Mission in the DRC may need a diverse group of peacekeepers. Conversely, Haiti is significantly smaller and shares a border with just the Dominican Republic. Thus, a more similar PKO may be more effective. We agree with Bove and Ruggeri's (2015) contention that the national origins of peacekeepers matter but move the discussion away from the broad notion of diversity, instead arguing it must be analyzed in the context of the host country. Indeed, their recent article (Bove and Ruggeri, n.d.) makes precisely this point, exploring how the "cultural distance" between peacekeepers and the local population shapes effectiveness, suggesting that they may recognize diversity is not the driving factor. Thus to reiterate, Bove and Ruggeri's (n.d.) latest paper and our contribution are similar in terms of moving away from diversity and considering how the composition of a PKO in relation to the local population matters, but our mechanisms and measures (see below) differ from theirs ("cultural distance").

After outlining our mechanisms, we describe our research design before presenting our findings. We find support for both mechanisms and overall show that when PKOs have more peacekeepers from geographically proximate countries they better protect civilians. Indeed, our results suggest that if just a quarter of a PKO's personnel came from geographically proximate countries then, all things being equal, it would go a long way to completely prevent civilians dying in civil conflicts.

Mechanisms

We expect that when more peacekeepers in a PKO are from states that are geographically proximate to the conflict, PKOs better protect civilians. We offer two mechanisms to explain this. First, proximate peacekeepers are more likely to have been exposed to the societal and cultural norms of the country, making them better placed to understand the divisive issues that often underpin disputes.[2] This can help peacekeepers collect intelligence and inform peacekeeping strategies (Smith, 1994: 180), deal with disputes, and prevent them escalating to violence, thus better protecting civilians. As Ruggeri et al. (2017: 164) wrote, peacekeepers must "address local conflict dynamics, highlight local grievances, and reassure citizens that national agreements provide sufficient local gains." Ultimately this means groups are less likely to keep fighting or take up arms again, meaning civilians are safer. Tomforde (2010: 450) previously alluded to this point, writing that a "lack of cultural knowledge can be substantially disruptive and contribute to the failure of a mission." For example, in May 2004 in the DRC, rebels took over the eastern city of Bakavu. One boy watched soldiers enter a neighbor's house and realized his neighbor was about to be raped. The boy found a Uruguayan peacekeeper and tried to explain what was happening. Eventually, the soldier intimated that he had understood and gave the boy some cookies. As Autesserre (2010: 41) writes, this was not just a language problem but an ingrained perception in a peacekeeper, whose nation is far from the DRC, about what Congolese children want. Proximate peacekeepers are more likely to understand the behavior of people in the host country, and be more effective at collecting local intelligence and using it to better protect civilians.

Second, peacekeepers from proximate states may be even more effective if they are particularly invested in stopping conflict to prevent it diffusing into their own country. This is related to but distinct from an argument about deployment motivations (e.g. Binder, 2015; Bove and Elia, 2011; Uzonyi, 2015). Proximate countries may be motivated to deploy more peacekeepers to prevent conflict diffusion, but we argue that when proximate countries deploy peacekeepers due to this fear, these peacekeepers are more effective at protecting civilians. These peacekeepers may be more effective at various tasks, including securing borders, but to stop conflict diffusing in the long-term, they are invested in stopping conflict to reduce instability in their own country via side effects such as refugee inflows (Salehyan and Gleditsch, 2006). Indeed, peacekeepers that are willing to put themselves between civilians and rebel factions can separate combatants and better protect civilians (Hultman et al., 2013: 877). Commanders of peacekeepers from geographically proximate countries are likely to be more willing to put them on the frontline in harm's way to prevent conflict diffusion. Thus, if commanders from proximate countries are more willing to use their troops to separate combatants, this will help further protect civilians.

The association between the percentage of peacekeepers from proximate (in this case contiguous) countries and civilian deaths is illustrated by the United Nations Observer Mission in Angola (MONUA). Figure 1 shows that when the percentage of MONUA's peacekeepers from contiguous countries decreases the number of civilian deaths increases.

