The Promise of Peacekeeping: Protecting Civilians in Civil Wars

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Abstract

Do peacekeepers protect civilians in civil conflict? Securing civilian safety is a key objective of contemporary peacekeeping missions, yet whether these efforts actually make a difference on the ground is an open question. This paper argues that because peacekeeping forces often need to maintain close ties with host governments, peacekeepers reduce civilian fatalities inflicted by rebels, but not those caused by governments. To test our claim, we overcome common problems of endogeneity and selection bias by using a novel natural experiment. Specifically, we leverage exogenous variation in which countries hold power in the United Nations Security Council to show that states that wield more power send more peacekeepers to their preferred locations, and that these peacekeepers in turn help to protect civilians from rebel factions. Using new data on the location of each conflict event, we also provide support for the mechanisms at work.

Keywords: civil war, conflict, international institutions, United Nations, peacekeeping, natural experiment

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Civilian populations often bear the brunt of violence in civil wars, as targeting civilians is a common tactic that both rebel groups and government forces use. Since the end of the Cold War, efforts to protect civilians in conflict theaters have preoccupied government leaders and practitioners around the world. Yet scholars and policy-makers remain divided on whether the international community's efforts to do so are effective, inconsequential, or even detrimental. While international interventions may lessen incentives to target civilians and provide barriers between civilians and combatants (Hultman, 2007), they may also cause opposed factions to step up civilian victimization due to changes in the balance of power (Wood, Kathman and Gent, 2012). Or, warring populations may believe that humanitarian intervention is biased towards those who inflict the most severe abuses, leading them to commit ever greater crimes (Ziemke, 2012). In tandem, policy-makers' views have also become divided, with many responding to calls for increased involvement with demands for cuts in the United Nations' peacekeeping forces amid high levels of civilian atrocities.[1]

We argue that these conflicting views are driven by theoretical issues along with pervasive and often intractable empirical difficulties. Theoretically, we adopt a more nuanced approach, demonstrating that the effect of intervention on civilian casualties depends on the relationship between combatants and peacekeepers. Specifically, we disaggregate civilian victims into those killed by the government and those killed by rebels, arguing that multilateral peace operations only have a positive effect on the latter group. Because peacekeepers rely on and are often required to collaborate with host governments, they are incentivized not to anger governments by interfering with their activities. By contrast, peacekeepers are much less constrained in condemning rebel groups and work actively to end their atrocities. By separating these two distinct causal processes, we provide a more complete account of peacekeepers' effects.[2]

Further, we flesh out the mechanisms by which peacekeepers prevent rebel-inflicted civilian

[1]See, e.g., "Darfur Peacekeeping Force to be Cut Back Amid Accusations of Incompetence." *The Guardian*. March 11, 2015; "Mission Creep in the Congo." *Huffington Post*. Sept. 21, 2010.

[2]Note that Hultman, Kathman and Shannon (2013) also disaggregates casualties, though they find that peacekeepers prevent civilian deaths by both government and rebel forces.

deaths by analyzing the strategic setting in which they operate. For instance, we further disaggregate the data to examine whether the effect is greater when peacekeepers are sent to combat or rear zones in the civil war country, since they perform different tasks in each area. We use this test to disentangle which set of mechanisms explains the impact of peacekeepers on the ground, finding that both sets of mechanisms are operative in curtailing rebel-inflicted deaths. We also conclude that peacekeepers are ineffective at preventing government-inflicted deaths in combat and rear areas. Given our theoretical mechanisms, we further expect greater numbers of peacekeepers to have a stronger impact on rebel atrocities. Using newly collected data, we find strong evidence to support our claim.

Moreover, in addition to offering a new theoretical perspective, we identify the causal mechanisms at work through the use of a unique natural experiment. A central issue that researchers must contend with is that international intervention is not randomly assigned; that is, the international community becomes involved in states' domestic affairs for reasons that are not independent of the treatment of civilians in those states. Many motivations for these attempts to influence other states are likely unobservable, which can lead to bias in the estimation strategy. States may require international assistance for a variety of reasons that make it appear that the international community's involvement does not protect civilians, when it is actually very effective, or vice versa. For example, states often send peacekeepers to help civilian populations in regions with the most casualties (Gilligan and Stedman, 2003). It may appear that these policies are ineffective, or lead to more deaths, when in reality these states would have higher levels of civilian abuse had they not received peacekeepers.

The central problem is that scholars do not observe what would have happened had the international community not intervened. Ideally, studies would compare the result of an intervention in a particular state to the outcome in the same state had no intervention occurred. This is the fundamental problem of causal inference–one never can observe the counterfactual outcome. The treatment of civilians in a particular state cannot simply be compared before and after an international intervention because of the problems mentioned above. Randomized experiments are often

an effective way to deal with this problem, because randomly assigning the treatment ensures that other factors are not driving the results. However, large-scale experiments are often impossible in international relations, since governments and international institutions are typically reluctant to randomly assign these interventions.

However, international institutions often offer the next best approach: natural experiments. Because international institutions operate with pre-determined rules and procedures, they often provide "as-if random" treatments. Through careful research into the inner workings of the UN Security Council, a previous paper written by one of the co-authors identifies a new natural experiment using two exogenous rotation rules to demonstrate that, contrary to the conventional wisdom about great powers' dominance over this institution, great powers exercise strategic restraint around the Council's horseshoe table, thus enabling minor powers to exercise substantial influence over its decisions whenever they rotate into a privileged position inside the institution (Mikulaschek, 2015). This natural experiment provides a novel identification strategy for disentangling the causal effects of peacekeeping on the protection of civilians. We are thus able to conduct an in-depth analysis of an intervention governed by specific institutional rules to obtain well-identified estimates of the effects.[3] We overcome the persistent empirical concerns of selection bias and endogeneity by examining as-if random components of these interventions to show that peacekeepers have been successful in protecting civilian populations from rebel groups–but not from governments–over time in countries around the world.[4]

This article makes several contributions. First, we use a novel research design and dataset to help adjudicate the scholarly debate over the effect of peacekeepers on civilian casualties, which gets to the heart of questions surrounding the effects of international institutions. Scholars have recognized the need for a method to overcome the problem of endogenous intervention, but have been largely unable to identify a plausible source of exogeneity. Though scholars have developed

[3]For other work using variation in the rotation of a presidency for causal identification, see Carnegie and Marinov (2012).

[4]For overviews of empirical difficulties in the study of international relations and the need for better identification strategies see Hyde (2015); Findley, Nielson and Sharman (2013).

innovative methodological approaches that control for a variety of factors that determine where peacekeepers are deployed, they have not found a random, or quasi-random source of variation. Fortna (2004, 115) notes, "Two-stage analysis, or instrumental variable analysis, is often used to evaluate the effect of a variable, in this case peacekeepers, that is itself affected by (or endogenous to) other variables in the model. Unfortunately, it is not possible here...Most of the variables that shape whether or not peacekeepers are deployed are likely to be directly related to the ease or difficulty of maintaining peace...These variables [are] unsuitable as instruments for two-stage analysis." Sambanis (2008, 19) concurs that valid instruments "are hard to come by in cross-country studies" so "it was not possible to find good instrumental variables" for an analysis of the effects of UN peace operations.[5] However, we present two plausible instrumental variables, allowing us to overcome these concerns to move the debate forward.

Second, this article's positive findings regarding the effects of peacekeeping on rebels' treatment of civilians can help to inform policy decisions about international interventions in civil conflicts. While many critics condemn these activities as being unproductive or detrimental to the civilians they intend to help, we show that on average, peacekeepers have had a strong, positive effect on the protection of civilians. At the same time, we find that the effect of peacekeepers on civilian protection is not uniform: while effective at saving civilians from victimization by rebels, larger UN peace operations do not significantly reduce the number of civilians killed by the government, on whose consent and collaboration the UN peace operation ultimately depends. Our findings thus suggest that rather than scale these activities back, they may be productively implemented in the future in conflict settings, particularly in response to rebel-inflicted casualties.

Third, our approach of identifying quasi-random variation in how international organizations function can be applied to other, diverse settings. For instance, many prominent institutions in-

[5]Note that other methods including matching techniques (Gilligan and Sergenti, 2008), seemingly unrelated probit (Melander, 2009), and semi parametric recursive bivariate probit (Bradshaw et al., 2015) can ameliorate certain concerns, but do not resolve many problems of endogeneity and selection bias and often introduce strong assumptions that may be difficult to substantively motivate. See Sekhon and Titiunik (2012*a*). Others have also noted the endogeneity of peacekeeping missions; for example, King and Zeng (2007) argue that Doyle and Sambanis (2006)'s findings of a positive relationship between peacekeepers and peace are model dependent, though Sambanis and Doyle (2007) challenge this critique.

cluding ASEAN, APEC, the EU, the UN General Assembly, and CARICOM feature exogenous leadership rotation. Other domestic institutions also incorporate power-sharing agreements which mandate rotation in the holding of leadership positions among various groups, such as the geographic rotation of Bosnia and Herzegovina's presidency and of the U.S. Federal Reserve's Federal Open Market Committee. Exploiting these institutional design features could lead to a multitude of interesting and well-identified studies.

Fourth, our findings contribute to debates over who holds power in international organizations and what these effects may be. We show that states that wield power in the Security Council use this power to try to benefit their own national interests. Further, we demonstrate that their efforts strongly impact important outcomes. Because states in leadership positions pursue their interests potentially at the expense of other states, these results raise questions about accountability within international organizations.

The article proceeds as follows. First, we discuss the theoretical underpinnings of our argument, detailing the mechanisms by which peacekeeping may affect the protection of civilians. We then explain the design of our natural experiment, in which we leverage exogenous variation in which countries hold power in the United Nations Security Council. We use this variation to show that states that wield more power send more peacekeepers to their preferred locations, and that these peacekeepers in turn have a positive effect on rebels' treatment of civilians, whereas they do not significantly reduce the number of fatalities at the hands of the government. Using our detailed knowledge of the nature of the interventions along with a new dataset, we also provide support for the mechanism at work by distinguishing between civilian casualties in combat areas and those in rear areas.

Peacekeeping and the Protection of Civilians

Targeting civilians has become a common tactic in civil conflicts by both government forces and rebel groups. Ambushing civilian convoys, shelling sites populated by civilians, ethnic cleansing,

and other atrocities occur frequently during civil wars. Indeed, from the end of the Cold War to 2004, 572,767 people were killed in one-sided violence (Eck and Hultman, 2007).[6] As a result, the chief goal of contemporary UN peace operations is typically to protect civilians; for example, it is the top priority for the largest and most expensive UN peacekeeping mission in history, which is deployed in the Democratic Republic of the Congo (United Nations, 2008; Doss, 2010).[7]

Why do warring factions victimize civilians? Rebel groups do so for a variety of reasons. Weak rebel groups with collective action problems often cannot secure the loyalty of civilians through benefit provision, and thus turn to violence instead (Wood, 2010). Insurgent violence against civilians may also depend on informational asymmetries (Kalyvas, 2006), rebels' original resource endowment (Weinstein, 2007), and pre-war cleavages (Balcells, 2010). Alternatively, rebels may turn to violence against civilians when they lose battles (Hultman, 2007), obtain additional resources (Hoffman, 2004), desire a more favorable bargaining position vis-a-vis the government (Lake, 2002), want to foster ethnic cohesion (Byman, 1998), or when they rise up due to urban issues that cannot be addressed in major towns (Mkandawire, 2002). Further, civilian abuse may depend on a variety of internal characteristics of the warring faction, social ties between the communities and rebels, the degree of control and contestation in a given area, and levels of poverty (Humphreys and Weinstein, 2006).

Governments, too, often target civilians, particularly when they believe that rebels enjoy broad support from the civilian population. Governments kill civilians to punish them, inducing them to withhold support from the rebels (Valentino, Huth and Balch-Lindsay, 2004; Valentino, 2004). Governments may also harm civilians to minimize their own military's fatalities or they may remove civilians from contested land in order to annex it (Downes, 2011). Finally, governments may do so to supplement their resources or to lessen the rebels' abilities to hide among civilians for

[6] 72,767 people were killed if Rwanda in 1994 is excluded (Eck and Hultman, 2007).

