Peacekeepers Help, Governments Hinder

Abstract

Conflict causes enormous suffering, but the study of peacekeeping is plagued by endogeneity issues. This paper uses an instrumental variables approach to estimate the effectiveness of U.N. peacekeepers at ending episodes of conflict, maintaining the peace once peace has been obtained, and preventing another episode from ever re-occurring. I find that the likelihood of being sent U.N. peacekeepers varies with temporary membership in the U.N. Security Council and exploit this variation in my estimation. This variation also suggests that the leaders of countries in conflict often do not want their country to receive peacekeepers. The results indicate that even though peacekeepers are often unwanted, they help to maintain the peace after an episode of conflict has ended and reduce the likelihood that the conflict resumes.

1 Introduction

Conflict exacts a tremendous toll on the people in the areas in which it occurs. Apart from battle deaths, it has been estimated that there are at least 200,000 indirect conflict deaths per year,[1] mostly from worse nutrition and increased disease, on top of any other physical or mental health problems that result from the conflict. Conflict is also implicated as a major cause of enduring poverty and lack of economic growth. Countries in conflict tend to grow approximately 2.2% less per year than they would in peace.[2] Thus, for a typical seven-year civil war, incomes would end up 15% lower and absolute poverty would increase by about 30% relative to the conflict not occuring.[3] Nor are the economic effects limited to

[1] Graduate Institute of International and Development Studies 2011.
[2] Collier 1999.
[3] World Bank 2003.

the time of the conflict; with infrastructure destroyed, education interrupted, and governance worsened, states continue to suffer economically long after a conflict has ended.[4]

A large literature exists focusing on the initial causes of conflict[5], and there has also been some study of the ending of conflicts or the post-conflict maintenance of peace, including the question of whether peacekeepers are effective.[6] However, the historical problem with evaluating the effectiveness of peacekeeping is that peacekeepers are not randomly sent to episodes of conflict and perhaps are sent to cases which would have longer or shorter durations of peace even without peacekeepers. This paper for the first time identifies an instrumental variable that predicts which episodes of conflict receive U.N. peacekeepers and uses this instrument to answer two questions: whether peacekeeping helps to extend the duration of peace after an episode of conflict has ended, and whether peacekeeping helps prevent another episode of the same conflict from ever reoccuring. Civil wars with at least 1,000 battle-related deaths will be the focus of this paper, as further described in the data section.

While the very name "peacekeepers" suggests an effective force, whether peacekeepers contribute to or lessen the odds of peace is theoretically ambiguous. Some ways in which peacekeepers have been predicted to help maintain peace are by increasing the cost of fighting through threatening the use of force and offering incentives to disarm; decreasing uncertainty about the actions and intentions of each party and making contracts more credible through monitoring and (more limited) enforcement; preventing isolated or small groups of actors from acting as "spoilers" as well as preventing accidental re-engagements by providing a neutral physical buffer zone between parties; and decreasing political oppression or extraction from one side in those conflicts in which this is relevant.[7] In contrast, peace can also be more likely to last when one side has a more decisive victory,[8] suggesting the unhappy conclusion

[4]Ibid.
[5]*E.g.* Fearon 1995; Collier and Hoeffler 1998; Powell 2002.
[6]*E.g.* Doyle and Sambanis 2000; Fortna 2004; Fortna 2008.
[7]Doyle and Sambanis 2000; Fortna 2004; Mattes and Savun 2010.
[8]Hensel 1994.

that if peacekeepers help to artificially end conflicts sooner but less decisively, they may actually harm the long-term prospects of peace. There is also the question of to what extent the initial causes of conflict remain untouched by peacekeeping. For example, externally imposed peace may leave a government in power that is not perceived as legitimate, leading to an unstable future. If these theories are correct, peacekeepers may at best serve only to tamp down violence momentarily, with peace not lasting once the peacekeepers withdraw.

In this paper I focus on U.N. peacekeeping since U.N. peacekeeping constitutes the majority of the world's peacekeeping. U.N. peacekeeping operations are the purview of the U.N. Security Council, which has five permanent members and ten temporary members that serve for staggered two year terms, with five new temporary members rotating in each year. The timing of the assignment of countries to the U.N. Security Council provides plausibly exogenous variation in the likelihood of being sent peacekeepers. While we might think that Security Council members are special in some way that could bias results, I perform a few robustness checks to mitigate this concern.

Although case studies suggest there is a lot of heterogeneity in peacekeeping, I will only be able to estimate the general effectiveness of U.N. peacekeeping. These estimates still provide a significant contribution to the literature. While peacekeeping has been studied for decades[9], little work has addressed the potential endogeneity of where peacekeepers get sent. Doyle and Sambanis[10] pay attention to the concern of endogeneity but do not find notable evidence of it in their dataset; this may be due to the particular data they use, including the different years of focus, or the variables they examine.[11] Fortna[12] does find evidence of

[9] *E.g.* Haas, Butterworth and Nye 1972; Wilkenfeld and Brecher 1984; Diehl, Reifschneider and Hensel 1996.

[10] 2000.

[11] They try to instrument for receiving U.N. peacekeepers using a variety of instruments (being in Europe; real GDP per capita; third party partial intervention; non-U.N. peacekeeping operation; and military outcome), but it is not clear that these instruments are particularly strong and we may suspect some violate the exclusion principle. In particular, if the fear is that U.N. peacekeeping operations are endogenously selected, one might also worry that non-U.N. peacekeeping operations are endogenously selected. Being in Europe, real GDP per capita and military outcome may also be directly correlated with the duration of peace. In short, while Doyle and Sambanis did a thorough job searching for endogeneity, they could have simply missed the right variables.

[12] 2004.

endogeneity in where peacekeepers get sent and explicitly focuses on dealing with it with a study that adds characteristics of conflicts as controls to mitigate this problem.

Gilligan and Sergenti[13] also focus on the concern of endogeneity. Their chosen method to address it, matching, is often considered an alternative to using a Heckman approach, whereby selection is characterized and taken into account, so they take care to explain why they think the use of matching estimators would be more appropriate. The instrumental variable (IV) approach and control function approach used in this paper are two forms of the Heckman selection model. While Gilligan and Sergenti are right to point out the assumptions that proper use of a Heckman approach requires, whether an instrument is valid can, of course, only be determined on a case-by-case basis, given a particular instrument and the question to be answered. I will argue in a later section that the instrument used in this paper fits the criteria for being a good instrument. First, however, it is important to further discuss the matching approach and why, if the instrument did prove to be valid, it would be appropriate to use in this situation.

To set the scene, let us review the assumptions required by three methods - standard regressions, which ignore potential endogeneity; matching methods; and the IV approach. A standard regression that ignores potential endogeneity simply assumes that treatment is exogenous conditional on covariates.[14] For us, this would mean that where peacekeepers get sent is not correlated with the duration of peace except through the presence of peacekeepers; in other words, there is no selection that biases results. This is known as the conditional independence assumption. Matching makes the same assumption. As a consequence, it is only unbiased when the assumption that the covariates that are matched on are the only relevant covariates is correct. If there remain any important characteristics that are not matched on (the analog of being controlled for), results will be biased. The instrumental variables approach, in contrast, assumes that rather than the treatment of receiving peacekeepers being

[13] 2008.

[14] In mathematical notation, $(Y_{i1}, Y_{i0}) \perp D_i | X_i$, where Y_{i1} and Y_{i0} are the outcomes of those treated and those not treated, respectively, D_i is a dummy variable representing assignment into treatment, and X_i are the covariates.

exogenous conditional on covariates, there is an instrument which is exogenous conditional on covariates and this instrument is not independent of the treatment when conditioning on the same covariates.[15] Matching is in this sense a special case of an IV approach in which the treatment dummy is the instrument; we use an IV when we do not think that treatment is exogenous conditional on the known covariates but we do think there is an exogenous instrument. The important point is that matching is not without its assumptions, requiring both stronger and weaker assumptions - simply different assumptions - than an IV approach. In particular, matching makes the same assumption as made when simply including controls, merely weighting estimated effects differently.[16] If a valid instrument could be found, it would allow us to avoid making this assumption and provide arobustness check on earlier results.

A second factor that points to the use of an IV rather than matching is the relatively small sample size of conflicts. While the IV approach is not immune to problems stemming from small sample sizes, it does have an advantage in that when matching if one has a particularly small sample the closest match might still be quite far away, whereas an IV approach is more robust, not relying on individual matches. Abadie and Imbens[17] recently highlighted the fact that matching estimators are typically biased due to imperfect matches (and the smaller the sample size, the more likely the matches to be imperfect), noting that this bias can be avoided with a regression-based correction. In the case of evaluating the effectiveness of peacekeepers, we do have a relatively small set of cases and hence, while there is always some debate about how much parametric structure one should impose, the balance of empirical papers points slightly towards more structure.[18]

For these reasons, an instrumental variable approach would be preferable if a valid instrument could be found. The question is whether such an instrument exists. This will be

[15] If the instrument is denoted Z_i, $(Y_{i1}, Y_{i0}) \perp Z_i | X_i$ but $Z_i \not\perp D_i | X_i$.

[16] Many believe matching is unlikely to yield substantially different results from a regression with controls for this reason; *e.g.* see Angrist and Pischke 2009.

[17] 2008.

[18] Angrist and Pischke 2009.

discussed in a later section.

The control function approach, closely related to a standard IV, also has a few additional advantages. When using an IV, or two-stage least squares (2SLS), we regress the endogenous variable, receiving peacekeepers, on an instrument and a set of covariates. We then use the results to predict the values the endogenous variable would take given these regressors and, in the second stage, regress the outcome variable of interest, duration of peace, on the predicted value of the endogenous variable. In a control function approach, we also regress the endogenous variable on an instrument and a set of covariates in the first stage. The difference is that we take the estimated errors from that regression and, in the second stage, regress the outcome variable on the actual value of the endogenous variable, the covariates, and this error term. The intuition behind including the residuals as a control is as follows. The reason endogeneity is a problem is that unobserved confounding factors distort the true relationship between the endogenous variable and the outcome variable. If we could observe all the unobservables and include them as controls, the endogeneity would not be a problem. The control function approach assumes the residuals from the first stage capture the unobservables, so including the residuals as a control is akin to controlling for all unobservables and resolves the endogeneity problem. While one may suspect that the residuals do not truly correctly model the relevant omitted variables, the control function approach at least acknowledges that unobserved relevant variables exist, unlike matching, and hence this method is more robust to omitted variables.[19]

This paper thus builds on and supports the earlier literature by addressing the endogeneity concerns using an instrumental variable and control function approach. If these methods result in similar findings, this would be an important robustness check using an entirely different methodology. The control function approach also allows me to explicitly test for endogeneity and characterize selection. Through these methods I discover that leaders of a country in conflict appear to often not want peacekeepers, a new empirical finding.