One might counter that peacekeepers from proximate countries could exacerbate conflict. If the proximate country has good relations with the government then their peacekeepers may antagonize rebels and exacerbate violence. We believe, however, that this mistakenly views the likely effectiveness of peacekeepers through the lens of government-to-government relations. Our argument is based on the notion that peacekeeping is often a local phenomenon, where gathering intelligence and engaging in positive community relations are important tasks. Government-to-government relations may or may not filter down to micro relationships between peacekeepers and civilians but we believe our mechanisms take precedence. Peacekeepers are more likely to consistently gain advantages in dealing with disputes in a host country by coming from a proximate country, even if it is as simple as understanding cultural and

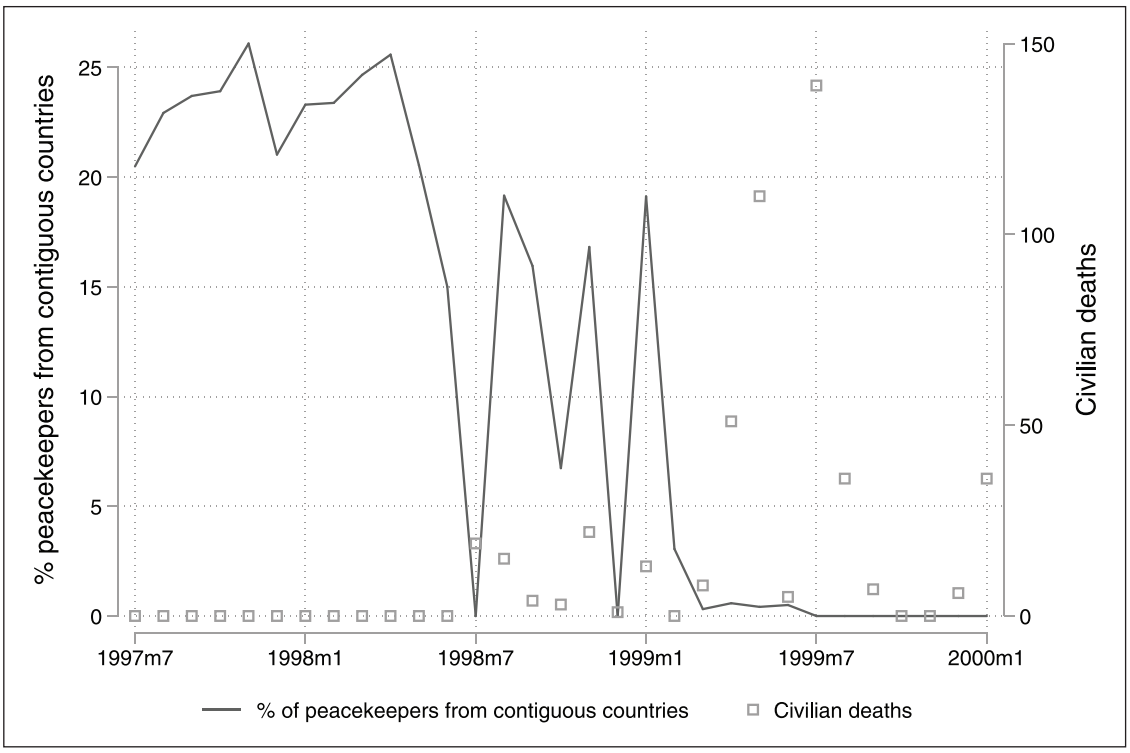

Figure 1. Peacekeepers from contiguous countries and civilian deaths in the United Nations Observer Mission in Angola, 1997–2000.

societal norms in the host country. Additionally, peacekeepers operate behind the scenes. Having insights into the host country's society can be crucial when working at a base and interpreting intelligence. Overall, we believe the benefits of being a peacekeeper from a proximate country outweigh potential risks. Therefore, we posit the following hypothesis:

> $\boldsymbol{H}_1$: *PKOs with more peacekeepers from geographically proximate countries are more effective at protecting civilians.*