[7] The protection of civilians was defined in this mission as "all activities aimed at ensuring the safety and physical integrity of civilian populations, particularly children, women, and other vulnerable groups, including IDPs; preventing the perpetration of war crimes and other deliberated acts of violence against civilians; securing humanitarian access; and ensuring full respect for the rights of the individual, in accordance with relevant national and international bodies of law, i.e. human rights law and international humanitarian law" (De Coning, Lotze and Stensland, 2011, 6).

support (Azam and Hoeffler, 2002).

However, despite the prevalence of civilian deaths at the hands of both rebels and the government, few scholars have focused on the impact of peacekeeping on civilian protection explicitly. This is surprising since many have recognized that preventing the resumption of war is a low bar for success, and that civilian victimization impacts the quality of peace and is thus a crucial outcome to consider (Kreps and Wallace, 2009). Yet those that have looked explicitly at this relationship find divergent results. Some claim that peacekeeping missions can reduce harm to civilians, particularly when the operations contain large numbers of police and military troops (Hultman, Kathman and Shannon, 2013), when they are specifically "traditional" or "enforcement" UN missions (Kreps and Wallace, 2009), when the Security Council explicitly considers the nature of the threat to civilians (Holt, Taylor and Kelly, 2009), when they directly confront the perpetrator or assist the target of the killings (Krain, 2005), or when the effects of neutral interventions are looked at in the long-term (Kathman and Wood, 2011). Similarly, the broader literature tends to reach a positive conclusion of the effect of peacekeeping on restoring and maintaining the peace in general (Doyle and Sambanis, 2006; Fortna, 2004; Gilligan and Sergenti, 2008).

Others, however, argue that peacekeepers are ineffective or even increase harm to civilians. A recent study commissioned by the UN Department of Peacekeeping Operations warns that "the 'chain' of events that lead from the Security Council to the field for delivering protection to civilians in peacekeeping missions is broken" (Holt, Taylor and Kelly, 2009, 214). Rebels may believe that peacekeepers tend to assist those who commit the most abhorrent violence, causing rebels to increase this behavior (Hoffman, 2004).[8] Or, intervention can alter the balance of power, leading the losing side to step up violence against civilians (Ziemke, 2012). Further, peacekeepers may lack a mandate to protect civilians, potentially reducing their effectiveness in this area (Lamp and Trif, 2009).

We argue that these assessments remain inconclusive largely due to empirical issues. Once

[8]In a report to the UN Security Council, the UN Secretary-General acknowledges that rebels often believe that the United Nations, as an intergovernmental forum, favor the government's side in a civil war (United Nations, 2009).

these are addressed using our novel identification strategy, we expect to find that UN peacekeeping operations protect civilians from rebels. In addition, we argue that their specific impact varies depending on the strategic setting, which includes the size of the operation and the nature of the area to which UNPOs are deployed.

Our primary hypothesis is that UNPOs are only effective at reducing civilian fatalities that rebels cause, for two reasons. First, UNPOs de facto rely on the consent of the host country's government. For instance, cases where the host government forced a UNPO out include Burundi in 2006 and Chad and the Central African Republic in 2010 (United Nations, 2006, 2010). In another case, Sudan blocked the expansion of the UN peace operation into the Darfur region in 2006 (International Crisis Group, 2006).[9] When the government victimizes civilians, it often does so to attain a military advantage in an ongoing armed conflict, and it is typically loath to allow a UNPO to prevent it from attaining this advantage. Knowing that the host government can force the UNPO out, the UNPO adopts a cautious - and ultimately ineffective - approach to protecting civilians from government forces. Thus, "these missions may have to choose, at times, between maintaining consent and thus being able to continue to invest in building an environment conducive to protection, and acting forcefully" (De Coning, Lotze and Stensland, 2011).

Second, an increasing number of UNPOs is mandated to actively collaborate with the host country's military and police by training and mentoring these forces (United Nations, 2000). To perform this task, UNPOs need to maintain collaborative relationships with the armed forces and police, which gives peacekeepers an incentive not to respond harshly to civilian victimization by those same security forces (Chappuis and Gorur, 2015). For instance, an extreme case in this regard is the Democratic Republic of the Congo, where UN blue helmets fought rebel forces alongside the government, even though the latter has often been accused of abusing civilians (see, e.g., Sheeran and Case 2014).

In effect, peacekeepers have little effect on government-inflicted deaths. Consider, for example,

[9]Note that in each of these cases, the UN mission had a mandate under Chapter VII of the UN Charter, meaning that it did not depend on the government's consent de jure, even though it did de facto.

the effect of UN peacekeepers on civilian deaths in the Democratic Republic of the Congo. In a recent account of his experience as the head of UN peacekeeping, Jean-Marie Guéhenno recalls that the peacekeepers "often turned a blind eye on violations committed by the Kinshasa side." He further notes that the president of the DRC "expected the mission to shore up his authority against the rebels" and that the head of the peace operation "had made a priority of helping the reunification of the country under Kinshasa's terms and did not want to damage his relations with the president" (Guehenno, 2015, 122). We thus do not expect to find an effect of UNPOs on civilian fatalities inflicted by the government.

We also test several additional observable implications of this theory. First, we argue that if UNPOs are effective at reducing rebel-caused civilian fatalities, an increase in the size of UNPOs should further reduce these civilian fatalities. Larger missions have greater resources to expend in this endeavor, so should be more able to assist civilians through activities such as disarming and separating combatants (Hultman, Kathman and Shannon, 2013; De Coning, Lotze and Stensland, 2011). This expectation is in line with previous work, which argues that more peacekeepers are better able to deploy where needed and can signal to combatants that the UN is determined to stop the conflict. Larger UNPOs are highly visible and thus incur greater costs if they fail in their mission or are recalled. Such demonstrations of resolve can encourage belligerents to stop fighting and thus cease targeting civilians (Hultman, Kathman and Shannon, 2013). However, unlike previous research, we expect this effect only to pertain to deaths caused by rebels; since we believe that the factors leading UNPOs to be ineffectual at protecting civilians from the government cannot be resolved by simply increasing the number of UNPOs. Regardless of the number of UNPOs, they still must depend on and collaborate with the host government.

Further, we investigate the mechanisms through which UNPOs decrease civilian deaths at the hands of rebels by examining whether the effect is more pronounced in conflict or rear areas. Combat areas are those regions of the civil-war country where armed clashes between warring parties have occurred. In these areas, UNPOs should reduce the targeting of civilians primarily through three activities. First, UNPOs separate the warring factions, interpositioning themselves between

them and thus creating a physical barrier. This has been the traditional task of UNPOs since the 1940s (Doyle and Sambanis, 2000; Fortna, 2008*c*; Hultman, Kathman and Shannon, 2013). Second, they lower the risk that violent encounters between the warring factions accidentally trigger large-scale fighting by facilitating communication and building trust between the parties (Fortna, 2008*c*). Third, they enable parties to strengthen their territorial control along a stable cease-fire line, making each faction more confident that it will profit in the future from today's restraint by not abusing civilians for short-term benefit. This last argument builds on Humphreys and Weinstein (2006)'s insight that the stronger a group's control over territory, the more confident its leaders can be that they will reap the long-term benefit that results from foregoing the short-term benefit from abusing civilians. It also follows from Kalyvas (2006)'s argument that the establishment of uncontested sovereignty in a given area of the civil-war theater diminishes the motivation to resort to violence against civilians.

By contrast, in rear areas, UNPOs have two primary ways to exert an impact. First, they patrol population centers and locations that are vulnerable to predatory attacks on civilians, such as markets (Hultman, Kathman and Shannon, 2013). Second, UNPOs monitor the behavior of warring factions' local units and commanders and thus help faction leaders oversee their subordinates and rein them in when they engage in abusive behavior that benefits them personally while hurting the group's overall objectives. This argument extends from Humphreys and Weinstein (2006)'s claim that collective action problems occur within each group when actions that benefit individuals detract from the group's overarching goals. Since we argue that each of these mechanisms plays an important role, we expect to find a positive impact of UNPOs in both areas, though again only for deaths that occur due to rebel activities.

Exogenous Variation in Peacekeeping

In order to isolate the effect of peacekeeping on civilian protection, we must first identify a source of "as-if random" variation in peacekeeping deployment. We do so by exploiting two sources of

predetermined variation in which states hold power in the Security Council: the rotating presidency and the rotation of seats on the Council between different regions.

Established in 1945, the UN Security Council is tasked with the maintenance of international peace and security. The Council included eleven members at its founding, though that was subsequently expanded to fifteen in 1965. There are five permanent members: China, France, Russia, the United Kingdom, and the United States, and ten non-permanent members. Non-permanent members are elected to two year terms with no immediate reelection, staggered such that five new members are elected each year. Non-permanent seats are reserved for states in specific regions.[10] The Security Council decides on the establishment, termination, mandate, staff composition, and authorized personnel size of UN peace operations. Once it has established a peace operation, the Council regularly reviews the size and mandate of the mission. It can establish new UN peace operations or wind down existing ones at any moment.[11]

The institutional rules of the UN Security Council yield exogenous variation in two ways. First, exogenous variation exists in the composition of the set of non-permanent Council members. Three of the ten non-permanent seats are reserved for African states. Under a formula devised in the 1960s and observed without exception since the 1970s, a Central or North African state must rotate into one of these seats once every two years, and the second seat is alternately held for two years by an Eastern and Southern African state; the third seat is always held by a Western African state (Mikulaschek, 2015). These temporary members exert influence in many ways: they chair most sanctions committees and working groups, their votes are essential for unanimity (which is strongly desired, so that 90% of votes are unanimous), and a positive vote from a conflict region helps prevent the appearance of neocolonialism and makes combatants more apt to comply with the

[10]The permanent members each wield a veto over any non-procedural decision, while the non-permanent members have no such power.

[11]The only formal prerequisite for the establishment of a peace operation is that the Council considers the crisis which it seeks to address as a threat to international peace and security or views the continuation of this crisis as likely to endanger international peace and security. In practice, the establishment of a peace operation is often preceded by the conclusion of a cease-fire by the warring factions. Our identification strategy accounts for such endogeneity by assessing the effect of those UN peacekeepers who are deployed solely as a function of two exogenous rotation rules in the Council.

mission. Further, the fact that states that are not permanent Security Council members contribute 96% of peacekeepers provides additional leverage. Thus, the natural experiment consists in the rotation of two seats on the Council between four African regions, as they can use their sway to impact peacekeeping missions when they attain representation.

Second, the presidency of the Council rotates monthly among all Council members in alphabetical order of the members' English names. Thus, the selection of the state holding the presidency has no relationship to any sort of political considerations. The president's formal responsibilities include calling and presiding over meetings, preparing the Council's agenda and determining the order of votes on amendments, issuing Presidential Statements and press statements, communicating with UN member states and the UN Secretary-General on behalf of the Council, and overseeing crises (Bailey and Daws, 1998; Dedring, 2008). However, the president's discretion often exceeds her formal responsibilities (Bosco, 2009, 162, 228). The Council's president regularly consults all Council members, and she is often put in charge of finding compromise and maintaining consensus in the Council (Nicol, 1981). Three examples from the 1991 Gulf War illustrate the de facto influence of the Council's president. In January, Zaire refused to hold an emergency meeting Cuba requested on the humanitarian impact of the U.S.-led air strikes, even though it lacked the authority to do so (Pilger, 2002; Gharekhan, 2006, 22). In late February, Council members entrusted the outgoing and incoming presidents (Zimbabwe and Austria) with the task of conducting bilateral consultations with Council members about the terms of a resolution on the cessation of hostilities in Iraq (Freudenschuss, 1994, 499). While Council members finalized the draft resolution on the end of the 1991 Iraq war, Belgium actively dissuaded some of them from jeopardizing the negotiations by introducing partial drafts early in its Council presidency in April (Liegeois, 1993, 35).