[19]Heckman and Navarro-Lozano 2004.

The rest of this paper proceeds as follows. The next section provides background information on U.N. peacekeeping, defining what I mean by U.N. peacekeeping operations and modelling the decision-making process that results in them. I then discuss the instrument in greater depth. Following this, I describe the data, detail the identification strategy, and present the results. Finally, I provide some robustness checks before concluding.

2 Peacekeeping and the U.N. Security Council

2.1 U.N. Peacekeeping Operations

U.N. peacekeepers' role and what they are allowed to do has evolved over time. Traditionally, U.N. peacekeepers acted as a buffer force, physically positioned between combatants following a ceasefire. While not great enough in numbers to prevent determined parties from attacking their opponents, the peacekeepers could prevent isolated or small groups of actors from acting as "spoilers" as well as preventing accidental engagements.[20] In this traditional role they were also sent to observe the carrying out of ceasefire agreements (*e.g.* withdrawals of troops from a specified area) to help detect violations. U.N. personnel also typically engage in diplomatic efforts, such as trying to arrange meetings between the different sides, though this activity may occur without a peacekeeping force. Peacekeeping operations are predicated on a few requirements: the host state must consent to the forces; the forces must maintain impartiality; and the forces can only use minimal force, as defined in the resolutions that established the mission. Typically, this last stipulation allows peacekeepers to use military force only in self-defense.

After the end of the Cold War, U.N. peacekeeping operations began to expand in frequency and also in scope. In 1995, U.N. Secretary-General Boutros Boutros-Ghali described this new form of peacekeeping as "peacebuilding", the "creation of a new environment" that would contribute to lasting peace. Peacebuilding activities include: disarmament, hu-

[20]For historical descriptions of spoilers, see Cochrane 2008.

man rights protection, humanitarian aid and programs to promote economic development, and election supervision. These peacebuilding activites, however, typically follow traditional peacekeeping operations rather than acting as substitutes for them. In this paper, I will define a peacekeeping operation as one in which military observers are sent to the country in conflict. Since there are relatively few cases of each type of peacebuilding, I will not try to evaluate the effectiveness of each type of peacebuilding separately; even apart from the small sample size, there is the concern that the type of peacebuilding activities chosen may be endogenous to the type of conflict that occurred. If as rigorous a method could be used to disaggregate effects by the type of conflict or the type of peacebuilding used, that would be preferable, however, there is a trade-off between disaggregation and robustness, and while papers that do attempt to distinguish between different types of peacebuilding are valuable, so too is a rigorous treatment of the basic question of whether peacekeeping helps at all.

2.2 The U.N. Security Council

U.N. peacekeeping operations come into existence when the U.N. Security Council passes a resolution authorizing them. It is in theory possible for the U.N. General Assembly to pass such resolutions, but the Security Council is the only body with the authority to make binding decisions. Since the Security Council determines whether peacekeepers are sent to a given episode of conflict, it is necessary to understand its structure and decision-making process.

The U.N. Security Council comprises five permanent members - the United States, Russia, the United Kingdom, France, and China - and, since 1966, ten non-permanent (or temporary) members elected for two-year terms, with five of these seats contested each year. The temporary members are chosen by regional groups. The African Group chooses three members; the Group of Latin American and Caribbean States, the Asian Group, and the Western European States and Other States Group[21] each choose two members; and the East-

[21]The "other states" are Canada, Australia, and New Zealand.

ern European Group chooses one member. A Security Council member must be nominated by its group and then receive a two-thirds vote in the U.N. General Assembly. The regional groups try to present a "clean slate" to the General Assembly, with one nominee per seat, however, on average, the U.N. General Assembly faces approximately seven candidates for the five seats up for election each year. Once a temporary member has served its two-year term, it is ineligible for immediate re-election. Seats are at least weakly desirable; thus, it is possible that larger, more influential states that can exert more pressure within their group are nominated more frequently.[22] Indeed, Japan and Brazil are disproportionately nominated. However, apart from these countries, which have particularly strong desires to serve on the Security Council, seats are assigned on more of a rotation system. For example, the African Group abides by a rotation system under which Northern Africa and Central Africa each receive 1 seat every 2 years in alternating succession; Eastern African and Southern Africa also rotate 1 seat every 2 years; and Western Africa receives one seat every 2 years. An Arab state is elected every 2 years, alternating between being from the Asian Group and from Northern Africa. One of Denmark, Norway, Finland and Sweden gets a seat every 4 years, as does one of Canada, Australia and New Zealand. Appendix A provides a list of the years that states in the dataset have served on the Security Council to date.

2.3 The Decision-Making Process of the U.N. Security Council

What determines where peacekeepers are sent? I discuss three factors that affect the likelihood that episodes of conflict receive peacekeepers - membership on the Security Council, the Cold War, and characteristics of the episodes of conflict - and the process under which decisions are made.

Permanent members of the Security Council have long been known to influence the selection of conflicts to receive peacekeepers. For example, in 1999, Russia intimated that it would veto any measure to end the civil war in Kosovo, and it was always "out of the

[22]Malone 2000.

question" that Russia would allow a peacekeeping operation in Chechnya.[23] China also used its veto power in 1997 to delay a U.N. mission in Guatemala, since Guatemala had been too close with Taiwan.[24]

But do temporary members, which do not possess veto power, also influence which conflicts receive U.N. peacekeepers? Intuitively, the answer would seem to be "no". However, the Security Council prefers to obtain consensus among its members, evidenced by its track record of pursuing "lowest common denominator" policies.[25] Observers have even written that in practice, due to this preference for consensus, the temporary members appear to contribute substantially to Security Council decisions.[26] Further, each member "puts its national interests ahead of any collective security interests."[27] It has been shown that conflicts in which no state has an interest tend not to receive attention;[28] it is only one step from there to the conclusion that states may also be able to block action in their own countries if they so chose. Indeed, we will see that in practice, when a country is a temporary member of the Security Council, it is rarely sent peacekeepers.

The Cold War and characteristics of conflicts also influence which are sent peacekeepers. Since the U.S. and the U.S.S.R. supported conflict in "proxy wars" during the Cold War, and since both the U.S. and the U.S.S.R. possessed vetoes on the U.N. Security Council, it is not surprising that U.N. peacekeepers were rarely sent before the end of the Cold War. Other studies have also suggested that the Security Council's likelihood of sending peacekeepers is dependent on episode characteristics such as the number of deaths caused by the conflict or the duration of the conflict.[29]

When a security issue is raised, the Security Council has several tools at its disposal to try to get the sides to negotiate a diplomatic solution. The strongest action it can take is

[23]Dunbabin 2008.
[24]Cockayne, Mikulaschek and Perry 2010.
[25]Lowe *et al.* 2008.
[26]For example, Mahbubani 2004 provides an excellent account from the persepctive of a permanent representative.
[27]Mahbubani 2004, p. 263.
[28]Ibid.
[29]Fortna 2004.

to pass a resolution, which requires nine affirmative votes and no permanent member veto. Resolutions can authorize a peacekeeping operation (PKO) if it is felt the situation calls for one. For such a resolution to be passed, however, the state(s) in conflict must agree to the peacekeeping operation.[30]

In sum, the U.N. Security Council's decision-making process can be modelled as in Figure 1.[31]

This paper will exploit the fact that a country is less likely to receive peacekeepers if it is serving on the Security Council at the time of the conflict to evaluate the effectiveness of U.N. peacekeeping. The validity of this instrument will be discussed in detail in the next section.

3 Justification of the Instrument

To be a good instrument, a variable has to meet two criteria. First, it has to be correlated with the treatment that is feared to be endogenous; this is known as the relevance criterion. In our case, this means our potential instrument has to be correlated with being or not being sent peacekeepers. Second, an instrument must also not be directly correlated with the outcome variable of instrument - in our case, the duration of peace. This is the exclusion criterion. In this section I will discuss how temporary membership in the Security Council meets these two criteria.

Let us first consider whether temporary membership on the Security Council is associated with peacekeeping. It is well accepted that a U.N. peacekeeping mission would not be authorized when a permanent member of the Security Council is involved in the conflict. The data clearly reflect this, with no U.N. peacekeeping mission ever sent when a permanent

[30]While it is true that any state can in principle refuse peacekeepers, states in practice often yield to pressure. To serve as an instrument, temporary membership on the Security Council merely needs to decrease this probability of yielding.

[31]Ideally, one would also be able to look at Security Council votes and use close votes to get further plausibly exogenous variation in the likelihood of being sent peacekeepers. I cannot do this here because no votes are close; resolutions will typically not be suggested unless it is known that they will pass or unless it is meant as a form of protest.

member is directly involved. This makes sense from a theoretical standpoint, since permanent members wield a large amount of power and could block such a measure. However, temporary membership on the Security Council is also significantly associated in the data with not being sent peacekeepers, as evident in Table 1. Despite their more limited power, temporary members still seem to exert extra influence when compared to countries not on the Security Council. This is well-established in the literature, for example with regards to the increased aid flows countries receive when members of the Security Council, as countries not on the Security Council try to buy their votes.[32] Case studies reach a similar conclusion about the importance of the membership of the Security Council.[33] While it is true that temporary members theoretically and practically have less say in the decisions of the Security Council than permanent members, the additional power they have relative to countries not at that time on the Security Council is all that is needed to ensure the instrument meets the relevance criterion. It is interesting to consider why it is the case that temporary membership on the Security Council is associated with not receiving peacekeepers. There are many possibilities and a full discussion could fill a separate paper, but for temporary membership to satisfy the relevance criterion it is enough to show the fact that this correlation exists.