Research Design

We test our argument using data on United Nations (UN) PKOs in African civil conflicts between 1991 and 2008.[3] We use this spatial-temporal domain for two reasons. First, as well as testing the above mechanisms, we also re-examine Bove and Ruggeri's (2015) finding that more diverse PKOs better protect civilians. This spatial-temporal domain is the same dataset that Bove and Ruggeri (2015) used, thus enabling clear comparison with their results.[4] Second, more UN PKOs have occurred in Africa than any other continent while, as of April 2018, more than half of ongoing UN PKOs are in Africa. We believe our mechanisms occur globally, but if readers have concerns about broader applicability of our findings, the results still have important implications given the volume of UN peacekeeping in Africa.

In the next section we provide results from several models to assess the robustness of our findings and concerns with previous results related to the diversity of PKOs. However, we present here a preferred specification that we contend is the methodologically appropriate specification. We examine the following model

$$\begin{aligned} Civilian\ deaths_{it} = f(&Proximate\ peacekeepers_{it-1};\\ &Conflict\ diffusion\ peacekeepers_{it-1};\\ &Proximate\ peacekeepers_{it-1} \times\\ &Conflict\ diffusion\ peacekeepers_{it-1};\\ &Conflict_{it}; PKO_{it-1}; Civilian\ deaths_{it-1}), \end{aligned}$$

where i and t refer to conflict i in month t. In line with Hultman et al. (2013) and Bove and Ruggeri (2015), the dependent variable (*Civilian deaths*) is the number of civilians killed each month. The *Conflict* variables are the number of monthly battle-related deaths lagged by one month; the monthly conflict duration; whether the sides are fighting over territory or government control; and the natural log of the host-country population. We deviate from Hultman et al. (2013) and Bove and Ruggeri (2015), by also including *Neighbors*, which is a count of a country's contiguous neighbors (separated by a land or river border) (Correlates of War Project, 2017). In examining the effect of proximate peacekeepers, this accounts for it being a function of a country's number of neighbors. The *PKO* variables are monthly lags of the number of troops, police,

and military observers divided by 1000. We also include a dummy variable that accounts for whether civilians were killed in the prior month. Data for the above variables come from Hultman et al. (2013).

We then include indicators to capture our individual mechanisms. The first mechanism concerns proximate peacekeepers understanding societal and cultural norms in the host country. Although it is not ideal, similar to Bove and Ruggeri (n.d) using contiguity to capture "cultural proximity," we use *Proximate peacekeepers*$_{t-1}$, which is a monthly lag of the percent of peacekeepers (Kathman, 2013) provided by contiguous countries (separated by a land or river border) (Correlates of War Project, 2017). The second mechanism concerns proximate peacekeepers being particularly effective when they are more invested to stop conflict diffusion. This may be true for all proximate countries, but Salehyan and Gleditsch (2006: 338) show that refugee inflows particularly motivate countries to stop conflict diffusing. *Conflict diffusion peacekeepers*$_{t-1}$ is a monthly lag, divided by 1000, of the level of refugees flowing from the host to the donor (United Nations High Commissioner on Refugees (UNHRC), 2012), weighted by the proportion of peacekeepers deployed by each donor (Kathman, 2013).

Additionally, we do not include these variables in our core specification, but we also estimate models with Bove and Ruggeri's (2015: 689) indicators of the PKO's *Diversity: Fractionalization*, which uses a variation of the Herfindahl-Hirschmann index to capture "the probability that randomly selected individuals in society will belong to different groups;" *Polarization*, which captures "how far the distribution of the group is from a bipolar distribution;" and *Number of countries*.[5]

With our count dependent variable of *Civilian deaths*, we use a negative binomial regression model with heteroskedastic-robust standard errors. This type of count model is more appropriate than a Poisson as it accounts for over-dispersion in *Civilian deaths*,[6] and is also more appropriate than a linear regression model, which can result in "inefficient, inconsistent, and biased estimates" (Long, 1997: 217). We use heteroskedastic-robust standard errors as the variance of the error term is not constant (Breusch-Pagan, 1979; Cook-Weisberg, 1983). Finally, a Wooldridge (2002) test indicates serial correlation is not a concern and a Breusch and Pagan (1980) Lagrange multiplier test suggests there is no evidence of unit-specific effects.[7]