Our approach should not be conflated with other empirical strategies that exploit variation in UNSC membership (Vreeland and Dreher, 2014; Vivalt, 2015). We do not require states' selection onto the Council to be quasi-random; we only need the presidency to change hands in an as-if random fashion, and/or for African regions to rotate onto the council in a quasi-random manner.

While other work has used interesting instrumental variables designs (Vivalt, 2015), the potential remains for the instruments to feature unobserved heterogeneity due to incomplete knowledge of the assignment process for the composition of the set of states with a seat on the Council (Sekhon and Titiunik, 2012*b*).[12]

Yet it is not enough to identify an exogenous power-sharing rule. We must also show that states use this power to affect peacekeeping missions. Why might these states use the Council to pursue their national interests, rather than using their own national resources? The UN became increasingly viewed as an effective tool to promote peace due to its ability to determine when the use of force is legitimate and to solve collective action problems between states. Peacekeeping is often seen as a more legitimate form of intervention by both the intervener's domestic population and the country that hosts the peacekeepers. While intervention by a single state is viewed as a form of neocolonialism, UN intervention does not carry the same stigma (Doyle and Sambanis, 2006). Further, great powers are able to promote peace without risking domestic casualties, especially in recent times. Indeed, the composition of peace-keepers has changed over time from citizens of developed countries to those of developing ones. In 1993, the states that contributed the most were the US, UK and France; but by 2003 were developing countries. Further, in 1993, only 14% of peacekeepers were not from the UN, but from 1996-2001 70% were. In addition, if a peacekeeping mission fails, blame can be shared. Rather than assume all of the blame for such a failure, a given country can deflect it to the entire UN.

However, while peacekeeping is politically expedient, it is also expensive. In 2015, the UN budget for these activities was $8.3 billion (United Nations, 2015*a*) and required 125,000 UN personnel (United Nations, 2015*b*). Further, just five states contribute 63% of the total budget, and 11 pay 82% (Stojek and Tir, 2014). As such, peacekeepers cannot deploy to all civil wars. What explains where they go? Several scholars find that peacekeepers are sent to hard cases with more casualties and longer lasting conflicts (Gilligan and Stedman, 2003; Fortna, 2008*a*). The more

[12]While Vreeland and Dreher (2014) limit the sample to African states, this does not fully resolve the selection concern since it does not exploit the quasi-random rotation between regions.

capacity is destroyed and the greater the number of people killed, the more international assistance is necessary to keep the peace (Doyle and Sambanis, 2006). Peacekeepers are not deployed to states with strong governments, large armies, and large economies, which are thought to be able to take care of themselves and which often resist intrusive multilateral missions (Fortna, 2008*a*; Gilligan and Stedman, 2003). Several studies agree that the deployment of UNPOs is not exclusively based on need: states generally use their material power and wealth to shape outcomes in the UN Security Council (Kuziemko and Werker, 2006; Vreeland and Dreher, 2014). Thus, Stojek and Tir (2014) find that states that trade more with the permanent members of the UN Security Council receive more UN peacekeepers than others, and the interests of these five great powers also shape the UN Secretariat's degree of autonomy in carrying out the mandates that the UN Security Council adopts (Allen and Yuen, 2014).

African countries that serve on the UN Security Council prefer larger UN peace operations in countries in their own region that undergo civil conflict. This is because civil conflict in neighboring countries creates negative externalities in the form of refugee streams and arms proliferation, which can lead to conflict contagion and political instability (Gleditsch, 2002, 2014; Salehyan and Gleditsch, 2006; Buhaug and Gleditsch, 2008).[13] International peace operations have been found to reduce the risk of conflict contagion (Beardsley, 2011), and they have been shown to be successful at preventing conflict relapse (Doyle and Sambanis, 2006), thus mitigating negative externalities. African UN Security Council members consistently vote in favor of more peacekeeping in civil-war countries in their own region, and they often deplore that the Council did not send more blue helmets to nearby conflict theaters (Mikulaschek, 2015). When exogenous rotation rules allocate more influence to African Council members, they use this leverage to push for an increase in the number of UN blue helmets in states in their own regional neighborhood that experience civil conflict. Thus, more blue helmets are deployed in regions when those regions are represented

[13]For example, the conflict in Chad was exacerbated by the spillover violence from Sudan, and the civil war in the Democratic Republic of the Congo was fueled by the conflict and genocide in neighboring Rwanda. If some states receive fewer peacekeepers when their regions are represented, this would violate our monotonicity assumption. However, the presence of defiers simply means that we identify a weighted average treatment effect that is weighted towards those the instrument has a greater effect on.

on the UN Security Council and, even more so, when a state in that region holds the Council's presidency, than at other times. Therefore, we can use the two sources of exogenous variation in influence on the UN Security Council as instruments for the size of UNPOs.

Research Design

To estimate the effect of peacekeeping on the protection of civilians in civil conflict, we rely on a dataset consisting of monthly observations of UN peacekeepers during civil conflicts between 1989 and 2010. We code civil conflict using the conventional definition from Themnér and Wallensteen (2014) and a measure of battle-related deaths from Harbom, Havard and Havard (2009). We focus on the post-Cold War period because prior to the end of the Cold War, the Security Council undertook few peacekeeping missions–and just one in Africa–due to the rivalry between the two most powerful states (Kreps and Wallace, 2009).[14] After the Cold War, however, UN peacekeeping dramatically expanded in size, such that the UN is now actively engaged in peacemaking, peacekeeping, and post-conflict peacebuilding activities in the majority of civil conflicts in the world. We measure the number of UN peacekeeping personnel (troops, police, and military observers) using data from Mikulaschek (2015). We focus on UN missions with a military component as well as missions sent to ongoing conflicts, thus excluding special political missions and UN envoys and mediators.[15] On average, 1,036 blue helmets were deployed to civil-war countries in our sample (see Table 4 in the Appendix).

Our outcome variable, civilian protection, is measured with a monthly count of civilian deaths in civil conflicts and was compiled from the UCDP's Geo-referenced Event Dataset (GED v.1.5)

[14]Moreover, systematically collected data on the monthly number of civilian casualties in civil wars is not available for the Cold War era.

[15]Stojek and Tir (2014) and Fortna (2008*a*) similarly exclude missions that lack a military component. Eck and Hultman (2007, 237) state, "The vast majority of attacks on civilians do take place in countries plagued by armed conflict; we found that less than 1% of the total fatalities took place in countries which did not see armed conflict during the period." Echoing Gilligan and Sergenti (2008), Kreps and Wallace (2009, 27) also caution against pooling war and peacetime phases together since the "effects of PKOs should not necessarily be the same."

presented by Sundberg and Melander (2013).[16] In total, 124,159 civilians were killed in the course of civil conflict; in the average civil conflict, 50 civilians were killed every month. [17] Table 5 in the Appendix displays the number of civilian causalities by country.

To test our hypothesis that UNPOs are more effective at protecting civilians against rebel-inflicted violence, we draw on the UCDP GED's distinction between civilian deaths inflicted by the government and those at the hands of armed opposition groups. Overall, rebels killed more non-combatants than governments did. In the average country experiencing civil conflict, rebels killed 28 civilians per month, while 22 per month were killed by government forces.

To investigate the effect of UNPOs in combat areas and rear areas, where they perform different tasks, we distinguish between civilian casualties in these locations as follows: Using the coordinates of each conflict event, we calculate the distance between the location of civilian killings and the closest fatal combat event (i.e., the closest event that resulted in the death of one or more government or rebel combatant) during the previous five years. The location of civilian deaths is in a combat area if the killing was preceded, within five years, by any fatal combat event that occurred within 50 kilometers from the location where civilians were killed. Otherwise the location of the civilian deaths is considered a rear area. This definition was chosen to ensure that we do not erroneously qualify as rear areas those areas along pacified front lines where UNPOs successfully pursue the tasks assigned to them in combat areas. We took two precautions to prevent miscatego-

[16] Whenever a conflict event recorded in the GED dataset extended over more than one calendar month, an equal proportion of casualties was assumed to have occurred on each day between the start and the end of the violent event. We follow the previous literature on civilian killings (Eck and Hultman, 2007; Kreps and Wallace, 2009) in excluding one country-month observation that is an extreme outlier: with 146,211 civilian deaths, the Rwandan genocide in April of 1994 accounts for more non-combatant fatalities than all other 2,459 observations combined.

[17] Data on conflict-related fatalities is inevitably susceptible to bias (Sundberg and Melander, 2013), but the way the data was compiled makes us confident that our analyses establish a lower bound on the true effect of UN peacekeepers on civilian casualties, especially on those inflicted by rebels. Human coders constructed the measure of civilian deaths by mining news sources, NGO reports, case studies, truth commission reports, historical archives, and other sources of information (Sundberg and Melander, 2013), but the vast majority of sources in the UCDP's GED are news reports. Since UN peace operations afford protection to foreign journalists (see, e.g., Holt, Taylor and Kelly 2009, p. 278), the latter are more likely to be present in areas where UN peacekeepers are deployed, meaning that the common problem of underreporting of civilian casualties is less severe in areas where peacekeepers are present than in areas where no peacekeepers are deployed. The discrepancy is much less severe in government-held areas than in rebel-held areas, because foreign journalists typically enjoy the protection of the national police and armed forces in government-held areas where no UN peacekeepers are present. Thus, our analyses underestimate the true effect of UN blue helmets on civilian deaths, especially with respect to deaths by rebel forces.

rization of combat areas as rear areas. First, an area is considered a combat area as long as there was a single fatal combat event along an otherwise pacified front line over a long time period. Second, imprecision in the UCDP's location data introduces bias against misidentifying combat areas as rear areas: whenever exact coordinates are not available, province midpoints are used instead, and thus civilian deaths at imprecisely measured locations are coded as located in combat areas as long as there was at least one fatal combat event in the same province during the preceding five years whose exact location could also not be determined. The map in Figure 1 illustrates the distinction between civilian deaths in combat areas and those in rear areas in the midst of civil conflict in the Democratic Republic of the Congo. The map matches our qualitative knowledge of where the combat zones were, giving us greater confidence in our coding.[18]

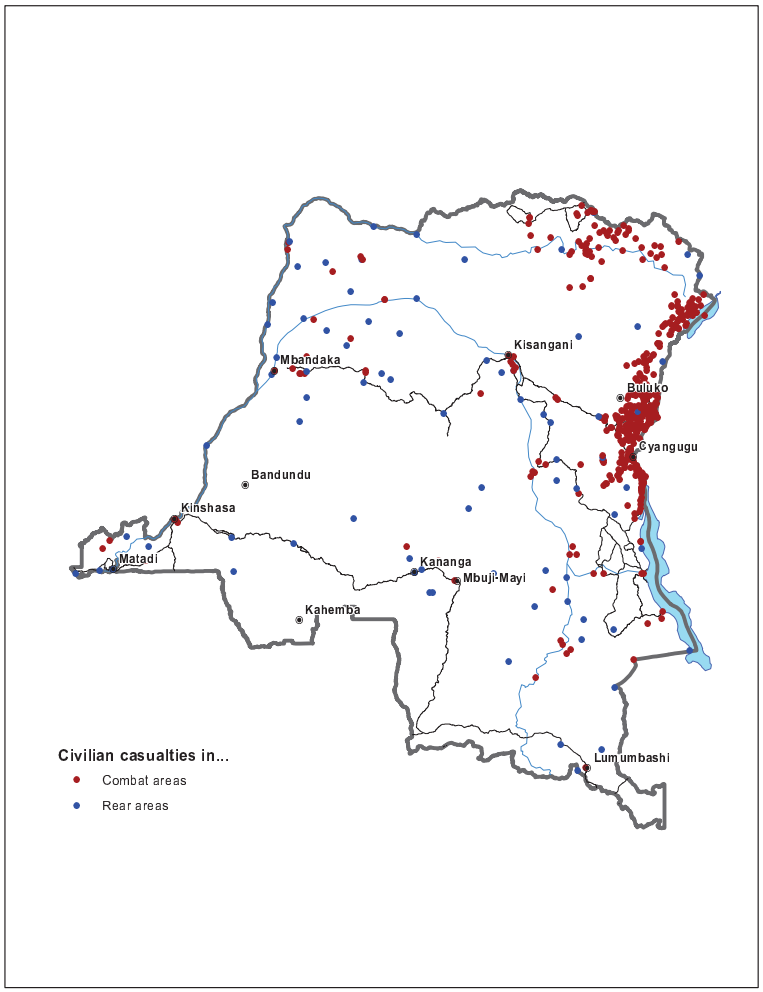
Data on which African region was represented on the UN Security Council in a given month is presented in Mikulaschek (2015). This variable takes a non-zero value for all civil conflicts during months when the region of the civil conflict was represented on the UN Security Council; it is lagged by one month. Data on the UN Security Council presidencies is from the website of the UN. To take into account the elevated leverage of the incoming Council president as well as delays in the deployment of any additional UN blue helmets that the Council's president is able to secure for civil-conflict theaters in her own region, the Council presidency instrument takes a non-zero value during the month the president holds office and the two preceding and the two following months; this measure is also lagged by one month.