It is more difficult to show that the proposed instrument satisfies the exclusion criterion. This criterion requires that temporary membership be unrelated to the duration of peace except through changing the probability of being sent peacekeepers. In the rest of this section I discuss several possible ways in which those making it onto the Security Council could be different, in turn.

First, it is possible that those countries that are in conflict are less likely to receive temporary membership on the Security Council. Table 2 shows the results of a logistic regression of temporary membership on whether the country was in conflict at the time, according to the UCDP/PRIO dataset with a 25-death threshold, the UCDP/PRIO dataset using a 1,000-death threshold to define episodes of conflict, and the Doyle and Sambanis (2006) civil

[32]Kuziemko and Werker 2006.
[33]Malone 1998.

war dataset. The results are not as strong as one might expect. Only if one considers results significant at the 10% level, it is slightly less likely for countries in any conflict with at least 25 deaths in a year to be on the Security Council that year (Column 1), quite possibly a fluke given neither those meeting the 1,000 deaths criterion in the UCDP/PRIO dataset (Column 2) nor in the Doyle and Sambanis dataset (Column 3) are less likely to be on the Security Council.

In fact, even if the timing of temporary Security Council membership is endogenous to the risk of conflict, this would not, by itself, invalidate the instrument. It is the kind of conflict that matters - whether countries with "easier" or "harder" conflicts are differentially selected to the Security Council. Column 4 of Table 2 shows that if we restrict attention to countries in some kind of conflict, those which are in more intense conflicts, which meet the 1,000-death criterion as opposed to merely the 25-death criterion, are again no less likely to become members of the Security Council during the time of the conflict. Not only is the effect insignificant, but the point estimate is very small in magnitude.

This is a bit surprising on the face of it but could partially be explained by the fact that if Security Council membership depends on the last time one was on the Security Council, countries may have their hands somewhat tied in seeking entrance to the Security Council, regardless of the intensity of their conflict.

The intensity of the conflict is not the only characteristic of a conflict, however, which could affect the duration of peace after the conflict has ended. It is possible, for example, that temporary membership on the Security Council partially reflects preferences in the UN Secretariat and other Security Council members to deploy peacekeepers in a particular conflict. Under this hypothesis, if members wanted to send peacekeepers to a country, they would prevent that country from joining the Security Council during the conflict. The preferences themselves would not improve the duration of peace after the end of the conflict, but they may be related to factors that do. To mitigate this concern, we can consider which factors might be related and check whether being on the Security Council during a conflict is

associated with these factors and, if so, whether they are associated with duration of peace.

Apart from the intensity of conflict, other factors that may influence preferences to send peacekeepers include the population, wealth, regime type, political connections such as having been a former colony, and trade of the country in question. The relation of each of these factors with temporary membership on the Security Council during conflict is investigated in Tables 3-5. Results suggest that countries that are larger are significantly more likely to become members of the Security Council, however, measures of democracy, autocracy, and the Polity IV index, as well as whether one was a former colony and one's exports, has no significant effect on whether one becomes a member of the Security Council during a conflict, whether broadly defined using the 25 battle deaths criterion to retain more observations, as in Table 3, or whether narrowly defined using the 1,000 deaths criterion, with lower power, as in Table 4. Indeed, even when considering the full dataset of countries which are at one time or another in conflict, regardless of whether or not they are in any conflict at the time, only political regime type (autocracy, Polity IV index measure) exhibits even a weak relationship to Security Council membership (Table 5).

These variables may not fully capture economic and political factors influencing whether or not a country joins the Security Council. To mitigate this concern, in later regressions I control for the total number of years a country spends on the Security Council regardless of the timing of the conflict. Thus, if a country is particularly influential due to its population, wealth, or political connections, among other factors, and regularly becomes a member of the Security Council, any relationship between this country's influence and the duration of peace will be reflected in this control variable and not the instrument. This increases the probability that the instrument captures the relationship between duration of peace and being on the Security Council during a conflict and not the relationship between duration of peace and being on the Security Council generally, exploiting changes of Security Council status over time. Those countries that had been on the Security Council in the immediate past may be less likely to join the Security Council for a few years, so I separate this variable

into two rolling windows: the number of times one had been on the Security Council in the last five years (which one might expect to be negatively associated with Security Council membership) and the number of times one had been on the Security Council in the last ten years (which one might expect to be positively associated with Security Council membership). I would also use longer time periods, but the data is quite short as it is, focusing on the post-1980 period when there were more UN peacekeeping missions in order to have a chance of a strong instrument, and with countries being dropped through the use of these windows if they did not exist as sovereign states throughout the course of the entire window.

Another concern is that perhaps countries that join the Security Council then begin to behave differently. These countries, not receiving peacekeepers, would have to change in such a way so as to reduce the prospects of peace in order for the finding that receiving peacekeepers increases the duration of peace to be a spurious artefact. One possibility is that the economic and military aid that Security Council members are known to receive could make the battle more lopsided. Yet lopsided conflicts have been associated with longer durations of peace in the literature (*e.g.* Fortna 2004), causing the bias to if anything work against finding a positive effect of peacekeeping. In my regressions, I include controls for economic and military aid, regardless, for robustness.

One might also think that some countries may be more or less likely to be able to mobilize opposition to a PKO as temporary members of the Security Council based on their resources and alliances. This is not a concern for the instrument, since even if this heterogeneity existed, even the weakest countries seem firmly able to prevent receiving a PKO.

It should be noted that there is only one case in which there is peacekeeping while the receiving country is a member of the Security Council: that of Rwanda, a very difficult case. There were very serious constraints on the UN mission to Rwanda, as several countries lost their appetite for peacekeeping in the wake of the US troop deaths in Somalia, and others who did contribute troops were quicker to withdraw them than they might otherwise have been. Still, the fact that a mission was sent despite the obstacles does suggest there are

incentives to send peacekeepers to the worst cases. While it is possible that the instrument, by virtue of *de facto* counting Rwanda as "not having received peacekeepers", would thereby bias results in the direction of showing an effect of peacekeeping, this is only one case, so it should play a relatively minor role.

Despite the regression results and additional controls and considerations already discussed, it is still possible that being on the Security Council during a conflict (as opposed to at any other time) is directly related to the duration of peace. For example, we might expect that at a given point in time, conditional on being in conflict, countries with *lesser* conflicts would be more likely to be selected to serve on the Security Council than those with *greater* conflicts, where the definition of "lesser" could span a variety of dimensions. These conflicts, by virtue of being "lesser" in some way - perhaps in number of deaths or in duration - could also be less likely to receive peacekeepers, if we believe peacekeepers are more likely to be sent to major conflicts.

In order for the exclusion criterion to be violated, the instrument must affect the outcome variable in some way other than through changing the probability of being sent peacekeepers. If temporary membership on the Security Council during a conflict is associated with having a lesser conflict and having a lesser conflict is itself associated with duration of peace, this poses a problem. One way I address this concern is by limiting the ways a conflict could be considered "lesser". We have already seen that countries in conflicts which meet a higher threshold of battle deaths are not significantly less likely to be on the Security Council than those with fewer deaths. Still, I include both the number of deaths and the duration of the conflict as controls. Thus, if an episode of conflict is somehow less complicated or weaker than others, it would have to be weaker in some way that is not captured by deaths or the duration of the episode in order to violate the exclusion criterion. It remains possible that countries in conflict which make it onto the Security Council are "lesser" in some way not captured by these variables. However, it is not clear in what way this would be, and that results are robust to different specifications and datasets would suggest that further controls

would yield similar results. I cannot control for political factors such as the Polity IV measures directly, since this would reduce the already-small sample size.

More importantly, these not-otherwise-captured "lesser" conflicts would have to be correlated in a particular direction with duration of peace in order to falsely drive results. Namely, these conflicts would have to both be less likely to receive peacekeepers and have a shorter duration of peace, a requirement that may clash with intuition. Other authors have argued that any bias caused by UN peacekeepers responding to the hardest caess would serve to work against finding a positive effect for peacekeeping, and the same argument applies here (Doyle and Sambanis 2000; Fortna 2004).

In summary, this discussion shows that the instrument satisfies the relevance criterion and provides evidence that it satisfies the exclusion criterion. Though the satisfaction of the exclusion criterion cannot be proven beyond all doubt as in the case of the relevance criterion, it should be noted it is always difficult to prove the absence of an effect. Instead, this discussion has aimed to show that the instrument is plausible, and results based on it can offer supporting evidence about the effects of peacekeeping from a new angle.

Having discussed the instrument in more depth, I will now turn to describe the data.

4 Data

For this analysis, I use the UCDP/PRIO Armed Conflict version 4[34] and Battle Deaths version 5[35] datasets which include both civil wars and interstate conflicts, though as stated I limit my attention in this paper to civil wars as there are many more of them in the data. Conflict is defined in the dataset as "a contested incompatibility that concerns government and/or territory where the use of armed force between parties, of which at least one is the government of a state, results in at least 1,000 battle-related deaths".[36] The dataset further divides the conflicts into episodes, where an episode is said to have ended if the number of

[34] 2009.
[35] 2014.
[36] Gleditsch *et al.* 2002.

battle deaths falls below 1,000 for at least one year. The episode is said to reoccur if in a subsequent year the threshold of 1,000 battle-related deaths is again reached in the same conflict as coded by UCDP/PRIO. It could be noted the data also contain a 25 battle-related deaths measure; utilizing it would preserve more observations, but at the cost of examining a different question than civil war. While the 1,000 deaths rule is artificial, some such rule must be chosen. This rule does appear to set a good bar since when it is used not many conflicts flicker in and out of existence, suggesting that episodes' entry or exit from the dataset is meaningful. The UCDP/PRIO dataset contains relatively more distinct episodes of conflict than the Doyle and Sambanis (2006) dataset, but I also use the latter for robustness checks.