Results

Our point of departure is Bove and Ruggeri's (2015) study on how the diversity of UN PKOs affects civilian deaths. We first replicate their results (see Table 1). There is no main model in Bove and Ruggeri's paper; we use Model 5 in their Table 1 (2015: 693) as it uses all three *Diversity* variables and the full sample of UN PKOs in Africa between 1991 and 2008. Model 1 in Table 1 successfully replicates their findings. Neither *Fractionalization* nor *Number of countries* are significant while *Polarization* is negative and significant, albeit only at 90% confidence.

Hultman et al. (2013) use a negative binomial regression model with standard errors clustered by conflict to account for heteroskedasticity and serial correlation. Yet, with a sub-sample of the same dataset, Bove and Ruggeri (2015) use a fixed effects negative binomial regression model. However, we do not find evidence to justify this econometric specification. When re-examining Bove and Ruggeri's (2015) model, a Breusch and Pagan (1980) Lagrange multiplier test for unit-specific effects does not provide any evidence to suggest a "cure" is necessary (Beck and Katz, 2001: 492). In other words, unobserved, time-constant confounders are not unduly influencing the model's estimates. Additionally, Guimarães (2008: 64) shows that fixed effects negative binomial models do not account for fixed effects unless there is a very "specific functional relation between the individual fixed effect and the individual overdispersion parameter." It is not clear that this is the case in Bove and Ruggeri's (2015) model. Additionally, the variance of the error term is not constant (Breusch-Pagan, 1979; Cook-Weisberg, 1983) and the errors are correlated across time periods (Wooldridge, 2002).

We therefore re-estimate Bove and Ruggeri's (2015) model with a negative binomial regression model with robust standard errors clustered by conflict but without fixed effects (Model 2 in Table 1).[8] Now, the direction of the coefficient for *Polarization* changes and is significant at 90% confidence. *Fractionalization* and *Number of countries* remain insignificant. This does not *disprove* Bove and Ruggeri's (2015) findings; no statistical test can do that. It does, however, raise doubts about the effect of *Polarization*, whose negative effect was the consistent finding in Bove and Ruggeri's (2015) paper.

Additionally, with this alternative specification, the coefficients for *Police*$_{t-1}$ and *Military observers*$_{t-1}$ increase, closer to Hultman et al.'s (2013: 884) results, while *Population* and the lag of *Civilian deaths* are no longer significant. The inconsistent results from these variables also match Hultman et al.'s (2013: 884) findings.

We then test our mechanisms. In Model 3 we find support for our first mechanism. As the percentage of proximate peacekeepers—who we argue better understand the social and cultural norms of the host country—increase, fewer civilians die. The non-linear nature of the negative binomial regression model means that the model's substantive effects are better understood through quantities of interest. We examine predicted values of *Civilian deaths* by increasing the percent of peacekeepers from proximate countries while holding all other variables at their mean (continuous) or mode (binary). We use *Clarify* (King et al., 2000) to run 1000 simulations and draw predicted values

Table 1. Effects of the composition of United Nations peacekeeping operations on civilian deaths in civil war, Africa, 1991–2008.