Our models control for several country- and conflict-characteristics that have been shown to influence whether a UNPO is established in a civil-war setting, its size, and the prospect for its success.[19] The baseline probability of peacekeeping success is higher if the warring factions have formally agreed to multilateral peacekeeping (Doyle and Sambanis, 2006; Fortna, 2008*b*). The data come from the UCDP Peace Agreement Dataset v. 2.0 (Hogbladh, 2011); the variable records

[18]Our results are robust to operationalizing the distinction between combat and rear areas in a different way. When we use a 25-kilometer cutoff we categorize about 30 percent of all civilian casualties differently; yet our results are fully consistent with those reported in this paper. Additional results are available from the authors.

[19]These controls are not strictly necessary due the exogeneity of representation on the council; however, adding these controls reduced variation and thus increases efficiency.

Figure 1: Civilian deaths in combat and rear areas in the Democratic Republic of the Congo, 1989-2010



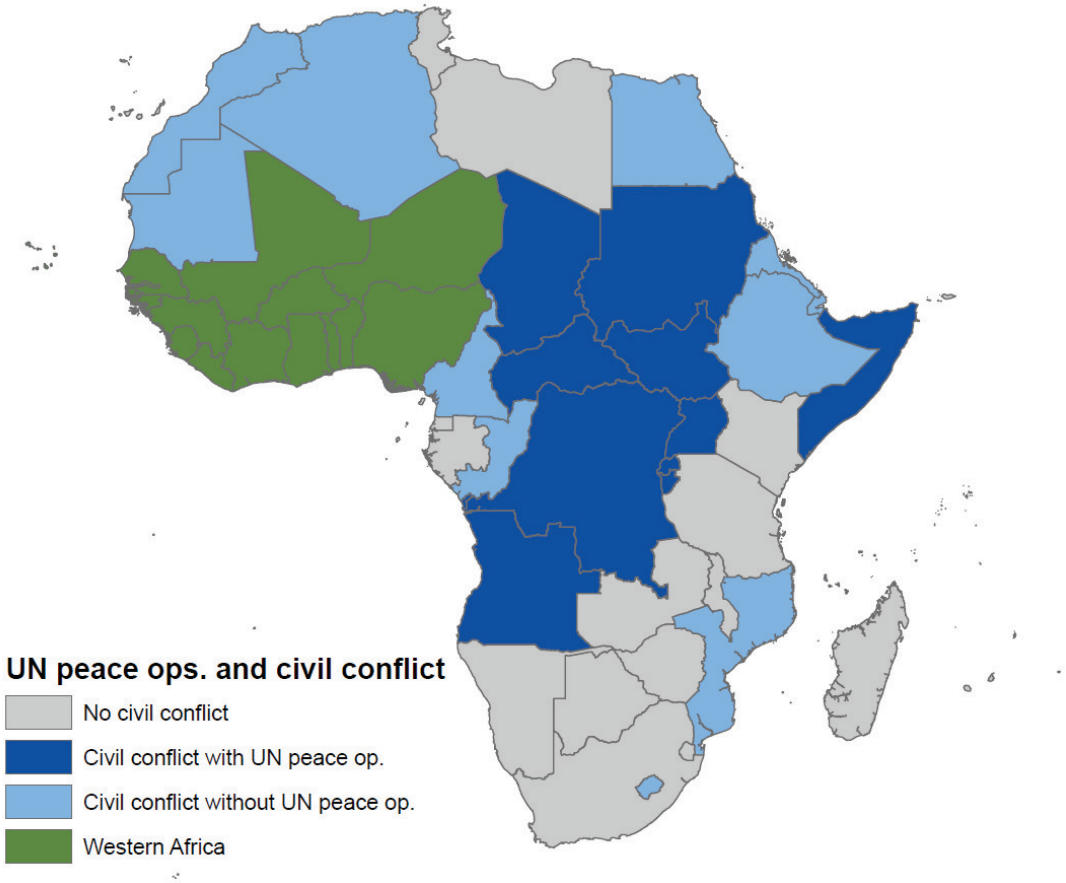
Note: The map depicts the location of all civilian deaths during periods of civil conflict in the Democratic Republic of the Congo between 1989 and 2010. Locations that are further than 50 kilometers away from the closest fatal combat event that occurred within five years from the time of the civilian killing are shown in blue (rear areas). Other locations of civilian deaths are depicted in red (combat areas). Major rivers appear in blue and main roads in black. When multiple killings occurred in the same location at different times, the map depicts this place as a combat area if the majority of fatal events occurred in proximity to a prior combat event. In contrast, our analyses allow for rear areas to turn into combat areas and vice versa. Data source: Sundberg and Melander (2013).

whether a peace agreement that was signed during the prior five years stipulated the establishment of a peace operation.[20] Data on conflict duration, which may also influence the establishment and success of peace operations, is taken from the UCDP Battle-Related Deaths Dataset v.5 (Sundberg, 2008). It captures the number of successive years with at least 25 battle-related deaths. The number of warring factions may also influence the baseline prospect of violence reduction (Doyle and Sambanis, 2006; Cunningham, 2011); therefore, we include the number of simultaneous conflicts in each state as recorded by the UCDP Georeferenced Event Dataset (GED v.1.5). In addition to controlling for population size (World Bank, 2014), we account for the government's per capita military expenditures (Stockholm International Peace Research Institute, 2014), because the baseline prospect for peacekeeping is lower where government forces are strong (Gilligan and Stedman, 2003). We also control for pre-war political rights (Freedom House, 2014) and per capita GDP in constant 2005 USD (World Bank, 2014) since economic development and political regime characteristics have been found to influence conflict duration and relapse risks (Buhaug and Gleditsch, 2008; Fortna, 2008*b*). Finally, the models account for the difficulty of terrain by including a time-varying measure of the percentage of land covered by forests (World Bank, 2014). Table 4 in the Appendix presents descriptive statistics for all variables.

The geographic scope of the study is limited to Central, Eastern, Northern, and Southern Africa, which are the four regions that rotate into two seats on the UN Security Council. Between 1989 and 2010, 23 countries in these regions suffered from a civil conflict. The UN Security Council deployed fifteen new peace operations to countries with ongoing civil conflict these four regions (see Table 6 for the names and size of these missions). During the post-Cold War era, almost one in three blue helmets in the world was deployed to a country in one of these four African regions, and almost four in ten US dollars that were spent on UN peacekeeping funded peace operations in these four regions. During the 2000s, the share of these four regions steadily increased to 63 percent of the UN's peacekeeping budget and 59 percent of the personnel of all UN peace operations as

[20]Controlling for this variable helps to mitigate the concern that violence drops off after a peace agreement is put in place, at which time many UN blue helmets also arrive. Further, note that we only investigate the local treatment effect from exogenous variation in influence on the UNSC.

Figure 2: Civil conflicts and UN peace operations in Africa, 1989-2010



Note: The map displays the 23 countries in Central, Eastern, North, and Southern Africa that experienced a civil conflict between 1989 and 2010 in blue. The ten countries where fifteen UN peace operations were deployed during ongoing civil conflict appear in dark blue whereas theaters of civil conflict without peacekeepers are shown in light blue. Conflicts and peacekeepers in Western Africa are not displayed, since Western Africa's representation on the UN Security Council is not subject to exogenous variation and the region is thus not part of this study. Mozambique and Morocco saw the deployment of UN peacekeepers in the aftermath of conflict. A peacekeeping mission was deployed on the border between Ethiopia and Eritrea in response to an interstate war between these two countries and not in the context of a civil conflict.

of December 2014 (Mikulaschek, 2015). Thus, our analyses capture a central part of the UN's peacekeeping efforts.

Model Specification

We are interested in estimating the following model:

$$DV_{it} = \beta_0 + \beta_1 Peacekeepers_{i(t-1)} + \sum_{k \in K} \beta_k I\,(i = k) + u_{it}, \tag{1}$$

where DV_{it} measures civilian casualties for country i in month t, $Peacekeepers_{i(t-1)}$ is the number of UN peacekeepers in month $t-1$, $I(\cdot)$ is an indicator function such that the summation represents country fixed effects, and u_{it} represents the unobserved error term. If $Peacekeepers_{i(t-1)}$ were randomly assigned (conditional on the fixed effects) we could estimate β_1, the marginal effect of a one unit increase in the number of peacekeepers, consistently with ordinary least squares. However, this condition remains unsatisfied since peacekeepers are allocated such that they are systematically related to the intensity of the civil conflict in the host country. In other words, $Peacekeepers_i$ is an endogenous variable.

To overcome this issue, we use an instrumental variables model, exploiting the as-if-randomly-assigned rotation of African regions onto the Council along with the exogenously determined rotation of the presidency as instruments for $Peacekeepers_{i(t-1)}$. As we have argued, the UNSC's president and temporary Council members can influence which countries peacekeepers are assigned to. We therefore utilize this as-if-random variation to generate predicted values from the first stage regression, thereby purging $Peacekeepers_{i(t-1)}$ of endogeneity:

$$Peacekeepers_{i(t-1)} = \gamma_0 + \gamma_1 IV_{i(t-1)} + \sum_{k \in K} \gamma_k I\,(i = k) + e_{it},$$

where $IV_{i(t-1)}$ is the instrumental variable.[21] We can now consistently estimate β_1 by regressing

[21] Three alternative model specifications use both instruments individually and in combination. The limited information maximum likelihood (LIML) estimator is chosen for the model that includes both instruments since it has been

DV_{it} on the predicted values of $Peacekeeping_{i(t-1)}$, along with the fixed effects.

In order to obtain consistent results, however, our instruments must satisfy the exclusion restriction: they can only affect the dependent variable through the endogenous variable.[22] While it is impossible to prove that the exclusion restriction is satisfied, we argue that it is after investigating possible violations. The first potential challenge to this assumption concerns the effect of UN Security Council membership on aid receipts. A seat on the Council has been shown to be associated with additional aid and more loans (Vreeland and Dreher, 2014), and this additional aid might alter the trajectory of civil conflict. This effect of serving on the Council would make it problematic to use individual states' Council membership as an instrument. However, this study leverages exogenous variation in the representation of African regions on the Council, and the vast majority of the civil conflicts that are examined in this study are not located in states that served on the Council themselves - and that were eligible for additional aid. In fact, temporary Council members only account for six percent of the observations. Excluding these observations from the analysis neither changes the magnitude nor the significance of the reported results.[23] Moreover, covariate balance analyses reported in Tables 12 and 13 show that countries experiencing civil war did not receive more aid when their region was represented on the Security Council or when a state in their region held the Council's presidency than they did otherwise.