I use the UCDP/PRIO episodes as the main unit of analysis and add a variable indicating whether or not a peacekeeping operation was sent. To do this, I match peacekeeping operations described by the U.N.[37] with the episodes of conflict in the UCDP/PRIO Armed Conflict dataset based on the following information provided in the latter: the location of the conflict, the sides involved, the start and end date of the episode of conflict, the original start date of the conflict if it has stopped and resumed, and UCDP/PRIO's own coding of which episodes of conflict were considered to be continuations of the same conflict. It should be noted that since peacekeepers are occasionally sent to a situation that does not qualify as an episode of conflict in my dataset, those peacekeeping operations are excluded. The Doyle and Sambanis data come with peacekeeping operations already coded.

For each episode, I also code how many years the main country in which the episode took place was a temporary member of the U.N. Security Council during that episode.[38] Additional variables code how many years in the preceding 5 and 10 years, respectively, the country was a temporary member of the U.N. Security Council. These will serve as controls, and the reason I code them separately is because one might expect them to have opposite effects on the likelihood of being on the Security Council during an episode and therefore on receiving peacekeepers. The logic is that some countries more frequently serve on the

[37]United Nations 2011b.

[38]The history of Security Council membership is given by United Nations 2011a.

Security Council due to size and influence, and this would be picked up by how often the country served in the last 10 years.[39] On the other hand, a country is less likely to serve if it has recently been on the Security Council, for example in the last 5 years.

Since I am interested in the effects of U.N. peacekeeping in the absence of other military interventions, I also code whether or not the CIA or KGB were involved in the conflict, using as sources Blum[40], Andrew and Mitrokhin[41], and Weiner[42], following Easterly, Satyanath and Berger.[43] I further identify and exclude conflicts which received peacekeepers from other international organizations: the AU, NATO and the OAS.[44] Finally, I code whether or not a permanent member of the U.N. Security Council was directly involved in the conflict, following Fortna.[45] Since no U.N. peacekeepers are sent when a permanent member of the Security Council is involved in the conflict in my dataset, I exclude these cases to make my instrument stronger. To help disaggregate effects later, I also code whether the peacekeepers arrived after a treaty following Fortna[46] and whether they arrived with a chronological gap after the end of the battle-related deaths.[47]

This paper deals with intrastate conflicts only. Very few episodes of conflict in the UCDP/PRIO data are of interstate conflicts, so I cannot evaluate the effects of peacekeeping on this sample separately.

Data on U.S. economic and military aid for each country and year is from the Greenbook compiled by USAID as in Kuziemko and Werker[48]. Whether a country was a former colony

[39]The reason I do not go farther back is that once I control for a country's influence 10 years ago that country's influence 20 years ago is largely irrelevant; also, if I were to go back much further I would introduce bias since some countries did not exist that many years before the start of my analysis.

[40]2004.

[41]2005.

[42]2007.

[43]2008.

[44]Coded using Council on Foreign Relations 2009, NATO 2011, and OAS 2011, respectively.

[45]2004.

[46]2004

[47]It should be noted this second measure is less reliable since we might not observe such a gap if the conflict resumed, depending on the timing of its resumption. For example, a conflict could appear to have ended in Dec. 1995; if it resumed in Jan. 1996 we would not observe a break even if there was a short break in the battle-related deaths.

[48]2006.

is also sometimes included as a control; this was hand-coded using the 2010 Encyclopaedia Britannica. Countries are not included in the dataset before independence. Data on GDP is from the Penn World Tables.[49] Finally, I add Polity IV data on regime types.[50]

It may be helpful to look at some summary statistics describing the data in more detail. Table 1 breaks the episodes of conflict into cells based on whether peacekeepers were sent and whether the country in conflict was a member of the Security Council during that episode of conflict.

As is evident from the table, peacekeepers were only sent to one episode of conflict in a state that was a member of the Security Council during that episode.[51] Episodes of conflict which did not receive peacekeepers and which were in states not on the Security Council during the episode appear to have been the "lightest" conflicts, judging by episode duration and the average low estimate of battle deaths. This accords with the literature that peacekeepers are usually sent to worse conflicts.[52] Interestingly, however, by the same measures, the episodes of conflict in states that were on the Security Council during the episode but did not receive peacekeepers seem to have been much worse than the episodes of conflict in states that were not on the Security Council that did receive peacekeepers. This provides some first suggestive evidence that temporary membership on the Security Council does allow states to "escape" being sent peacekeepers even when their conflicts are arguably "worse" and thus would otherwise be more likely to receive peacekeepers.

[49] I use the real GDP per capita derived from growth rates of domestic absorption.

[50] 2013.

[51] This would be Rwanda's civil war in the early 1990s; however, peacekeepers were authorized in 1993 and Rwanda did not join the Security Council until 1994. In a sense, it thus should not count. The fact that, within an episode of conflict, a country might have served on the Security Council earlier or later in the conflict could add a second layer of analysis; perhaps, for example, Security Council membership is most influential in determining whether peacekeepers are sent early in the conflict. However, if we imagine that for each year that the conflict continues, there remains a chance that peacekeepers will be sent, it is not necessary to include when exactly a country serves on the Security Council in the identification strategy, and any effects could be expected to be second-order.

[52] Fortna 2004.

5 Identification Strategy

As discussed, being a temporary member of the U.N. Security Council seems to affect the likelihood of being sent peacekeepers. The basic empirical strategy of this paper is to use this plausibly exogenous variation in the likelihood of being sent peacekeepers to evaluate the effects of peacekeeping. In my main regressions I use both two stage least squares (2SLS) and a control function approach. In each, I estimate how the likelihood of receiving peacekeepers depends on other characteristics with the following first stage:

$$P_e = \alpha + \beta_1 SC_e + \beta_2 Z_e^p + \epsilon_e \tag{1}$$

where P is a binary variable indicating whether U.N. peacekeepers were sent to the episode, e is the episode of conflict, SC is how many years the country was a temporary member of the Security Council during that episode of conflict, Z^p are controls, including how many years the episode of conflict lasted, the year the episode of conflict began, the low estimate of how many deaths occurred in the last year of the episode from the UCDP/PRIO Battle Deaths dataset[53], and other controls depending on the specification, and ϵ is an error term. Since U.N. peacekeepers were largely sent only after the end of the Cold War, I truncate my sample to those that ended after 1980 so as to have a reasonably strong instrument; I would restrict focus to even later cases but there is clearly a trade-off as I do not want to overly limit sample size.

In the 2SLS regressions, equation (1) is estimated by an ordinary least squares regression. Any nonlinear model, such as would be specified by a probit or logit regression, would yield inconsistent estimates if used as the first stage of a 2SLS regression.[54] With the control function approach, I can instead choose to estimate equation (1) using a probit. 2SLS and the control function approach numerically coincide in the case in which the first stage is

[53]Other estimates of deaths could be used, but the best guess and high estimates of deaths have more noise and the deaths in the last year of the conflict seem to have the most explanatory power.

[54]Hausman 1983.

estimated using ordinary least squares and there is only one instrument. I will thus continue my exposition in terms of the control function approach, as it can be more flexibly applied with, for example, a probit in the first stage and a censored regression model in the second stage, though I also use the control function approach in its incarnation as an instrumental variables or 2SLS regression.

The second stage regressions on the duration of peace are then represented by the following equation:

$$D_e^p = \alpha + \gamma_1 P_e + \gamma_2 Z_e^p + \gamma_3 Resid_e^p + \epsilon_e \tag{2}$$

where D^p is the duration of peace after peace has been obtained and $Resid^p$ are the residuals. Including the residuals as a control both addresses the endogeneity and also tests whether the endogeneity was an issue. If the residual term is significant, there was selection on the variables included in the first stage, and not accounting for this selection would bias results.

There are three possible types of cases in the data: the case in which peacekeepers are sent but there is no peace, so the duration of peace is 0; the case in which peacekeepers are sent and there is a positive duration of peace before the conflict resumes; and the case in which peacekeepers arrive and the conflict does not re-occur. In this last case, the duration of peace is right-censored, as the peace has been maintained to date but we do not know how long it will continue to last. Tobit regressions are designed to account for censoring, though one must specify the cut-off threshold. A flexible form of the Tobit model, which allows for observation-by-observation cut-off points, is possible to estimate and will be used here in the second stage as a robustness check to the instrumental variable regressions. Several different marginal effects can be estimated, such as the effect of the unobserved, uncensored latent variable, the effect on the observed, censored variable, the probability of being uncensored, etc. The coefficients reported in the regressions presented here should be interpreted as providing an estimate of how the uncensored variable duration of peace changes with the presence of peacekeepers, as though it were not censored.

We are also interested in whether peacekeeping helps prevent conflicts from ever reoc-

curring. Of course, we cannot know whether a conflict will ever reoccur, so instead I look at the distribution of how many years it takes conflicts that do reoccur to reoccur. The plots in Figures 2 and 3, based on the 1,000-deaths criterion, show that if a conflict reoccurs, it will likely reoccur within the 10 years after the end of the conflict and will be especially prone to reoccur within 5 years, though this is far from a guarantee. If the estimates obtained in the second stage show that peacekeepers increase the duration of peace by many years, this would then provide suggestive evidence that peacekeepers can help keep the peace long enough to reduce the risk that countries fall back into conflict.[55]

6 Estimating the Effects of U.N. Peacekeeping

In Table 6, we see that the proposed instrument, the number of years a country is on the Security Council during an episode of conflict, has the expected mechanical relationship with the duration of the episode. Other covariates are not significant.

When one does not consider the endogeneity of peacekeeping, U.N. peacekeeping does not seem to have a significant effect on the duration of peace, regardless of whether one uses OLS or a censored regression model (Tables 7 and 10). Yet using the number of years a country is on the Security Council during an episode as an instrument, we see that peacekeeping increases the duration of peace after an episode of conflict has ended (Table 8).