	Model 1	Model 2	Model 3	Model 4	Model 5
Proximate peacekeepers$_{t-1}$			−.14**	−.21**	−.20**
			(.04)	(.07)	(.07)
Conflict diffusion peacekeepers$_{t-1}$				.28*	.15*
				(.13)	(.07)
Proximate peacekeepers$_{t-1}$ ×				−.02*	−.01*
Conflict diffusion peacekeepers$_{t-1}$				(.01)	(.00)
Fractionalization	.03	−1.56			−.94
	(.55)	(3.22)			(1.32)
Polarization	−.94†	3.20†			4.19**
	(.53)	(1.90)			(1.58)
Number of countries	−.02	−.07			−.13*
	(.01)	(.17)			(.05)
Troops$_{t-1}$	−.12**	−.43**	−.48***	−.69***	−.53***
	(.04)	(.14)	(.08)	(.18)	(.08)
Police$_{t-1}$	.29	−8.93	−4.19***	−1.75	−2.62*
	(.52)	(6.13)	(1.16)	(1.64)	(1.02)
Military observers$_{t-1}$	1.91**	20.79***	16.96***	18.38***	17.10***
	(.71)	(4.54)	(2.38)	(2.99)	(2.76)
Battle deaths$_{t-1}$	.00*	.00**	.01**	.01	.01**
	(.00)	(.00)	(.00)	(.00)	(.00)
Conflict duration	.00**	−.00	−.00	−.00	.01
	(.00)	(.01)	(.01)	(.01)	(.01)
Population	.72***	1.36	−1.10	−1.25*	−.92
	(.13)	(1.55)	(.56)	(.55)	(.61)
Government conflict	.72*	2.26			
	(.34)	(5.07)			
Civilian deaths$_{t-1}$	1.19***	−.11	.81	.74	−.09
	(.16)	(.55)	(.54)	(.55)	(.48)
Neighbors			.22†	.30	.52**
			(.12)	(.12)	(.17)
Constant	−11.11***	−14.90	11.51*	12.41**	8.01
	(1.63)	(22.62)	(.4.81)	(4.66)	(5.39)
α		12.30***	11.53***	11.29***	10.24***
		(2.80)	(1.23)	(1.17)	(1.10)
N	577	748	520	520	455

Notes: Unit of analysis is conflict-month when peacekeeping operations occurred.
Model 1 is estimated with fixed effects and robust standard errors clustered by conflict; Model 2 is estimated with robust standard errors clustered by conflict; all other models estimated with robust standard errors.
Government conflict omitted from Models 3, 4, and 5 due to perfect collinearity.
***Significant at $p < .001$; **$p < .01$; *$p < .05$; †$p < .10$.

from a multivariate normal distribution based on Model 3's estimates and variance-covariance to approximate the standard errors for the quantities of interest. The effect is displayed in Figure 2.

When there are no peacekeepers from proximate countries there are expected to be almost 14 civilian deaths every month. The figure is *relatively* low because there are already average numbers of troops and police. Then, having only 6% of peacekeepers from contiguous countries means the predicted number of civilian deaths more than halves. Finally, although this predicted effect is dependent on the values of the other coefficients, having just over a quarter (26%) of peacekeepers from contiguous countries—essentially the maximum value in the sample—means that violence towards civilians is almost completely mitigated.

In Model 4 we test our second mechanism: whether proximate peacekeepers are even more effective when they come from countries concerned about conflict diffusing. We interact *Proximate peacekeepers*$_{t-1}$ with the monthly lag, divided by 1000, of the level of refugees flowing from the host to the donor (UNHCR, 2012), weighted by the proportion of peacekeepers deployed by each donor (Kathman, 2013). *Proximate peacekeepers*$_{t-1}$ remains negative and