A second potential challenge to the exclusion restriction concerns other UN policies besides peacekeeping (such as sanctions and mediation) through which regional representation on the Council and the presidency could affect the targeting of civilians in civil-conflict theaters. However, it is implausible that temporary exogenous shifts in power on the UN Security Council significantly alter UN sanctions in countries with ongoing civil conflict: irrespective of rotation in the

found to perform better in terms of bias and mean absolute error than alternative estimators with two instruments in a wide range of circumstances (Angrist and Pischke, 2009).

[22] γ_1 must also be nonzero, which we test by examining the estimated coefficients' significance.

[23] Results are available from the authors. The only countries in Central, Eastern, North, and Southern Africa that experienced civil conflict while serving on the UN Security Council were Algeria (2004-5), Angola (2004), Djibouti (1993-4), Egypt (1996-7), Ethiopia (1989-90), Rwanda (1994), and Uganda (2009-10). Years with Council membership and conflict are in parentheses.

presidency and in the representation of African regions on and off the Council, UN sanctions are hardly ever lifted before the end of a civil conflict, and this study only investigates the targeting of civilians during civil conflict. Finally, UN mediation is conducted by the UN Secretary-General and the Department of Political Affairs and not by the UN Security Council; therefore, it is implausible that temporary power in the UN Security Council would alter civilian casualties through changes in UN mediation. Covariate balance analyses confirm that regional representation on the Council and its presidency did not have a significant impact on UN sanctions and mediation (see Tables 10 and 11).[24]

Besides likely meeting the exclusion restriction, the instrumental variables meet the requirement of not being "weak" in the statistical sense. Tables 1-3 present the results from a statistical test designed to probe the strength of the instrument. Critical values for the Donald-Cragg statistic test whether the nominal 5% two-stage least-squares t-test for the hypothesis that $\beta = 0$ potentially exceeds 15% (Stock and Yogo, 2002).[25] In all models that include the rotating UN Security Council representation or both instruments the Donald-Cragg statistic exceeds this critical value (except in Model 9, where the two instruments pass the 20% threshold); the rotating UN presidency as sole instrument only exceeds the 15% threshold in Model 2. Thus, this instrument is weaker than the rotating regional representation and the combination of both. At the same time, all three model specifications (with both IVs included separately and together) yield the same results, both in terms of the magnitude and significance of the effect of peace operations.[26]

[24] Another potential challenge to the exclusion restriction is regime type: perhaps UNPOs effect the political regime in the host country which then leads to fewer civilian deaths. However, an effect on regime type would materialize much more slowly, and therefore they cannot easily explain short-term changes in patterns of civilian targeting that are associated with relatively short-term exogenous changes in the distribution of influence in the UNSC. We also control for political rights in the civil conflict country.

[25] For a recent political science application to a natural experiment and cross-sectional data see Ramsay (2011).

[26] Note also that we find no evidence that the first-stage results are driven by individual terms on the UNSC or by individual UNSC presidencies. See Mikulaschek (2015).

Results and discussion

We begin by analyzing the overall effect of peacekeepers on civilian casualties and find that the greater the number of blue helmets, the better is the protection of civilians. Table 1 reports the results from six models that support this finding. Models 4-6 include the full set of control variables as well as the endogenous measure of UNPO size. Model 5 uses the Council's rotating presidency as an instrument; when a state in the regional neighborhood of the conflict theater holds the presidency, the Council tends to deploy additional peacekeepers to the conflict area, and thus the UNPO staff is larger (by 322 persons on average) than it is in other months. In turn, every 100 additional peacekeepers deployed as a function of the rotating UN Security Council presidency are associated with 17 fewer civilian casualties per month, on average ($p<0.01$). Model 4 obtains a very similar result while exploiting a different source of exogenous variation in power on the UN Security Council. Whenever an African region is represented on the UN Security Council, the Council tends to increase the size of UNPOs in countries in that region that experience civil conflict; thus, UNPOs are larger (by 220 persons on average) when a Council seat is held by a state in that region. In turn, every 100 additional peacekeepers deployed due to exogenous variation in the representation of African regions on the UN Security Council reduce the monthly number of civilian casualties by 12, on average ($p<0.01$). Model 6 uses both instrumental variables and confirms these results on the effect of UNPO size. It indicates that UNPOs in civil-conflict countries tend to have 308 more peacekeepers whenever the regional neighborhood of these countries is represented on the Council; when a state in the conflict theater's neighborhood holds the presidency, the personnel size of UNPOs is higher by 357 people, on average, than it is during years when the region is absent from the Council. 100 additional blue helmets that are due to these exogenous increases in the influence of states in the region of the conflict theater on the UN Security Council are associated with 12 fewer civilian casualties by month, on average ($p<0.01$). Since the two sources of influence on the Council that we leverage to instrument for endogenous UNPO size are determined by pre-determined rotation rules, the two instruments are exogenous to confounding variables, in expectation. Thus, we would expect the estimate of the effect of UNPO size on civil-

ian casualties to be robust to excluding the control variables in Models 4-6. Models 1-3 present the same analyses without any controls, and they corroborate the result on the effect of UNPOs. Even without accounting for potential confounders, the IV models find an effect of UNPO size on civilian casualties ($p<0.1$).

We next investigate our central hypothesis: that the effect on rebel-inflicted deaths drives the overall negative effect of peacekeepers on civilian casualties. The models in Table 2 show that indeed, UNPOs have a much larger effect on civilian casualties that rebels inflict than on those at the hand of the host government, on whose consent and cooperation UNPOs ultimately depend. Models 7-9, which use both instruments individually and jointly, indicate that an additional 100 UN blue helmets that are deployed due to exogenous variation in influence on the UN Security Council, are associated with 11-15 fewer deaths of civilians at the hands of rebels ($p<0.04$). At the same time, larger UNPO size is not significantly associated with fewer civilian casualties caused by the government in any of the three models. The coefficient for the effect on government-inflicted civilian deaths is also much smaller than the corresponding quantity for killings by rebels, both in absolute terms and in relative terms, when a standardized measure of civilian killings is used.

Which causal mechanisms explain the negative effect of peacekeepers on rebel-inflicted deaths? The separate analyses of civilian deaths in areas with past fighting between warring factions and deaths in rear areas show that UNPOs are not merely effective at lowering civilian casualties in combat areas, where they are interpositioned between the warring factions, prevent accidents by facilitating communication, and stabilize control over areas close to the front lines. UNPOs also ameliorate the plight of civilians far behind the front lines, where they police vulnerable areas and help conflict parties' leaders monitor the behavior of their units. Models 13-18 in Table 3 indicate that the effect of UNPOs is larger in combat areas, both in absolute and in relative terms. The magnitude of the effect in combat areas is four to five times larger than it is in rear areas, which is partly explained by the fact that almost eight times more civilians are killed in combat areas than in rear areas. The effect is highly significant in combat and rear areas ($p<0.01$ except for model

Table 1: Two-stage least squares: Effect of UNPO size on civilian casualties

	Number of civilian casualties					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
UNPO size (*t-1*)	-0.050	-0.038	-0.045	-0.121	-0.166	-0.124
	(0.030)	(0.023)	(0.026)	(0.055)	(0.060)	(0.055)
Peace agreement provision on PK				17.83	36.68	19.29
				(158.6)	(218.5)	(162.9)
Conflict duration				2.594	3.013	2.626
				(2.854)	(4.362)	(2.968)
Simultaneous conflicts				43.01	37.31	42.57
				(17.95)	(14.74)	(17.66)
Political rights				-93.42	-140.1	-97.05
				(111.8)	(145.6)	(113.6)
Population size (ln.)				116.0	328.2	132.4
				(325.9)	(488.6)	(334.0)
Forest cover (%)				-8.313	-9.770	-8.426
				(14.93)	(22.56)	(15.50)
GDP per cap. (ln.)				-131.1	-296.3	-144.0
				(186.4)	(402.9)	(198.8)
Mil. expenditure per cap. (ln.)				0.671	1.294	0.720
				(1.562)	(2.159)	(1.606)
	Number of UNPO personnel					
	(1)	(2)	(3)	(4)	(5)	(6)
UNSC representation (*t-1*)	672.7		500.6	321.9		307.7
	(365.7)		(291.6)	(180.7)		(185.4)
UNSC presidency (*t-1*)		866.6	1,090.5		220.3	356.5
		(434.3)	(557.5)		(103.0)	(176.2)
Peace agreement provision on PK				363.8	421.5	367.1
				(1,359.7)	(1,388.0)	(1,360.0)
Conflict duration				9.727	9.254	9.703
				(36.01)	(36.06)	(36.02)
Simultaneous conflicts				-120.6	-127.2	-121.0
				(125.8)	(128.0)	(125.8)
Political rights				-1051.9	-1,033.3	-1050.9
				(679.4)	(677.0)	(679.7)
Population size (ln.)				4,567.6	4650.9	4,562.7
				(3,354.5)	(3,415.4)	(3,358.1)
Forest cover (%)				-39.84	-35.26	-40.16
				(176.5)	(173.9)	(176.8)
GDP per cap. (ln.)				-3,648.6	-3,659.4	-3,649.4
				(2,996.1)	(3,027.6)	(2,996.1)
Mil. expenditure per cap. (ln.)				14.00	13.84	14.00
				(15.63)	(15.62)	(15.62)
Observations	2,459	2,459	2,459	2,063	2,063	2,063
R-squared	0.007	0.007	0.010	0.194	0.190	0.194
Cragg-Donald statistic	18.28	15.99	12.27	13.04	3.18	6.58

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 2: Two-stage least squares: Effect of UNPO size on civilian casualties: variation by faction

	Number of civilian casualties inflicted by rebels			Number of civilian casualties inflicted by government		
Variables	(7)	(8)	(9)	(10)	(11)	(12)
UNPO size (*t-1*)	-0.116	-0.149	-0.118	-0.005	-0.017	-0.006
	(0.047)	(0.071)	(0.047)	(0.026)	(0.015)	(0.025)
Peace agreement provision on PK	40.37	54.19	41.37	-22.54	-17.51	-22.26
	(145.3)	(191.8)	(148.5)	(23.02)	(28.16)	(22.89)
Conflict duration	1.991	2.299	2.014	0.602	0.714	0.608
	(2.989)	(3.961)	(3.059)	(0.528)	(0.769)	(0.526)
Simultaneous conflicts	29.76	25.58	29.46	13.25	11.73	13.17
	(16.86)	(12.06)	(16.50)	(10.34)	(6.894)	(10.16)
Political rights	-100.7	-135.0	-103.2	7.315	-5.152	6.629
	(103.1)	(150.0)	(105.6)	(28.63)	(11.30)	(26.99)
Population size (ln.)	209.2	364.8	220.5	-93.30	-36.66	-90.18
	(281.2)	(513.9)	(293.0)	(138.2)	(51.26)	(131.5)
Forest cover (%)	1.958	0.889	1.880	-10.27	-10.66	-10.29
	(12.79)	(17.78)	(13.15)	(2.912)	(5.326)	(3.022)
GDP per cap. (ln.)	-88.77	-209.9	-97.52	-42.37	-86.45	-44.79
	(200.0)	(384.6)	(209.3)	(43.31)	(102.8)	(38.85)
Mil. expenditure per cap. (ln.)	0.574	1.030	0.607	0.0977	0.264	0.107
	(1.495)	(2.129)	(1.534)	(0.290)	(0.313)	(0.273)
	Number of UNPO personnel					
Variables	(7)	(8)	(9)	(10)	(11)	(12)
UNSC representation (*t-1*)	321.9		307.7	321.9		307.7
	(180.7)		(185.4)	(180.7)		(185.4)
UNSC presidency (*t-1*)		220.3	356.5		220.3	356.5
		(103.0)	(176.2)		(103.0)	(176.2)
Peace agreement provision on PK	363.8	421.5	367.1	363.8	421.5	367.1
	(1,359.7)	(1,388.1)	(1,360.0)	(1,359.7)	(1,388.1)	(1,360.0)
Conflict duration	9.728	9.254	9.703	9.728	9.254	9.703
	(36.02)	(36.06)	(36.02)	(36.02)	(36.06)	(36.02)
Simultaneous conflicts	-120.6	-127.2	-121.0	-120.6	-127.2	-121.0
	(125.8)	(128.1)	(125.8)	(125.8)	(128.1)	(125.8)
Population size (ln.)	4,567.6	4,651.0	4,562.7	4,567.6	4,651.0	4,562.7
	(3,354.5)	(3,415.4)	(3,358.1)	(3,354.5)	(3,415.4)	(3,358.1)
Political rights	-1,051.9	-1,033.3	-1,050.9	-1,051.8	-1,033.3	-1,050.9
	(679.4)	(677.1)	(679.7)	(679.4)	(677.1)	(679.7)
Forest cover (%)	-39.83	-35.26	-40.16	-39.83	-35.26	-40.16
	(176.4)	(173.9)	(176.8)	(176.4)	(173.9)	(176.8)
GDP per cap. (ln.)	-3,648.6	-3,659.5	-3,649.4	-3,648.6	-3,659.5	-3,649.4
	(2,996.1)	(3,027.6)	(2,996.0)	(2,996.1)	(3,027.6)	(2,996.0)
Mil. expenditure per cap. (ln.)	14.00	13.84	14.00	14.00	13.84	14.00
	(15.63)	(15.62)	(15.62)	(15.63)	(15.62)	(15.62)
Observations	2,063	2,063	2,063	2,063	2,063	2,063
R-squared	0.194	0.190	0.194	0.194	0.190	0.194
Donald-Cragg statistic	13.04	3.18	6.58	13.04	3.18	6.58