As discussed, it is possible that which countries get elected to the Security Council is dependent on country influence and this has an effect on peace itself. I thus use an alternative specification which includes not just how many years the country was a temporary member during the episode of conflict but also how many years it was a temporary member leading up to the episode of conflict. This specification helps quell fears that the temporary

[55]We might consider the alternative of using a probit in the second stage, with the outcome variable taking the value of 1 if the conflict has not yet reoccurred and 0 if it has, to seek to estimate the determinants of a conflict ever re-occurring. This is problematic. There are as yet few good ways to directly deal with problems that require a probit in both the first and second stage (Wooldridge 2002, 2007). Further, treating the second stage as a probit would be discarding information relative to keeping the (albeit censored) lengths of time that peace has endured to date. Thus, the censored regression model is better suited to address this issue.

members of the U.N. Security Council are special in some way that is directly correlated with the outcome variables, as was discussed at length in an earlier section. The results are lent further credence by the fact that, while the assignment of seats is not random, states exogenously leave the Council since they cannot immediately be re-elected. Also, once a less prominent country has served, it is unlikely to serve again until most of the other less prominent countries in its regional group have served. Thus, if a country in conflict wanted to get on the U.N. Security Council to prevent peacekeepers from being sent, it would have difficulty in doing so if it had served before, and the possibility of later being in conflict and needing to be on the Security Council is unlikely to have entered into its previous decision to serve.

Since an earlier study showed that U.N. Security Council membership is associated with an increase in aid[56], I also include aid as a control to verify that the U.N. Security Council's effect on our outcome variables is not through aid rather than through the absence of peacekeepers.

As previously discussed, when using the 1,000 battle deaths criterion, there are not that many distinct episodes of conflict, and adding controls further reduces this due to missing data. Throughout, my preferred specification is that in Column 2, as it contains the important controls for Security Council membership in the last 5 or 10 years yet preserves enough observations that the instrument tends to have the highest F statistics in the first stage across the different slices of data (whether or not a treaty was signed; after a gap in fighting; and the Doyle and Sambanis data).

Finally, to guard against weak instrument concerns, I report the Anderson-Rubin Wald and the Stock-Wright LM test statistics for the IV regressions. These statistics provide adjusted p-values for the significance of the potential endogenous variable (in our case, receiving peacekeepers) that are robust in the presence of weak instruments.

The censored regression models in Tables 10 and 11, respectively representing regressions

[56]Kuziemko and Werker 2006.

without and with the control function approach, tell a similar story as in the OLS and IV regressions: in the regressions that explicitly include the generalized residual as a control, the residual tends to be significant, again indicating that endogeneity is a concern, and PKOs exhibit a more positive relationship with the duration of peace.

The regressions in Tables 8 and 11 illustrate three things.

First, they reinforce the idea that endogeneity is a problem in peacekeeping.

Second, the control function approach allows me to characterize the selection. A negative coefficient on the residual in the censored regression model suggests that the cases which are more likely to be selected to receive peacekeepers are also the cases that have lower values of the dependent variable, duration of peace; *i.e.* the cases which are more likely to be selected are those in which peace is more likely to fail. While it has previously been noted that cases which are "harder" along certain dimensions seem more likely to receive peacekeepers, the control function approach provides a direct test of this hypothesis. The magnitudes of the effects of peacekeepers are also notably larger than those found in Fortna.[57]

Finally, and perhaps most interestingly, one implication of these results is that the leaders of countries that have fallen into conflict appear to want to avoid being sent peacekeepers. A full discussion of the reasons why this may be the case is beyond the scope of this paper, but some possibilities are that governments fear that intervention is not likely to be of much use or would cause more damage, *e.g.* through worsening the country's reputation domestically or abroad. Further, it should be noted that the incentives of government leaders do not necessarily align with those of citizens. At minimum, government leaders may not bear all the costs of fighting; in the worst case, as in Chiozza and Goemans,[58] war could even be *ex post* efficient for leaders.

The empirical finding that governments do not want peacekeepers opens up many new avenues for further research. Are governments more willing to receive peacekeepers if they are losing the conflict? What are the true costs of peacekeeping to the recipient country, who

[57] 2004.
[58] 2004.

bears those costs within the country, and how can they be lowered? Doyle and Sambanis[59] found that peacekeepers also encourage democratization; is part of why governments want to avoid peacekeepers a lessened ability to extract rents?

I run the main OLS and IV regressions on a few other datasets for robustness, though each of these reduces the sample and thus weakens the instrument. If we only consider PKOs that began after a treaty was signed or which began after a gap in hostilities (the limitations of which were previously discussed), or when using the Doyle and Sambanis dataset, the OLS results are slightly more promising (Tables 12, 15 and 19 in the Appendix). The IV results still regularly yield higher values for the effects of peacekeepers on the subsequent duration of peace (Tables 13, 16 and 20). Given the weak instrument concerns, Column 2 is still the preferred specification in Tables 13, 14, 16 and 17. In the Doyle and Sambanis data, there are even fewer unique episodes meeting the criteria and the instrument is even weaker. The column with the strongest instrument judging by the F statistic is actually Column 3, which also is the only specification with a significant result in the second stage. However, while these results are presented, it would seem preferable to stick to the larger UCDP/PRIO dataset.

7 Conclusion

This paper estimates the effects of peacekeeping on the duration of peace after peace has been obtained, using plausibly exogenous variation in the likelihood of being sent peacekeepers. In particular, the longer countries in conflict serve as temporary members of the Security Council during the conflict itself, the less likely they are to be sent peacekeepers, even after controlling for previous service on the Security Council, and this relationship does not go away after controlling for various measures of the intensity of the conflict. While it is still possible that this reflects something about the conflicts themselves, with only those countries in lighter conflicts able to serve on the Security Council, if we believe that worse

[59] 2000.

conflicts lead to generally more fragile peace[60] this would create bias in the opposite direction of my findings. The results suggest that U.N. peacekeepers do indeed prolong the peace once peace has been obtained and lower the chance that another episode of the same conflict will ever reoccur.

The control function approach further told us that the greater the chance of being selected to receive peacekeepers, the shorter the duration of peace. Thus, when peacekeepers increase the duration of peace and lower the chance that another episode of the same conflict will ever reoccur, they do so for the worst conflicts.

Finally, and perhaps most interestingly, this paper reveals that the leaders of countries in conflict often do not want to receive peacekeepers. This empirical finding adds to the literature and calls for further research. In this way, this paper opens the door for many more theoretical investigations in international relations. Why are governments avoiding peacekeepers and if peacekeepers do help, should intervention be more strongly encouraged? What can be done to reduce the barriers or drawbacks to receiving peacekeepers? Do the barriers differ depending on which international organization is involved in the peacekeeping mission?

In summary, these methods provide evidence from a completely new angle that supports the effectiveness of peacekeeping operations and that suggests that governments can be a hindrance to peace. A lot of heterogeneity in outcomes remains which can be modelled in future work. However, overall the results are encouraging because they suggest that even relatively small peacekeeping forces can often help countries avoid or escape from potential conflict traps - if governments will let them.

[60] Doyle and Sambanis 2000; Fortna 2004.

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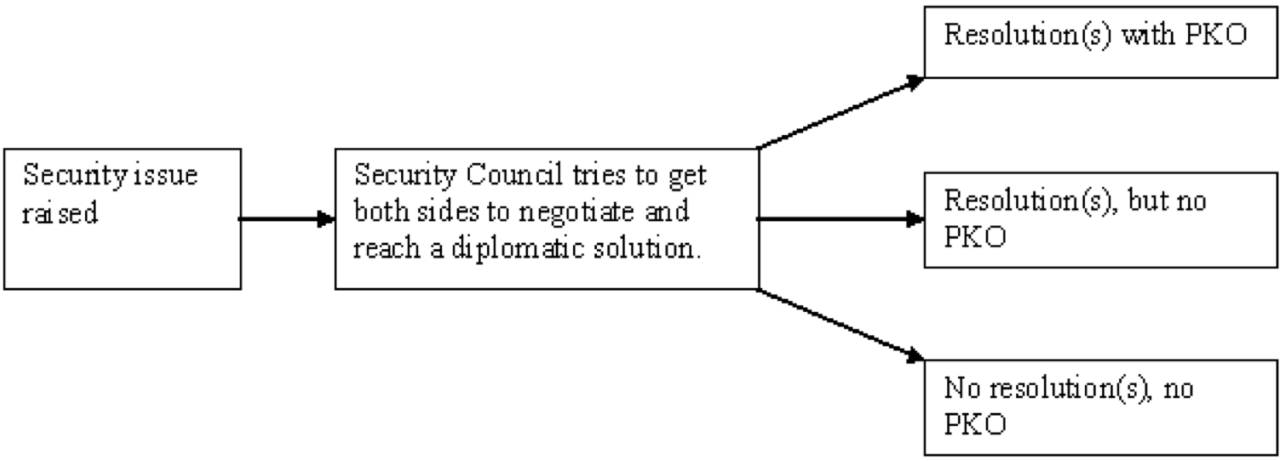