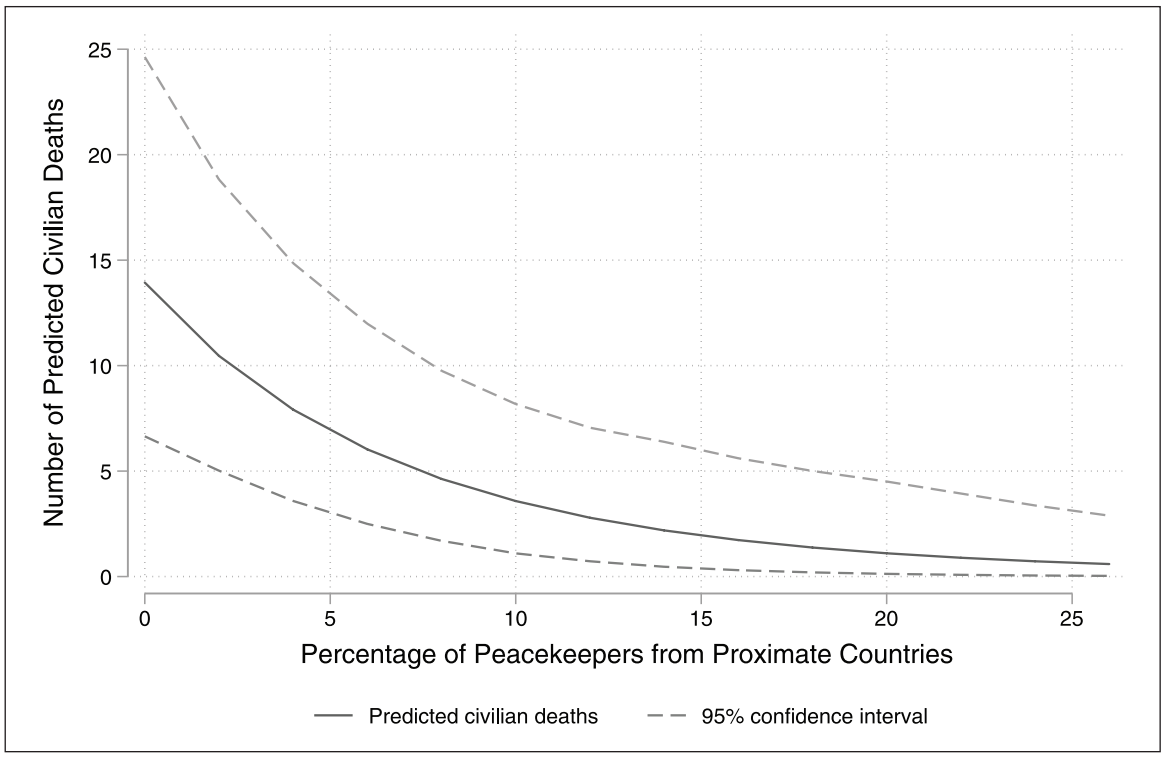

Figure 2. The predicted number of civilian deaths as the percentage of peacekeepers from proximate countries increases.
Note: Figure plotted after estimating Model 3.

significant while the significant interaction term suggests there is support for our second mechanism. Again, this is more easily seen visually. Figure 3 shows that proximate peacekeepers have an even larger negative effect on *Civilian deaths* as conflict diffusion becomes a greater concern for peacekeepers.

Finally, in Model 5 we show our findings are robust to including Bove and Ruggeri's (2015) *Diversity* variables. The *Diversity* variables continue to perform largely contrary to expectations: *Fractionalization* remains insignificant, while *Polarization* is significant at 99% but positive, contrary to Bove and Ruggeri's (2015: 693–695) findings. *Number of countries* is negative and significant as Bove and Ruggeri expect. Of the control variables, those that had a consistent statistically significant effect in Hultman et al.'s (2013: 884) models perform similarly: increasing troops or police generally leads to a decrease in *Civilian deaths*, while increasing the number of military observers has the opposite effect.

We conduct a number of sensitivity and robustness tests to further examine our findings. After estimating Model 4, we calculate variance inflation factors for each variable and find multicollinearity does not affect the significance of our results. We also confirm that influential observations are not driving the results. Additionally, we examine our mechanisms with conflict-monthly fixed effects with and without Bove and Ruggeri's (2015) *Diversity* variables. The effect of *Proximate peacekeepers*$_{t-1}$ is robust to fixed effects with or without the *Diversity* variables, but the findings are not significant once the interaction term is included. We are not overly concerned by this though, because, as discussed earlier, fixed effects negative binomial models are often problematic (Guimarães, 2008). Finally, we show the effect of proximate peacekeepers is robust to accounting for the percentage of peacekeepers from Africa as well as checking temporal dependence is not a concern. Full details of these tests are in the Online Appendices.