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

17 where $p<0.05$).[27]

Moreover, when we disaggregate fatalities in combat and rear areas, we find that rebel-inflicted deaths drive the result in both areas: in all parts of the conflict theater, UNPO size has a significant effect on civilian casualties inflicted by rebels whereas we do not find evidence of an effect on government killings. Whether on the front lines or far from heavy conflict areas, peacekeepers only impact the behavior of the rebel groups.[28]

What explains the discrepancy between our null finding for protection from government forces and previous findings of a significant reduction of government-inflicted civilian killings (Hultman, Kathman and Shannon, 2013)? While we cannot rule out the explanation that different temporal and geographic scopes and model specifications account for this discrepancy, additional analyses lead us to suspect that endogeneity in UNPO size is part of the answer. As shown in Table 7 in the Supplemental Appendix, regression models that do not instrument for UNPO size and are otherwise identical to models 4-6 in Table 1 indicate a significant effect of UNPO size on all civilian casualties and on those inflicted by rebels. Interestingly, they also suggest an effect on killings by the government. The latter effect becomes insignificant when we account for the endogeneity of UNPO size by exploiting exogenous variation in power on the UN Security Council. This suggests that results that indicate a significant effect of UNPO size on civilian casualties inflicted by the government are driven by selection on unobservable variables, such as the government's resolve to improve the plight of its population, which determine both its resort to violence against civilians and its willingness to consent to larger UNPOs in the conflict theater.

[27] Table 4 shows that a single country-month observation accounts for a sizable share of all civilian casualties in rear areas. This outlier relates to the targeting of civilians in Sudan in 1989. The results in models 16-18 are robust to excluding this observation. Neither the sign nor the level of significance of the estimated effect of UNPOs on civilian deaths in rear areas change when the outlier is dropped. Results are available from the authors.

[28] Additional results are available from the authors.

Table 3: Two-stage least squares: Effect of UNPO size on civilian casualties: spacial variation

	Number of civilian casualties in combat areas			Number of civilian casualties in rear areas		
Variables	(13)	(14)	(15)	(16)	(17)	(18)
UNPO size (*t-1*)	-0.043	-0.049	-0.044	-0.009	-0.011	-0.009
	(0.017)	(0.015)	(0.017)	(0.003)	(0.006)	(0.003)
Peace agreement provision on PK	-2.711	-0.178	-2.545	0.491	1.036	0.525
	(61.80)	(70.16)	(62.33)	(12.08)	(14.57)	(12.23)
Conflict duration	0.592	0.662	0.597	0.326	0.335	0.327
	(1.173)	(1.356)	(1.186)	(0.224)	(0.266)	(0.226)
Simultaneous conflicts	17.79	17.13	17.74	7.747	7.373	7.724
	(8.914)	(9.914)	(8.962)	(3.433)	(3.305)	(3.415)
Political rights	-29.37	-35.71	-29.79	-9.341	-10.97	-9.443
	(16.41)	(23.08)	(16.48)	(6.806)	(10.30)	(6.921)
Population size (ln.)	51.46	79.67	53.31	16.52	23.78	16.98
	(90.55)	(118.2)	(90.91)	(29.63)	(45.47)	(30.14)
Forest cover (%)	-6.883	-7.058	-6.895	-0.396	-0.430	-0.398
	(6.080)	(7.052)	(6.143)	(1.516)	(1.778)	(1.532)
GDP per cap. (ln.)	-99.69	-121.1	-101.1	-11.35	-17.09	-11.71
	(51.85)	(91.04)	(53.20)	(21.62)	(37.35)	(22.24)
Mil. expenditure per cap. (ln.)	0.372	0.453	0.377	0.0733	0.0951	0.0747
	(0.463)	(0.484)	(0.463)	(0.130)	(0.172)	(0.132)

	Number of UNPO personnel					
Variables	(13)	(14)	(15)	(16)	(17)	(18)
UNSC representation (*t-1*)	309.3		296.0	334.3		320.5
	(175.2)		(180.4)	(188.0)		(193.0)
UNSC presidency (*t-1*)		210.4	341.5		226.3	368.4
		(98.83)	(169.6)		(106.0)	(182.5)
Peace agreement provision on PK	360.1	415.7	363.2	291.6	348.5	294.6
	(1,362.3)	(1,389.7)	(1,362.7)	(1,379.3)	(1,407.1)	(1,380.1)
Conflict duration	11.72	11.36	11.70	5.930	5.458	5.898
	(34.49)	(34.56)	(34.49)	(36.23)	(36.31)	(36.23)
Simultaneous conflicts	-101.0	-107.3	-101.5	-235.3	-239.4	-235.9
	(114.9)	(116.5)	(114.9)	(111.6)	(115.5)	(111.6)
Political rights	-1,046.4	-1,028.7	-1045.5	-1,048.7	-1,029.8	-1,047.7
	(677.9)	(675.7)	(678.2)	(686.2)	(684.0)	(686.5)
Population size (ln.)	4,462.2	4,541.5	4,457.9	4,461.0	4,541.0	4,455.8
	(3,297.4)	(3,353.7)	(3,301.1)	(3,248.7)	(3,310.8)	(3,252.8)
Forest cover (%)	-36.05	-31.27	-36.34	-29.70	-24.44	-29.99
	(174.8)	(172.3)	(175.1)	(162.5)	(159.2)	(162.7)
GDP per cap. (ln.)	-3,478.6	-3,480.2	-3,479.1	-3,627.4	-3,635.3	-3,628.1
	(2,910.3)	(2,934.7)	(2,910.4)	(2,943.2)	(2,977.4)	(2,943.2)
Mil. expenditure per cap. (ln.)	13.43	13.25	13.42	14.01	13.84	14.01
	(15.36)	(15.35)	(15.35)	(15.35)	(15.34)	(15.35)
Observations	2,058	2,058	2,058	2,064	2,064	2,064
R-squared	0.188	0.185	0.188	0.196	0.192	0.196
Cragg-Donald statistic	12.06	2.91	6.09	14.13	3.36	7.13

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Robustness and sensitivity checks

All results reported above are robust to adding a linear time trend to the model to account for the increase in the overall number of UN peacekeepers between 1989 and 2010, the time period under investigation. Moreover, the results hold when the models are run with country- and year-fixed effects; both the magnitude and significance of the effect remains the same.[29]

The civil war in the Democratic Republic of the Congo accounts for a large share of the civilian casualties investigated in this study (see Table 5 in the Appendix), but excluding this conflict from the analysis leaves the results qualitatively unchanged. Even when the Congolese observations are omitted, the effect of UNPO size on civilian casualties inflicted by rebels is at least weakly significant ($p<0.05$, $p<0.06$, or $p<0.07$ depending on which instrument is used; see Table 8 in the Supplementary Appendix). At the same time, the coefficient of the effect of UNPO size on civilian casualties at the hands of the governement is not consistenly signed in the three models that include alternative IVs.[30]

Consistent with our argument that the UN Security Council's presidency and representation of African regions are as-if-randomly determined, the two instrumental variables are weakly predicted by the other covariates. Table 9, which is shown in the Supplemental Appendix due to space constraints, shows that not a single coefficient is significant at the conventional 95% level in either of the two models that regress the instruments on the covariates and state fixed effects. The UNSC presidency is weakly associated with larger population size and greater forest coverage; we cannot think of any plausible explanation of this weak correlation besides random chance. Overall, this result suggests that the rotation rules ensure the exogeneity of the two instruments.

Additional covariate balance analyses confirm that the exclusion restriction is highly plausible.

[29] In some of the two-way fixed-effects models some of the year-fixed effects drop out of the model due to collinearity, and the estimated covariance matrix of moment conditions is not of full rank since the number of covariates in the model becomes high relative to the sample size. That is why the models without year-fixed effects are reported in the paper. All additional results are available from the authors.

[30] In two of the models, larger UNPOs are associated with an insignificantly higher number of civilian casualties inflicted by the government, while the association is negative and significant when the UNSC presidency instrument is used. In the latter model, the instrument is weak, as it is also in one of the other two models.

They show that a given civil-war country did not experience more UN sanctions, more mediation attempts, larger aid inflows, and more support from foreign combatants allied to warring parties when its region was represented on the UN Security Council - or when a state in its region held the Council's presidency - than that same civil-war country did otherwise (see Tables 10 and 12 in the Supplemental Appendix).[31] These results are robust to including the controls found in the main model (see Tables 11 and 13 in the Supplemental Appendix). Since neither instrument has a significant effect on aid flows, UN sanctions, mediation, and foreign troop support to warring factions (at the 90% confidence level), the exclusion restriction likely holds even if these policy interventions increase or lower the number of civilian casualties.

We also do not find evidence that would suggest that warring factions that might anticipate the deployment of additional peacekeepers due to either of the exogenous variations in influence inside the UN Security Council strategically choose a different behavior when their region is represented on the Security Council or when its representative holds the Council's presidency than during other years. Specifically, civil war parties are not more or less likely to conclude or to break peace agreements during years when their region has elevated influence in the Council than during other years (see Table 14 in the Supplemental Appendix).[32] This null result is robust to including the controls in the main model (see Table 15).

[31]Data on aid commitments was extracted from the Aid Data 2.1 dataset (Tierney et al., 2011). The aid variables record the total amount of all aid commitments and multilateral aid commitments, respectively, to a country in a given year in million constant 2011 USD. A country-month dataset on UN sanctions was coded for this study from the data presented in Biersteker (2015). Analyses of sanctions is limited to the period starting in 1991 due to limited data availability. The binary UN sanctions variable takes a positive value if sanctions were in place against any actor in the civil-war country at the end of the month. Data on mediation was compiled by DeRouen, Bercovitch and Pospieszna (2011). The binary UN mediation takes a positive value for when a mediation episode was ongoing at the end of the month if the UN or a UN representative were identified as a third-party mediator in DeRouen, Bercovitch and Pospieszna (2011); the mediation measure captures whether any mediation episode was unfolding at the end of the month. The binary foreign troop support variable takes a positive value when a foreign state or non-state actor provides troops that fight alongside the government or rebels in the civil conflict. The data was coded for the period ending in 2009 and is presented in Hogbladh, Pettersson and Themner (2011). Descriptive statistics for all variables are shown in Table 4 in the Appendix.