Figure 1: U.N. Security Council Decision-Making Process

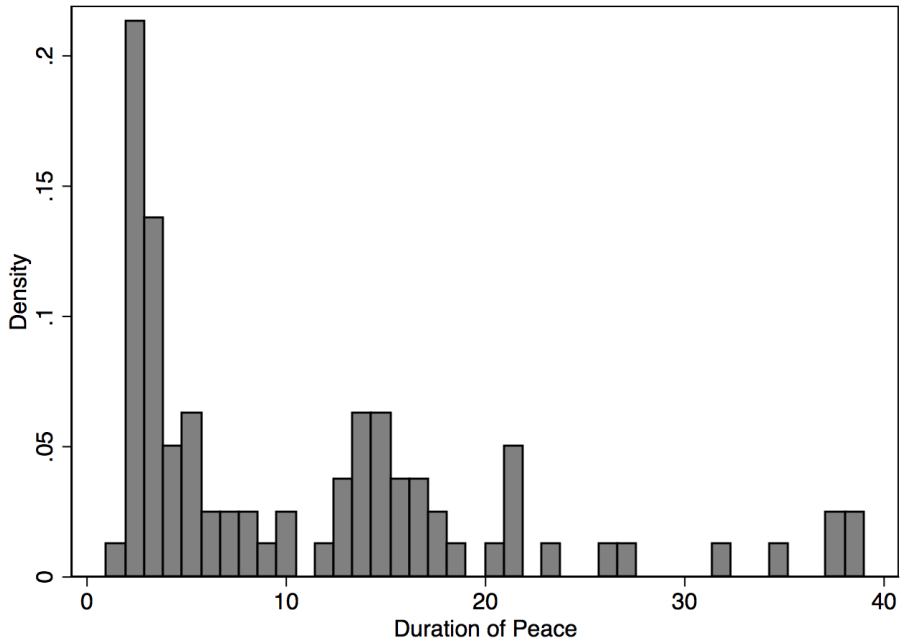

Figure 2: Duration of Peace Among Conflicts that Re-Occur

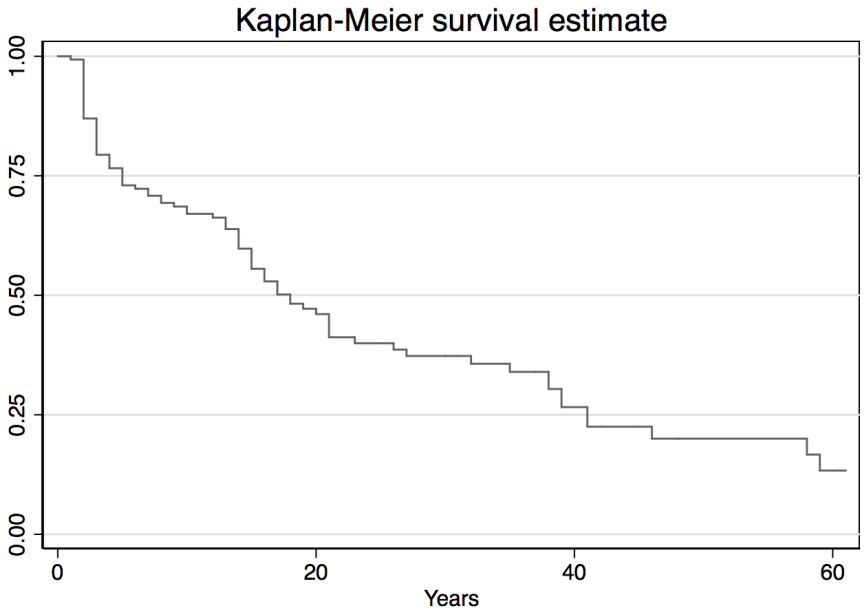

Figure 3: Kaplan-Meier Survival Estimate

Table 1: Summary Statistics

	Received peacekeepers, on S.C. during episode	Received peacekeepers, not on S.C. during episode
Observations	1	18
Ave. Episode Duration	4.0	15.0
Ave. Annual Battle Deaths	2741	5473
Ave. GDP/capita (real USD) 476	1710	
Ave. Population (millions)	5.6	298.3
	Did not receive peacekeepers, on S.C. during episode	Did not receive peacekeepers, not on S.C. during episode
Observations	11	45
Ave. Episode Duration	10.9	2.9
Ave. Annual Battle Deaths	5908	4295
Ave. GDP/capita (real USD)	4847	2618
Ave. Population (millions)	23.5	72.3

Here "annual battle deaths" uses the low estimate of battle deaths from the UCDP/PRIO Battle Deaths dataset; "GDP/capita" and "population" are from the Penn World Tables.

Table 2: Relationship Between Temporary Membership on the Security Council and Conflict Status

	(1) Temp. S.C. b/se	(2) Temp. S.C. b/se	(3) Temp. S.C. b/se	(4) Temp. S.C. b/se
Conflict (>25 Battle Deaths)	0.681* (0.15)			
Conflict (>1,000 Battle Deaths)		0.706 (0.22)	0.969 (0.31)	0.969 (0.33)
Observations	4887	4887	1188	5016

Exponentiated coefficients

Columns 1-3 are based on the UCDP/PRIO dataset; Column 4 is based on the Doyle and Sambanis dataset. Column 3 restricts attention to episodes of any sort of conflict; the coefficient thus represents the difference in Security Council membership between those with more severe conflicts that had reached the 1,000 battle deaths criterion and those that had reached only the 25 battle deaths criterion.

Table 3: Relationship Between Temporary Membership on the Security Council and Country Characteristics (>25 Deaths)

	(1) Temp. S.C. b/se	(2) Temp. S.C. b/se	(3) Temp. S.C. b/se	(4) Temp. S.C. b/se	(5) Temp. S.C. b/se	(6) Temp. S.C. b/se	(7) Temp. S.C. b/se
Temp. S.C.							
Population (in millions)	1.002***						
	(0.00)						
GDP per Capita (real USD, thousands)		0.948					
		(0.06)					
Democracy Index (Polity IV)			0.998				
			(0.07)				
Autocracy Index (Polity IV)				0.993			
				(0.07)			
Polity IV Index					1.001		
					(0.04)		
Former Colony						1.191	
						(0.58)	
Exports (real USD, billions)							0.997
							(0.00)
Observations	955	955	410	410	410	1162	955

Exponentiated coefficients

Table 4: Relationship Between Temporary Membership on the Security Council and Country Characteristics (>1,000 Deaths)

	(1) Temp. S.C. b/se	(2) Temp. S.C. b/se	(3) Temp. S.C. b/se	(4) Temp. S.C. b/se	(5) Temp. S.C. b/se	(6) Temp. S.C. b/se	(7) Temp. S.C. b/se
Temp. S.C.							
Population (in millions)	1.001***						
	(0.00)						
GDP per Capita (real USD, thousands)		1.019					
		(0.15)					
Democracy Index (Polity IV)			1.098				
			(0.07)				
Autocracy Index (Polity IV)				0.884			
				(0.08)			
Polity IV Index					1.057		
					(0.04)		
Former Colony						0.685	
						(0.45)	
Exports (real USD, billions)							0.996
							(0.01)
Observations	260	260	87	87	87	388	260

Exponentiated coefficients

Table 5: Relationship Between Temporary Membership on the Security Council and Country Characteristics (Any or No Deaths)

	(1) Temp. S.C. b/se	(2) Temp. S.C. b/se	(3) Temp. S.C. b/se	(4) Temp. S.C. b/se	(5) Temp. S.C. b/se	(6) Temp. S.C. b/se	(7) Temp. S.C. b/se
Temp. S.C.							
Population (in millions)	1.002***						
	(0.00)						
GDP per Capita (real USD, thousands)		0.948					
		(0.06)					
Democracy Index (Polity IV)			1.050				
			(0.03)				
Autocracy Index (Polity IV)				0.942*			
				(0.03)			
Polity IV Index					1.029*		
					(0.02)		
Former Colony						1.392	
						(0.34)	
Exports (real USD, billions)							1.002
							(0.00)
Observations	3920	955	1781	1781	1781	4776	3920

Exponentiated coefficients

Table 6: Poisson Regression of Years on Security Council During an Episode on Controls

	(1) Years on Security Council During Episode b/se
Battle Deaths	0.876
(thousands)	(0.10)
Episode Duration	1.200***
(years)	(0.04)
Year Episode Ended	1.001
	(0.05)
Years on Security	1.956
Council in Last 5 Years	(0.91)
Years on Security	1.402
Council in Last 10 Years	(0.44)
Non-Military Aid	2.548
(real USD, billions)	(5.76)
Military Aid (real	0.036
USD, billions)	(0.12)
GDP per Capita (real	0.991
USD, thousands)	(0.25)
Observations	41

Exponentiated coefficients

Table 7: OLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace once Peace is Obtained

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	-0.829	-0.624	-0.854
	(1.35)	(1.35)	(2.37)
Episode Duration	0.057	0.085	0.038
(years)	(0.06)	(0.06)	(0.09)
Year Episode Ended	-0.876***	-0.841***	-0.769***
	(0.10)	(0.10)	(0.14)
Battle Deaths	0.009	0.004	0.015
(thousands)	(0.03)	(0.03)	(0.04)
Years on Security		1.009	1.469**
Council in Last 10 Years		(0.64)	(0.70)
Years on Security		0.168	-0.545
Council in Last 5 Years		(0.93)	(1.32)
Non-Military Aid			0.098
(real USD, billions)			(3.39)
Military Aid (real			1.028
USD, billions)			(1.16)
Constant	1758.642***	1687.958***	1544.144***
	(191.54)	(207.39)	(275.55)
Observations	75	71	41
R^2	0.53	0.56	0.49

Table 8: 2SLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	1.930	6.781**	12.960*
	(3.62)	(3.14)	(7.36)
Episode Duration	0.015	-0.025	0.068
(years)	(0.08)	(0.08)	(0.14)
Battle Deaths	0.010	0.019	0.055
(thousands)	(0.03)	(0.05)	(0.06)
Year Episode Ended	-0.883***	-0.868***	-0.648**
	(0.11)	(0.15)	(0.28)
Years on Security		-0.867	-5.691
Council in Last 5 Years		(1.27)	(4.15)
Years on Security		2.112***	4.213**
Council in Last 10 Years		(0.80)	(1.76)
Non-Military Aid			-0.735
(real USD, billions)			(4.13)
Military Aid (real			-3.234
USD, billions)			(2.23)
Constant	1771.993***	1741.194***	1298.289**
	(214.24)	(294.05)	(563.41)
Observations	75	71	41
R^2	0.50	0.36	0.02

Table 9: 2SLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace (First Stage)

	(1) PKO b/se	(2) PKO b/se	(3) PKO b/se
Years on Security	-0.203***	-0.252***	-0.211**
Council During Episode	(0.06)	(0.06)	(0.08)
Episode Duration	0.024***	0.027***	0.017
(years)	(0.00)	(0.00)	(0.01)
Battle Deaths	0.002	0.000	-0.006
(thousands)	(0.01)	(0.01)	(0.00)
Year Episode Ended	-0.004	-0.002	-0.009
	(0.01)	(0.01)	(0.02)
Years on Security		0.223*	0.435***
Council in Last 5 Years		(0.12)	(0.14)
Years on Security		-0.065	-0.101
Council in Last 10 Years		(0.06)	(0.07)
Non-Military Aid			-0.272
(real USD, billions)			(0.29)
Military Aid (real			0.353***
USD, billions)			(0.06)
Constant	8.686	3.288	18.594
	(22.04)	(23.09)	(30.26)
Observations	75	71	41
R^2	0.21	0.26	0.40
First Stage F-stat	12.55	17.41	7.25
p-values of tests:			
Anderson-Rubin Wald test	0.63	0.02	0.03
Stock-Wright LM S	0.56	0.04	0.07