Conclusion

Overall, we advise caution in emphasizing the diversity of a PKO and suggest international organizations recruit peacekeepers from countries near to conflicts. Peacekeepers who understand the country's societal and cultural norms are better at protecting civilians. This effect is enhanced when peacekeepers are more invested in preventing conflict diffusion. There is clear scope for improvement among UN PKOs. Figure 4 shows that for almost three-quarters of the conflict-monthly observations in the sample used for Model 3, less than 5% of the PKO came from proximate countries. All else being equal, Figure 2 suggests over six civilians will die every month in each of those missions.

There are many other characteristics of PKOs worthy of future attention. For example, it seems likely that

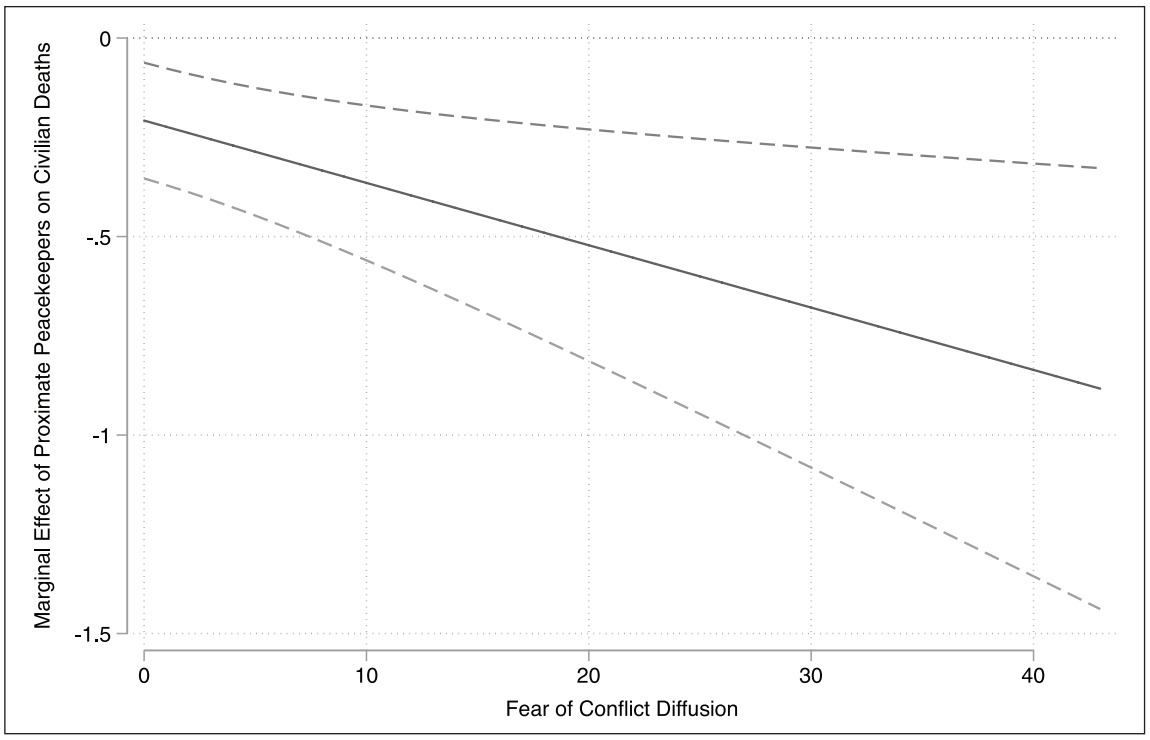

Figure 3. The effect of peacekeepers from proximate countries on civilian deaths as the number of peacekeepers from countries fearing conflict diffusion increases.
Note: Figure plotted after estimating Model 4.