[32]Data on the conclusion and collapse of peace agreements was extracted from the UCDP's Peace Agreement Dataset v. 2.0 (Hogbladh, 2011). 'New peace agreement' is a binary measure that indicates whether civil conflict parties concluded a new agreement during a given month. The dichotomous measure 'peace agreement collapse' takes a positive value for months that marked the failure of the implementation of the pact.

Conclusion

We have introduced a novel research design to analyze whether peacekeeping can protect civilians. While peacekeepers seek to promote this objective, the effect of their efforts remains the subject of extensive debate. This controversy is largely the product of theoretical and empirical difficulties, as previous work does not disaggregate civilian deaths,[33] and endogeneity and selection bias have presented largely intractable problems for empirical examinations of this question. Furthermore, the direction of the selection bias is unclear, as governments with large armed forces may receive fewer UN peacekeepers (Fortna, 2008*b*; Gilligan and Stedman, 2003). However, our novel theoretical argument combined with a natural experiment allowed us to overcome these issues by utilizing exogenous variation in which countries hold power in the UN Security Council. We demonstrated that when states hold more power, they deploy more peacekeepers to their preferred locations, and that these additional peacekeepers positively impact the treatment of civilians who are otherwise victimized by rebels, but not by the government. Our results using instrumental variables stand in contrast to the results from OLS models with the same controls, suggesting that endogeneity is indeed a serious issue without a credible identification strategy.

Our results are in line with our expectations which were derived from two considerations: (1) UNPOs rely de facto on the consent of the host country's government. When the government victimizes civilians, it often does so in pursuit of a military advantage, and it typically does not permit the UNPO to prevent it from attaining it. Knowing that the host government can force the UNPO out (as it did in Burundi and Chad), the UNPO adopts a very cautious approach to protecting civilians from government forces. (2) An increasing number of UNPOs is mandated to actively collaborate with the host country's military and police by training these forces. To perform this task, UNPOs need to maintain collaborative relationships with the host country's armed forces and police, which gives them an incentive not to respond harshly to civilian victimization by those same security forces.

[33] Though see Hultman, Kathman and Shannon (2013).

Furthermore, we probed the causal mechanism driving our results by using our knowledge of the nature of these dynamics. Specifically, we distinguished between civilian casualties in combat areas and those in rear areas. We interpret the effect of an increase in the size of a UNPO on civilian casualties in both of these areas as evidence of an impact of both sets of functions that UNPOs have in combat and rear areas. Consistent with our expectation, in both areas UNPO size only has an impact on civilian casualties inflicted by rebels, and not on civilian deaths at the hands of government forces.

As with any experimental analysis, it is important to consider the scope conditions on our findings. While our our empirical strategy necessitated a focus on four out of five African regions, the mechanisms driving our results are highly general and thus likely to apply in a variety of settings. We therefore believe that our results should pertain to countries outside of the African continent, in a variety of conflict scenarios around the world.[34] Further, while we analyze these dynamics over a specific time period, we expect that our main effect will become stronger over time, as almost all peacekeeping missions now come with an explicit mandate to protect civilians.

From a scholarly perspective, we have demonstrated the feasibility and importance of using quasi-random variation, in this case as a result of institutional rules, to go beyond analyzing correlations. Our identification strategy improves upon past work by isolating the effect of our variable of interest. In so doing, we contribute to the vast literature on the effects of peacekeeping, providing two credible instrumental variables that have eluded previous work. Further, we also confirmed the utility of looking at the size of peacekeeping operations, rather than simply relying on an indicator variable. The size of operations provides a more precise measure that leads to more accurate results and a more interpretable effect (Hultman, Kathman and Shannon, 2013). We also showed the importance of disaggregating civilian casualties. While we find that peacekeepers reduce rebel-inflicted fatalities, our null result on government-inflicted fatalities cast doubt on peacekeepers' abilities to remain unbiased. Whether having a larger impact on rebel activities affects the quality

[34]We expect our first stage results, however, only to obtain where states from a particular region represented on the UNSC prefer larger UNPOs in their own regional neighborhoods. Future work could determine whether this is the case in the Middle East and Asia.

of the peace or the type of post-conflict arrangements reached remains an interesting area for future work.

From a policy perspective, our findings have a number of important implications. For example, our findings have implications for the numerous reforms that have been proposed that would alter the Council's composition. Specifically, our first stage results show that representation on the UNSC alters the locations to which peacekeepers are deployed, which should be taken into account when evaluating the merits of the various proposals. Second, our identification strategy allows us to determine that greater numbers of peacekeepers cause improved protection of civilians from rebel atrocities, not merely that a correlation exists. Thus, common concerns with increasing peacekeeping budgets, such as perceptions that it is wasteful, seem to be largely unfounded. On the contrary, incrementally boosting peacekeeping funds has an impact on the ground. In addition, our results challenge the traditional view of peacekeeping as merely placing people on the blue line – instead, we show that these activities also affect civilians behind the front lines. Finally, our finding of no effect on government-caused deaths suggests that perhaps alternative strategies for preventing these casualties should be sought, and current activities could focus more heavily on limiting violence perpetrated by rebel groups. Indeed, successful avenues for diminishing violence at the hands of the government remains an interesting and productive area for future research.

Appendix

Table 4: Descriptive statistics

Variable	*N*	Mean	St.dev.	Min.	Max.
Civilian deaths	2,459	50.49	402.2	0	13,095
Civilian deaths by rebels	2,459	28.26	377.8	0	12,844
Civilian deaths by government	2,459	22.22	116.4	0	2,631
Civilian deaths in combat areas	2,459	45.24	397.1	0	13,058
Civilian deaths in rear areas	2,459	5.243	37.71	0	1,004
UNSC representation	2,459	0.526	0.499	0	1
UNSC presidency	2,459	0.153	0.359	0	1
UNPO size	2,459	1,036.3	4,226.6	0	32,698
Peace agreement provision on PK	2,459	0.036	0.188	0	1
Conflict duration	2,459	8.967	8.003	1	33
Simultaneous conflicts	2,459	1.845	1.554	0	9
Political rights	2,411	6.059	0.890	3	7
Population size (ln.)	2,459	16.50	1.060	12.91	18.28
Forest cover (%)	2,459	18.67	18.95	0.0487	69.91
GDP per cap. (ln.)	2,291	6.132	0.922	4.736	8.052
Mil. expenditure per cap. (ln.)	2,123	26.92	34.18	1.362	192.9
UN sanctions	2,255	0.231	0.422	0	1
UN mediation	2,459	0.046	0.210	0	1
Mediation	2,459	0.110	0.313	0	1
Multilateral aid	2,459	0.407	2.938	0	36.37
All aid	2,459	21.83	70.82	0	840.1
Foreign troop support	2,327	0.175	0.380	0	1
New peace agreement	2,459	0.022	0.147	0	1
Peace agreement collapse	2,459	0.005	0.070	0	1

Table 5: Number of civilian deaths in Central, Eastern, North, and Southern Africa by country

Country	Total number of civilian deaths	Share of civilian deaths in %	Number of civilians killed by rebels	Number of civilians killed by government
Algeria	1,934	1.6	1,710	224
Angola	3,976	3.2	2,326	1,634
Burundi	8,253	6.6	2,794	5,459
Cameroon	2	0.0	0	2
Central African Republic	348	0.3	157	191
Dem. Rep. of the Congo	52,756	42.5	43,404	9,353
Chad	2,171	1.7	1,053	111,8
Djibouti	2	0.0	0	2
Egypt	244	0.2	205	39
Eritrea	140	0.1	0	140
Ethiopia	3,129	2.5	212	2,917
Mozambique	1,573	1.3	1,323	250
Republic of Congo	1,567	1.3	127	1,440
Rwanda	16,824	13.6	2,290	14,534
Somalia	5,062	4.1	650	4,412
Sudan	20,675	16.7	8,386	12,289
Uganda	5,503	4.4	4,876	627
Sum	124,159	100	69,513	54,647

Note: The table shows the number of civilian casualties during ongoing civil conflicts in Central, Eastern, Southern, and North Africa between 1989 and 2010. Note that the Democratic Republic of the Congo accounts for a large share of all civilian casualties; to ensure that the results of this study are not driven solely by this conflict some of our robustness checks exclude the Congolese observations. The figures exclude the 146,211 identified casualties of the Rwanda genocide in April 1994, because this event is an extreme outlier; as a single country-month observation, it accounts for more civilian fatalities in a single month than all other 2,459 civil-conflict-month observations combined (see fn. 16 above for more details). Data source: Sundberg and Melander (2013).

Table 6: Names and size of UNPOs in Central, Eastern, North, and Southern Africa

Country	Names of UNPOs	UNPO size mean	UNPO size max.
Angola	UNAVEM I, UNAVEM II, UNAVEM III, MONUA	1,125.4	7,302
Burundi	ONUB	699.2	5,665
Central African Republic	MINURCAT	98.88	296
Dem. Rep. of the Congo	MONUC, MONUSCO	6,315.8	18,536
Chad	MINURCAT	241.9	3,518
Rwanda	UNAMIR	232.0	5,645
Somalia	UNOSOM I, UNOSOM II, UNSOA	1,374.4	24,566
Sudan	UNMIS, UNAMID	4,607.4	32,860
Uganda	UNOMUR	2.452	81
Total		1,043.8	32,860

Note: The table indicates the names and size of the fifteen UN peace operations deployed during ongoing civil conflicts in Central, Eastern, Southern, and North Africa between 1989 and 2010. The average (maximal) size represents the mean (maximal) number of troops, military observers, and civilian police deployed as part of the peace operation while the conflict was ongoing. For each country, the minimal number of UN peace operation staff deployed while the conflict was ongoing was zero. Two additional peace operations were established in the aftermath of conflicts in Morocco and Mozambique. Moreover, an additional peace operation was deployed on the border between Ethiopia and Eritrea in response to an interstate war between these two countries and not in the context of a civil conflict. Data source: Mikulaschek (2015).

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Appendix of Supporting Information

(Not for publication)

Table 7: State f.e. OLS regressions: Size of UNPOs on civilian casualties

Variables	Number of all civilian casualties (19)	Number of civilian casualties inflicted by government (20)	Number of civilian casualties inflicted by rebels (21)
UNPO size (*t-1*)	-0.028	-0.023	-0.004
	(0.004)	(0.003)	(0.002)
Peace agreement provision on PK	-21.01	1.794	-22.81
	(39.68)	(31.23)	(14.16)
Conflict duration	1.731	1.134	0.596
	(0.909)	(1.095)	(0.525)
Simultaneous conflicts	54.77	41.44	13.33
	(29.94)	(32.12)	(8.022)
Political rights	2.837	-5.140	7.977
	(18.81)	(16.94)	(4.144)
Population size (ln.)	-321.4	-225.1	-96.30
	(241.5)	(225.2)	(35.23)
Forest cover (%)	-5.309	4.941	-10.25
	(7.195)	(6.258)	(3.656)
GDP per cap. (ln.)	209.2	249.2	-40.03
	(286.1)	(255.8)	(58.55)
Mil. expenditure per cap. (ln.)	-0.613	-0.702	0.089
	(0.722)	(0.612)	(0.155)
Constant	4,123	1,830	2,293
	(2,231)	(1,911)	(972.4)
Observations	2,063	2,063	2,063
R-squared	0.120	0.089	0.111

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 8: Two-stage least squares: Effect of UNPO size on civilian casualties: omitting DRC