Table 10: Censored Regression Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace once Peace is Obtained

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	-0.829	-0.624	-0.854
	(1.32)	(1.29)	(2.12)
Episode Duration	0.057	0.085	0.038
(years)	(0.06)	(0.06)	(0.08)
Year Episode Ended	-0.876***	-0.841***	-0.769***
	(0.09)	(0.10)	(0.12)
Battle Deaths	0.009	0.004	0.015
(thousands)	(0.03)	(0.03)	(0.04)
Years on Security		1.009	1.469**
Council in Last 10 Years		(0.61)	(0.63)
Years on Security		0.168	-0.545
Council in Last 5 Years		(0.89)	(1.18)
Non-Military Aid			0.098
(real USD, billions)			(3.03)
Military Aid (real			1.028
USD, billions)			(1.03)
Constant	1758.642***	1687.958***	1544.144***
	(186.29)	(198.30)	(246.46)
Constant	4.531***	4.496***	4.969***
	(0.54)	(0.55)	(0.70)
Observations	75	71	41

Table 11: Censored Regression Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace once Peace is Obtained, Control Function Approach

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	4.152	5.970**	1.367
	(3.18)	(2.66)	(4.57)
Residual	-3.434*	-4.740***	-1.948
	(1.84)	(1.65)	(2.66)
Episode Duration	-0.020	-0.013	0.043
(years)	(0.07)	(0.07)	(0.08)
Year Episode Ended	-0.888***	-0.865***	-0.750***
	(0.09)	(0.09)	(0.12)
Battle Deaths	0.012	0.017	0.022
(thousands)	(0.05)	(0.04)	(0.04)
Years on Security		1.991***	1.910**
Council in Last 10 Years		(0.62)	(0.72)
Years on Security		-0.754	-1.372
Council in Last 5 Years		(0.80)	(1.09)
Non-Military Aid			-0.036
(real USD, billions)			(3.08)
Military Aid (real			0.343
USD, billions)			(1.57)
Constant	1782.739***	1735.352***	1504.609***
	(185.08)	(182.78)	(250.05)
Constant	4.430***	4.299***	4.937***
	(0.51)	(0.50)	(0.70)
Observations	75	71	41

Table A.1: Data Summary

Country	Years Peacekeepers Were Sent	Years in Conflict	Years as Temporary Members of the S.C.
Afghanistan	1988-1990	1978-2001, 2003-	
Algeria		1991-	1968-1969, 1988-1989, 2004-2005
Angola	1988-1999	1975-2002, 2004, 2007-	2003-2004
Argentina		1955, 1963, 1974-1977	1948-1949, 1959-1960, 1966-1967, 1971-1972, 1987-1988, 1994-1995, 1999-2000, 2005-2006
Azerbaijan		1992-1995, 2005	
Bangladesh		1975-1992	1979-1980, 2000-2001
Bolivia		1946, 1952, 1967	1964-1965, 1978-1979
Bosnia and Herzegovina	1992-2002	1992-1995	
Brunei		1962	
Burkina Faso		1987	1984-1985, 2008
Burundi	2004-2007	1965, 1991-1992, 1994-2006, 2008-	1970-1971
Cambodia		1967-1975, 1978-1998	
Cameroon		1960-1961, 1984	1974-1975, 2002-2003
Central African Republic	1998-2000, 2007-	1996-1997, 2001-2002, 2006	
Chad	1994, 2007-	1966-1972, 1976-1984, 1986-1987, 1989-1994, 1997-2002, 2005-	
Chile		1973	1952-1953, 1961-1962, 1996-1997, 2003-2004
Colombia		1964-	1947-1948, 1953-1954, 1957-1958, 1969-1970, 1989-1990, 2001-2002
Comoros		1989, 1997	
Congo		1993-1994, 1997-1999 2002	1986-1987
Costa Rica		1948	1974-1975, 1997-1998, 2008
Cote D'Ivoire	2004-	2002-2004	
Croatia	1992-1998, 1996-2002	1992-1993, 1995	2008
Cuba		1953, 1956-1958, 1961	1949-1950, 1956-1957, 1990-1991
Democratic Republic of Congo	1960-1964, 1999	1960-1962, 1964-1965, 1967, 1977-1978, 1996-2001, 2006-	
Djibouti		1991-1994, 1999	1993-1994
Dominican Republic	1965-1966	1965	

Egypt	1956-1967	1993-1998	1946, 1949-1950, 1984-1985, 1996-1997
El Salvador	1991-1995	1972, 1979-1991	
Equatorial Guinea		1979	
Eritrea	2000-2008	1997-1999, 2003	
Ethiopia		1960, 1964-1992, 1994-1996, 1998-	1967-1968, 1989-1990
Gabon		1964	1978-1979, 1998-1999
Gambia		1981	1998-1999
Georgia	1993-2009	1991-1993, 2004, 2008-	
Ghana		1966, 1981, 1983	1962-1963, 1986-1987, 2006-2007
Greece		1946-1949	1952-1953, 2005-2006
Guatemala	1997	1949, 1954, 1965-1995	
Guinea		2000-2001	1972-1973, 2002-2003
Guinea-Bissau		1998-1999	1996-1997
Haiti	1993-2001, 2004-	1989, 1991, 2004	
India	1949-	1947-1951, 1956-1959, 1961-1971, 1978-	1950-1951, 1967-1968, 1972-1973, 1977-1978, 1984-1985, 1991-1992
Indonesia	1999-2005	1950, 1953, 1958-1961 1965, 1967-1969, 1975-1992, 1997-2005	1973-1974, 1995-1996, 2007-2008
Iran	1988-1991	1946, 1966-1968, 1979-1988, 1990-1993, 1996-1997, 1999-2001, 2005-	1955-1956
Iraq	1991-2003	1958-1959, 1961-1970, 1973-1996, 2004-	1957-1958, 1974-1975
Israel	1948-	1949-2006	
Kenya		1982	1973-1974, 1997-1998
Laos		1959-1961, 1963-1973 1986-1990	
Lebanon	1958, 1978-	1958, 1975-1976, 1982-1986, 1989-1990	1953-1954
Lesotho		1998	
Liberia	1993-1997, 2003-	1980, 1989-1995, 2000-2003	1961
Macedonia		2001	
Madagascar		1971	1985-1986,
Malaysia		1958-1960, 1963-1966, 1974-1975, 1981	1965, 1989-1990, 1999-2000
Mali		1990, 1994, 2007-	1966-1967, 2000-2001
Mauritania		1975-1978	1974-1975, 1977-1978

Mexico		1994, 1996	1946, 1980-1981 2002-2003
Moldova		1992	
Morocco	1991-	1971, 1975-1989	1963-1964, 1992-1993
Mozambique	1992-1994	1977-1992	
Myanmar		1948-2003, 2005-	
Nepal		1960-1962, 1996-2006	1969-1970, 1988-1989
Nicaragua	1989-1992	1978-1979, 1981-1989	1970-1971, 1983-1984
Niger		1991-1992, 1994, 1996-1997, 2007-	1980-1981
Nigeria		1966-1970, 2004	1966-1967, 1978-1979, 1994-1995
North Korea		1949-1953	
North Yemen	1963-1964	1948, 1962-1970 1980-1982	
Oman		1957, 1972-1975	1994-1995
Pakistan		1971, 1974-1977, 1990, 1995-1996, 2004-	1952-1953, 1968-1969, 1976-1977, 1983-1984, 1993-1994, 2003-2004
Panama		1989	1958-1959, 1972-1973, 1976-1977, 1981-1982, 2007-2008
Papua New Guinea		1989-1990, 1992-1996	
Paraguay		1947, 1954, 1989	1968-1969
Peru		1965, 1982-1999, 2007-	1955-1956, 1973-1974, 1984-1985, 2006-2007
Philippines		1946-1954, 1969-	1957-1963, 1980-1981, 2004-2005
Romania		1989	1962, 1976-1977, 1990-1991, 2004-2005
Rwanda	1993-1996	1990-1994, 1997-2002	1994-1995
Saudi Arabia		1979	
Senegal		1990, 1992-1993, 1995, 1997, 2000-2001, 2003	1968-1969, 1988-1989
Sierra Leone	1998-2005	1991-2000	1970-1971
Somalia	1992-1995	1978, 1982-1984, 1986-1996, 2001-2002, 2006-	1971-1972
South Africa		1966-1988	2007-2008
South Vietnam		1955-1964	
South Yemen		1986	
Spain		1980-1981, 1987, 1991-1992	1969-1970, 1981-1982, 1993-1994, 2003-2004
Sri Lanka (Ceylon)		1971, 1984-2001, 2003, 2005-	

Sudan	2005-	1963-1972, 1976, 1983-	1972-1973
Suriname		1986-1988	
Syria		1966, 1979-1982	1947-1948, 1970-1971, 2002-2003
Tajikistan	1994-2000	1992-1996, 1998	
Thailand		1951, 1974-1982, 2003-	1985-1986
Togo		1986, 1991	1982-1983
Trinidad and Tobago		1990	1985-1986
Tunisia		1980	1959-1960, 1980-1981, 2000-2001
Turkey		1984-	1951-1952, 1954-1955, 1961
Uganda	1993-1994	1971-1972, 1974, 1978-1992, 1994-2007	1966, 1981-1982
Uruguay		1972	1965-1966
Uzbekistan		1999-2000, 2004-	
Venezuela		1962, 1982, 1992	1962-1963, 1977-1978, 1986-1987, 1992-1993
Yemen		1994	1990-1991
Yugoslavia		1991	1950-1951, 1956, 1972-1973, 1988-1989
Zimbabwe		1973-1979	1983-1984, 1991-1992

"Years in Conflict" is not identical to the episodes of conflict. For example, if a country has one episode of conflict from 1980-1984 and a different conflict occurs from 1983-1990, that country will be listed as being in conflict from 1980-1990 under "Years in Conflict".