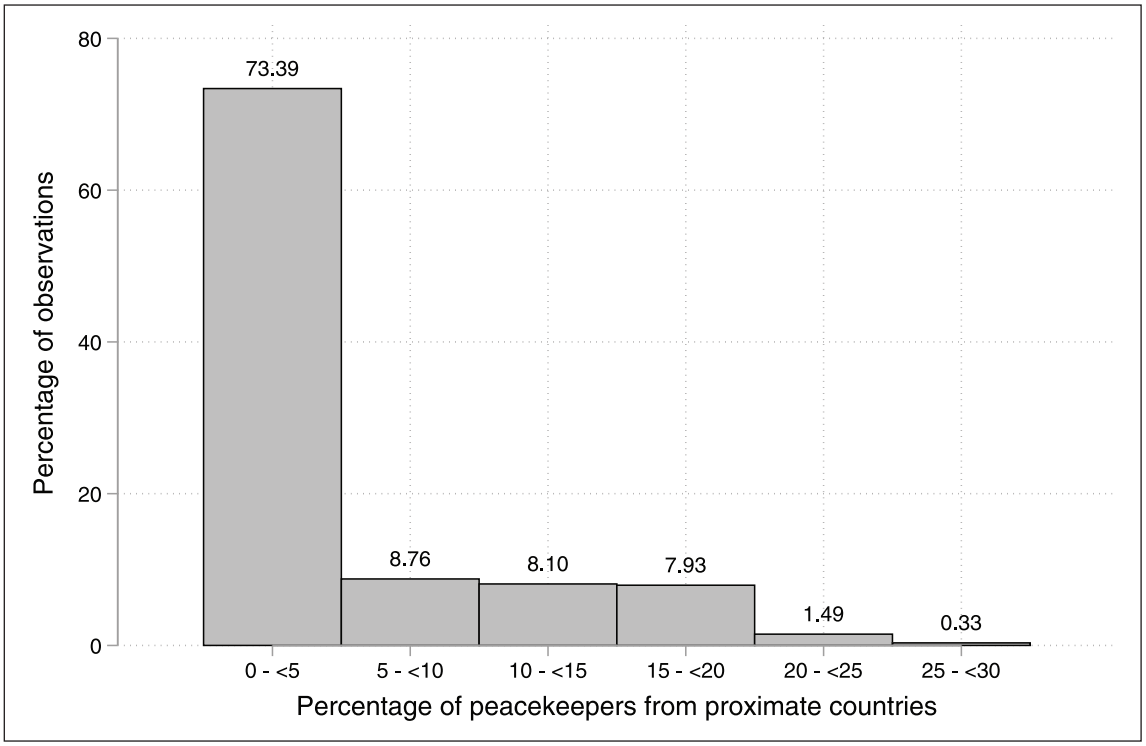

Figure 4. The Percentage of Proximate Peacekeepers Present in Each Conflict-Month.

whether contributors send soldiers experienced in peacekeeping would matter. Our study was also restricted to the UN and Africa. Empirically, this reflects the main areas of post-Cold War peacekeeping but future studies should look towards other international organizations and other regions. In short, there are many PKO characteristics with important policy consequences that deserve further study.

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Notes

1. There are, of course, other indicators of PKO success but we focus on civilian deaths as the UN (2017) describes it as the ultimate goal of UN peacekeeping, and recent work that we engage with (Bove and Ruggeri, 2015) also uses it as an indicator of PKO effectiveness.
2. Bove and Ruggeri (n.d.) emphasize the "cultural distance" between peacekeepers and the local population—which includes sharing language—but we stress the need to understand local cultures and divisive cleavages, which can come from sources other than shared characteristics.
3. Similar to Bove and Ruggeri (2015), we use a sub-sample of Hultman et al.'s (2013) conflict-monthly dataset based on when PKOs were deployed.
4. Ideally, we would also test this globally using the International Peace Institute's "Providing for Peacekeeping" data, but space constraints preclude that here.
5. For a detailed discussion of how these variables are calculated see Bove and Ruggeri (2015: 688-689), and for a summary of how all variables are coded and their sources see the Online Appendices.
6. The variance of *Civilian deaths* is larger than the mean. When using a Poisson, this over-dispersion can result in inflated z-values (Long and Freese, 2014: 512). This choice is supported by the results in Table 1 as there is significant evidence of over-dispersion as shown by the significant coefficients for α.
7. We conduct robustness tests with conflict-monthly fixed effects; see the Online Appendices.
8. The sample increases from 577 to 748 because the fixed effects model omits observations without within-group variation on the dependent variable.

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