	Number of civilian casualties inflicted by rebels			Number of civilian casualties inflicted by government		
Variables	(22)	(23)	(24)	(25)	(26)	(27)
UNPO size (*t-1*)	-0.051	-0.055	-0.052	0.014	-0.045	0.007
	(0.027)	(0.028)	(0.026)	(0.071)	(0.021)	(0.068)
Peace agreement provision on PK	26.97	29.67	27.37	-36.57	6.676	-31.29
	(65.97)	(72.94)	(66.55)	(54.08)	(58.98)	(53.88)
Conflict duration	1.855	2.001	1.876	-0.099	2.246	0.186
	(1.230)	(1.388)	(1.188)	(2.810)	(1.130)	(2.703)
Simultaneous conflicts	8.813	8.890	8.824	16.30	17.54	16.45
	(2.529)	(2.529)	(2.521)	(6.817)	(8.517)	(6.928)
Political rights	-14.89	-16.02	-15.06	12.20	-6.043	9.971
	(15.51)	(16.75)	(15.39)	(23.66)	(11.56)	(22.46)
Population size (ln.)	42.12	46.58	42.78	-98.09	-26.67	-89.38
	(48.93)	(47.19)	(47.31)	(94.62)	(29.99)	(94.02)
Forest cover (%)	-0.331	-0.269	-0.321	-10.57	-9.583	-10.45
	(4.034)	(4.405)	(4.084)	(3.434)	(5.859)	(3.715)
GDP per cap. (ln.)	-33.18	-35.41	-33.51	-53.24	-89.08	-57.61
	(34.36)	(38.15)	(34.39)	(32.77)	(84.97)	(32.15)
Mil. expenditure per cap. (ln.)	-0.066	-0.072	-0.067	0.229	0.133	0.217
	(0.302)	(0.356)	(0.309)	(0.228)	(0.326)	(0.221)

	Number of UNPO personnel					
Variables	(22)	(23)	(24)	(25)	(26)	(27)
UNSC representation (*t-1*)	127.9		115.8	127.9		115.8
	(100.5)		(106.4)	(100.5)		(106.4)
UNSC presidency (*t-1*)		106.2	157.9		106.2	157.9
		(63.22)	(97.69)		(63.22)	(97.69)
Peace agreement provision on PK	710.4	735.1	713.7	710.4	735.1	713.7
	(1,393.6)	(1,415.3)	(1,396.2)	(1,393.6)	(1,415.3)	(1,396.2)
Conflict duration	39.81	39.66	39.78	39.81	39.66	39.78
	(19.49)	(19.32)	(19.50)	(19.49)	(19.32)	(19.50)
Simultaneous conflicts	23.18	20.27	22.71	23.18	20.27	22.71
	(30.86)	(30.04)	(30.93)	(30.86)	(30.04)	(30.93)
Population size (ln.)	1,166.0	1,188.7	1,162.1	1,166.0	1,188.7	1,162.1
	(511.3)	(494.7)	(510.8)	(511.3)	(494.7)	(510.8)
Political rights	-318.9	-309.3	-318.2	-318.9	-309.3	-318.2
	(243.3)	(244.3)	(243.9)	(243.3)	(244.3)	(243.9)
Forest cover (%)	13.70	15.31	13.41	13.70	15.31	13.41
	(86.62)	(85.96)	(86.83)	(86.62)	(85.96)	(86.83)
GDP per cap. (ln.)	-614.0	-611.3	-615.1	-614.0	-611.3	-615.1
	(594.3)	(597.4)	(594.8)	(594.3)	(597.4)	(594.8)
Mil. expenditure per cap. (ln.)	-1.483	-1.583	-1.480	-1.483	-1.583	-1.480
	(5.431)	(5.408)	(5.429)	(5.431)	(5.408)	(5.429)
Observations 1,967	1,967	1,967	1,967	1,967	1,967	
R-squared	0.113	0.111	0.113	0.113	0.111	0.113
Donald-Cragg statistic	6.51	2.33	3.41	6.51	2.33	3.41

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 9: State f.e. regression: covariates on instruments

Variables	UNSC presidency (28)	UNSC representation (29)
Peace agreement provision on PK	0.166	-0.019
	(0.156)	(0.077)
Conflict duration	-0.001	0.0001
	(0.005)	(0.001)
Simultaneous conflicts	-0.018	0.004
	(0.011)	(0.005)
Pre-conflict pol. rights	0.055	-0.005
	(0.049)	(0.021)
Population size (ln.)	0.409	0.220
	(0.261)	(0.106)
Forest cover (%)	0.024	0.014
	(0.012)	(0.006)
GDP per cap. (ln.)	-0.03	0.009
	(0.221)	(0.085)
Mil. expenditure per cap. (ln.)	-0.0006	-0.0002
	(0.0008)	(0.0004)
Constant	-6.808	-3.773
	(3.217)	(1.433)
Observations	2,063	2,063
R-squared	0.016	0.007

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 10: State f.e. regression: instruments on potential determinants of civilian casualties

	UN sanctions		UN mediation		Mediation	
	(30)	(31)	(32)	(33)	(34)	(35)
UNSC representation (*t-1*)	0.004		0.001		-0.021	
	(0.027)		(0.013)		(0.019)	
UNSC presidency (*t-1*)		0.011		-0.018		-0.014
		(0.021)		(0.022)		(0.029)
Constant	0.229	0.230	0.046	0.049	0.121	0.112
	(0.013)	(0.003)	(0.007)	(0.003)	(0.010)	(0.004)
Observations	2,255	2,255	2,459	2,459	2,459	2,459
R-squared	0.0001	0.0004	0.0001	0.001	0.0001	0.000

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 11: State f.e. regression: instruments and controls on potential determinants of civilian casualties

	UN sanctions		UN Mediation		Mediation	
	(36)	(37)	(38)	(39)	(40)	(41)
UNSC representation (*t-1*)	-0.019		0.012		-0.019	
	(0.025)		(0.012)		(0.024)	
UNSC presidency (*t-1*)		-0.006		-0.011		-0.014
		(0.0132)		(0.0131)		(0.027)
Conflict duration	0.008	0.008	-0.004	-0.004	0.001	0.001
	(0.008)	(0.008)	(0.002)	(0.002)	(0.003)	(0.003)
Simultaneous conflicts	0.018	0.018	-0.011	-0.012	-0.003	-0.003
	(0.019)	(0.019)	(0.007)	(0.007)	(0.010)	(0.010)
Political rights	0.036	0.035	0.016	0.016	0.025	0.024
	(0.070)	(0.069)	(0.022)	(0.022)	(0.034)	(0.033)
Population size (ln.)	0.782	0.774	-0.313	-0.306	-0.287	-0.291
	(0.329)	(0.328)	(0.195)	(0.195)	(0.198)	(0.197)
Forest cover (%)	-0.010	-0.010	-0.013	-0.013	-0.031	-0.031
	(0.034)	(0.034)	(0.011)	(0.011)	(0.011)	(0.011)
GDP per cap. (ln.)	-0.670	-0.671	0.199	0.198	0.349	0.350
	(0.401)	(0.398)	(0.222)	(0.222)	(0.272)	(0.272)
Mil. expenditure per cap. (ln.)	-0.002	-0.002	0.003	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	-8.786	-8.660	4.141	4.023	3.073	3.143
	(3.919)	(3.977)	(3.156)	(3.154)	(3.509)	(3.458)
Observations	1,931	1,931	2,063	2,063	2,063	2,063
R-squared	0.149	0.148	0.241	0.240	0.148	0.019

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 12: State f.e. regression: instruments on potential determinants of civilian casualties

	Multilateral aid		All aid		Foreign troop support	
	(42)	(43)	(44)	(45)	(46)	(47)
UNSC representation (*t-1*)	-0.414		12.78		0.051	
	(0.358)		(7.933)		(0.041)	
UNSC presidency (*t-1*)		-0.394		10.87		0.039
		(8.233)		(7.369)		(0.029)
Constant	0.625	0.468	15.14	20.18	0.149	0.169
	(0.187)	(0.043)	(4.151)	(1.124)	(0.021)	(0.004)
Observations	2,459	2,459	2,459	2,459	2,327	2,327
R-squared	0.006	0.002	0.007	0.003	0.003	0.003

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 13: State f.e. regression: instruments and controls on potential determinants of civilian casualties

	Multilateral aid		All aid		Foreign troop support	
	(48)	(49)	(50)	(51)	(52)	(53)
UNSC representation (*t-1*)	-0.477		13.28		0.068	
	(0.390)		(8.366)		(0.049)	
UNSC presidency (*t-1*)		-0.494		7.575		0.045
		(0.309)		(6.953)		(0.036)
Conflict duration	0.007	0.007	-0.244	-0.256	0.007	0.007
	(0.010)	(0.009)	(0.876)	(0.875)	(0.005)	(0.004)
Simultaneous conflicts	0.358	0.368	-2.939	-3.190	-0.010	-0.010
	(0.239)	(0.245)	(4.776)	(4.875)	(0.009)	(0.009)
Political rights	0.201	0.174	-14.35	-13.63	0.048	0.051
	(0.236)	(0.237)	(16.77)	(16.66)	(0.0583)	(0.058)
Population size (ln.)	-2.956	-3.039	77.34	81.00	-0.132	-0.122
	(3.267)	(3.344)	(53.80)	(55.11)	(0.289)	(0.292)
Forest cover (%)	0.092	0.088	-4.114	-3.935	-0.029	-0.029
	(0.074)	(0.073)	(5.219)	(5.096)	(0.039)	(0.039)
GDP per cap. (ln.)	4.885	4.907	23.19	22.65	0.451	0.444
	(3.800)	(3.835)	(69.11)	(70.13)	(0.226)	(0.221)
Mil. expenditure per cap. (ln.)	-0.012	-0.012	0.241	0.234	0.002	0.002
	(0.009)	(0.009)	(0.372)	(0.374)	(0.001)	(0.001)
Constant	16.57	17.83	-1,248	-1,306	-0.302	-0.418
	(29.66)	(30.61)	(775.9)	(782.0)	(5.009)	(5.094)
Observations	2,063	2,063	2,063	2,063	1,967	1,967
R-squared	0.062	0.059	0.105	0.098	0.139	0.129

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 14: State f.e. regression: instruments on potential determinants of civilian casualties

	New peace agreement		Peace agreement collapse	
	(54)	(55)	(56)	(57)
UNSC representation (*t-1*)	0.000		-0.004	
	(0.007)		(0.003)	
UNSC presidency (*t-1*)		0.006		-0.002
		(0.007)		(0.004)
Constant	0.022	0.022	0.007	0.005
	(0.003)	(0.001)	(0.002)	(0.001)
Observations	2,351	2,351	2,351	2,351
R-squared	0.000	0.0003	0.001	0.0001

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.

Table 15: State f.e. regression: instruments and controls on potential determinants of civilian casualties

	New peace agreement		Peace agreement collapse	
	(58)	(59)	(60)	(61)
UNSC representation (*t-1*)	0.004		-0.005	
	(0.008)		(0.003)	
UNSC presidency (*t-1*)		0.008		-0.001
		(0.009)		(0.004)
Conflict duration	0.002	0.002	0.0003	0.0003
	(0.001)	(0.001)	(0.0002)	(0.0002)
Simultaneous conflicts	0.007	0.007	0.002	0.002
	(0.003)	(0.003)	(0.001)	(0.001)
Political rights	-0.014	-0.014	-0.003	-0.003
	(0.008)	(0.008)	(0.002)	(0.002)
Population size (ln.)	-0.085	-0.085	-0.040	-0.042
	(0.054)	(0.054)	(0.014)	(0.015)
Forest cover (%)	-0.009	-0.009	-0.003	-0.003
	(0.002)	(0.002)	(0.001)	(0.001)
GDP per cap. (ln.)	0.042	0.042	0.008	0.009
	(0.046)	(0.046)	(0.025)	(0.025)
Mil. expenditure per cap. (ln.)	0.00007	0.00007	0.00005	0.00005
	(0.0001)	(0.0001)	(0.00005)	(0.00005)
Constant	1.400	1.404	0.682	0.705
	(0.776)	(0.789)	(0.157)	(0.164)
Observations	1,991	1,991	1,991	1,991
R-squared	0.014	0.014	0.007	0.005

Note: Heteroskedasticity consistent s.e. clustered by state in parentheses.