Appendix B

Table 12: OLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace once Peace is Obtained (After Treaty)

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	0.842	1.339	5.446**
	(1.57)	(1.63)	(2.03)
Episode Duration	0.036	0.065	0.062
(years)	(0.05)	(0.05)	(0.10)
Year Episode Ended	-0.871***	-0.831***	-0.712***
	(0.11)	(0.12)	(0.18)
Battle Deaths	0.006	0.001	0.020
(thousands)	(0.03)	(0.02)	(0.04)
Years on Security		1.197*	2.371**
Council in Last 10 Years		(0.66)	(0.87)
Years on Security		0.004	-2.230
Council in Last 5 Years		(0.94)	(1.60)
Non-Military Aid			-1.182
(real USD, billions)			(3.62)
Military Aid (real			1.315
USD, billions)			(0.91)
Constant	1747.837***	1667.478***	1429.249***
	(218.76)	(235.78)	(359.83)
Observations	75	71	41
R^2	0.53	0.56	0.53

Table 13: 2SLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace (After Treaty)

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	2.579	8.370**	17.209*
	(4.75)	(4.03)	(9.39)
Episode Duration	0.020	0.006	0.110
(years)	(0.07)	(0.07)	(0.12)
Battle Deaths	-0.001	-0.025	0.026
(thousands)	(0.03)	(0.05)	(0.04)
Year Episode Ended	-0.855***	-0.765***	-0.605***
	(0.11)	(0.14)	(0.22)
Years on Security		-0.399	-5.181
Council in Last 5 Years		(1.12)	(3.25)
Years on Security		1.693***	3.955***
Council in Last 10 Years		(0.63)	(1.34)
Non-Military Aid			-3.836
(real USD, billions)			(4.89)
Military Aid (real			2.503
USD, billions)			(1.55)
Constant	1717.307***	1536.399***	1213.905***
	(228.14)	(279.96)	(447.08)
Observations	75	71	41
R^2	0.53	0.44	0.32

Table 14: 2SLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace (After Treaty, First Stage)

	(1) PKO b/se	(2) PKO b/se	(3) PKO b/se
Years on Security	-0.152**	-0.204***	-0.159**
Council During Episode	(0.06)	(0.06)	(0.07)
Episode Duration	0.016**	0.018**	0.010
(years)	(0.01)	(0.01)	(0.01)
Battle Deaths	0.006	0.006	-0.003
(thousands)	(0.01)	(0.01)	(0.00)
Year Episode Ended	-0.014**	-0.014**	-0.009
	(0.01)	(0.01)	(0.01)
Years on Security		0.125	0.298*
Council in Last 5 Years		(0.11)	(0.15)
Years on Security		-0.002	-0.061
Council in Last 10 Years		(0.04)	(0.05)
Non-Military Aid			-0.025
(real USD, billions)			(0.24)
Military Aid (real			-0.068
USD, billions)			(0.08)
Constant	27.706**	27.132**	18.906
	(11.76)	(12.74)	(14.99)
Observations	75	71	41
R^2	0.21	0.24	0.24
First Stage F-stat	7.18	10.12	5.56
p-values of tests:			
Anderson-Rubin Wald test	0.63	0.02	0.03
Stock-Wright LM S	0.56	0.04	0.07

Table 15: OLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace once Peace is Obtained (After Pause in Fighting)

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	2.531**	2.698**	3.920*
	(1.05)	(1.13)	(1.87)
Episode Duration	-0.020	0.003	0.051
(years)	(0.06)	(0.06)	(0.12)
Year Episode Ended	-0.984***	-0.953***	-0.973***
	(0.07)	(0.08)	(0.12)
Battle Deaths	0.026	0.017	0.039
(thousands)	(0.04)	(0.02)	(0.04)
Years on Security		0.959	1.491**
Council in Last 10 Years		(0.62)	(0.55)
Years on Security		0.202	-1.368*
Council in Last 5 Years		(0.81)	(0.69)
Non-Military Aid			3.033
(real USD, billions)			(4.03)
Military Aid (real			-46.574*
USD, billions)			(25.08)
Constant	1974.472***	1911.948***	1951.704***
	(145.71)	(163.28)	(241.99)
Observations	62	59	35
R^2	0.63	0.66	0.66

Table 16: 2SLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace (After Pause in Fighting)

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	3.557	8.904**	13.521*
	(4.70)	(4.09)	(7.83)
Episode Duration	-0.034	-0.081	0.084
(years)	(0.09)	(0.08)	(0.10)
Battle Deaths	0.032	0.064	0.022
(thousands)	(0.05)	(0.04)	(0.04)
Year Episode Ended	-0.985***	-0.967***	-0.867***
	(0.07)	(0.09)	(0.18)
Years on Security		-0.492	-4.258
Council in Last 5 Years		(1.10)	(2.96)
Years on Security		1.406**	3.041**
Council in Last 10 Years		(0.64)	(1.28)
Non-Military Aid			-4.233
(real USD, billions)			(7.57)
Military Aid (real			-42.586*
USD, billions)			(22.92)
Constant	1976.743***	1938.849***	1739.836***
	(137.54)	(175.74)	(354.63)
Observations	62	59	35
R^2	0.63	0.59	0.54

Table 17: 2SLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace (After Pause in Fighting, First Stage)

	(1) PKO b/se	(2) PKO b/se	(3) PKO b/se
Years on Security	-0.138**	-0.213***	-0.194**
Council During Episode	(0.05)	(0.06)	(0.07)
Episode Duration	0.022***	0.027***	0.014
(years)	(0.01)	(0.01)	(0.01)
Battle Deaths	-0.004*	-0.005**	-0.001
(thousands)	(0.00)	(0.00)	(0.00)
Year Episode Ended	-0.003	-0.002	-0.010
	(0.01)	(0.01)	(0.01)
Years on Security		0.175	0.361*
Council in Last 5 Years		(0.11)	(0.17)
Years on Security		-0.002	-0.075
Council in Last 10 Years		(0.03)	(0.06)
Non-Military Aid			0.472
(real USD, billions)			(0.57)
Military Aid (real			-0.039
USD, billions)			(1.68)
Constant	6.719	3.937	19.361
	(13.75)	(14.51)	(20.64)
Observations	62	59	35
R^2	0.26	0.36	0.40
First Stage F-stat	6.35	13.94	7.32
p-values of tests:			
Anderson-Rubin Wald test	0.51	0.01	0.02
Stock-Wright LM S	0.43	0.04	0.07

Table 18: Poisson Regression of Years on Security Council During an Episode on Controls, Doyle and Sambanis Data

	(1) Years on Security Council During Episode b/se
Number Deaths	1.000*
	(0.00)
Episode Duration	1.214***
(years)	(0.06)
Year Episode Ended	1.543**
	(0.32)
Years on Security	1.063
Council in Last 5 Years	(0.80)
Years on Security	2.353*
Council in Last 10 Years	(1.07)
Non-Military Aid	1.000
(real USD, billions)	(0.00)
Military Aid (real	1.000
USD, billions)	(0.00)
GDP per Capita (real	0.526**
USD, thousands)	(0.17)
Observations	23

Exponentiated coefficients

Table 19: OLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace once Peace is Obtained, Doyle and Sambanis Data

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	1.270	1.318	1.871**
	(1.30)	(1.34)	(0.78)
Episode Duration	0.022	0.025	0.131**
(years)	(0.07)	(0.08)	(0.06)
Year Episode Ended	-0.659***	-0.662***	-0.838***
	(0.12)	(0.12)	(0.07)
dead	-0.000	-0.000	-0.000***
	(0.00)	(0.00)	(0.00)
Years on Security		0.119	0.127
Council in Last 10 Years		(0.87)	(0.47)
Years on Security		0.019	0.080
Council in Last 5 Years		(1.27)	(0.57)
Non-Military Aid			0.000
(real USD, billions)			(0.00)
Military Aid (real			-0.000
USD, billions)			(0.00)
Constant	1318.484***	1324.147***	1674.963***
	(241.69)	(246.28)	(138.59)
Observations	49	47	26
R^2	0.53	0.53	0.89

Table 20: 2SLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace, Doyle and Sambanis Data

	(1) Years of Peace b/se	(2) Years of Peace b/se	(3) Years of Peace b/se
PKO	-3.580	-3.335	2.955**
	(4.97)	(4.47)	(1.16)
Episode Duration	0.015	0.011	0.131***
(years)	(0.08)	(0.09)	(0.05)
dead	0.000	0.000	-0.000***
	(0.00)	(0.00)	(0.00)
Year Episode Ended	-0.657***	-0.661***	-0.828***
	(0.12)	(0.12)	(0.06)
Years on Security		0.554	0.017
Council in Last 5 Years		(1.07)	(0.46)
Years on Security		-0.334	0.254
Council in Last 10 Years		(0.89)	(0.40)
Non-Military Aid			0.000
(real USD, billions)			(0.00)
Military Aid (real			-0.000
USD, billions)			(0.00)
Constant	1314.752***	1322.220***	1655.347***
	(236.79)	(230.19)	(116.16)
Observations	47	47	26
R^2	0.39	0.41	0.88

Table 21: 2SLS Estimation of the Effects of Being Sent Peacekeepers on the Duration of Peace (First Stage), Doyle and Sambanis Data

	(1) PKO b/se	(2) PKO b/se	(3) PKO b/se
Years on Security	-0.074**	-0.130**	-0.227**
Council During Episode	(0.03)	(0.06)	(0.10)
Episode Duration	0.002	0.003	0.016*
(years)	(0.01)	(0.01)	(0.01)
dead	0.000**	0.000*	0.000
	(0.00)	(0.00)	(0.00)
Year Episode Ended	-0.000	0.002	0.003
	(0.01)	(0.01)	(0.01)
Years on Security		0.169	0.140
Council in Last 5 Years		(0.10)	(0.12)
Years on Security		-0.022	0.048
Council in Last 10 Years		(0.03)	(0.06)
Non-Military Aid			0.000
(real USD, billions)			(0.00)
Military Aid (real			-0.000
USD, billions)			(0.00)
Constant	0.910	-3.602	-5.112
	(13.19)	(13.53)	(19.24)
Observations	54	54	31
R^2	0.14	0.18	0.39
First Stage F-stat	5.41	5.80	7.86
p-values of tests:			
Anderson-Rubin Wald test	0.41	0.48	0.01
Stock-Wright LM S	0.43	0.50	0.04