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Abstract: Previous statistical studies of the effects of UN interventions have generally suggested that they have a positive effect on fostering peace, however recent methodological developments have called these findings into question. UN missions are not randomly assigned. The cases in which the UN intervenes are quite different in a variety of factors than those in which they do not. We argue that standard approaches for dealing with this problem (Heckman regression and instrumental variables) are invalid and impracticable in this context and lead to estimates of the effects of UN operations that are largely a result of the assumptions of the statistical model rather than the data. As such we cannot be sure if the causal effect of peacekeeping estimated by such techniques is due to peacekeeping itself or to the functional form of the model that the researchers chose. For this reason we correct for nonrandom assignment with matching techniques on a sample of UN interventions in post-Cold-War conflicts and find that UN interventions are indeed causally effective in the sample of post-civil conflict interventions, but that UN interventions while civil wars are still ongoing have no causal effect.



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# 1 Introduction

Do UN interventions *cause* peace? Establishing causality as opposed to mere correlation is one of the most difficult endeavors a social scientist can undertake.<sup>1</sup> Nowhere within the field of international relations is that endeavor more pressing than in evaluating the effectiveness of UN operations. Previous statistical studies have generally suggested that UN interventions have a positive effect on the likelihood of peace after civil war (Doyle and Sambanis (2000, 2006); Fortna (2004, 2007)). However recent methodological developments have cast something of a shadow over these results (King and Zeng, 2006*b*). The problem is that UN interventions are not randomly assigned to civil wars so the cases in which the UN intervened were quite different from those in which they did not. As such estimates of the effects of the UN operations were largely extrapolations from the data. If we fail to control for this problem we cannot be sure if the estimated causal effect of UN intervention is due to the intervention itself or to other factors that are common among cases that received interventions and not common among cases that did not.

Researchers often address the problem of nonrandom assignment by employing a Heckman selection model. The researcher creates a model of the treatment-assignment (selection) process, uses that model to generate predictions of counterfactuals and then compares the factual cases to these predicted counterfactuals. When using such a technique, the researcher implicitly assumes that she can account for nonrandom assignment by means of a single, estimable selection equation that accurately predicts the presence of the nonrandomly assigned treatment—in this case a UN intervention. What's more, in order to operationalize such a procedure, the researcher must find an instrument—a variable that influences whether the

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<sup>1</sup>A fuller discussion of the different approaches to causality is beyond the scope of this paper. For a complete discussion of the various approaches to causality see Pearl (2001). We adopt the Rubin causal model (Rubin, 1979; Rosenbaum and Rubin, 1984).

UN will be present in a given country but does not influence the chances of peace in that country. Both of these requirements raise troubling questions about the validity of estimating the causal effect of UN intervention with a Heckman model.

With regard to the functional form of the underlying selection equation, it is not clear that one equation can accurately account for the presence of the UN. Even if we could develop a model of the UN Security Council selection process, some cases are never considered by the Security Council for any number of reasons. Thus, in order to model accurately the UN's selection of intervention the researcher must model which cases are brought to the UN's attention in the first place—yet another selection process. Combining these processes into one equation to be used as an initial selection equation in the Heckman procedure would severely complicate the analysis and make the predictions of such a model highly dependent on the functional form of the equations chosen. With regard to the need for an instrument, we have good theoretical reasons for thinking that one does not exist. Any factor that effects how long a war or its subsequent peace will last should also be taken into account by the UN Security Council when it is deciding whether or not to allocate a mission.

We correct for nonrandom assignment by using a method advanced by Rubin (1979); Rosenbaum and Rubin (1984) and Ho et al. (2005). We find observations within our “control” group that match as closely as possible our “treated” cases before analyzing the subset of data with a Cox proportional hazard model. We make inferences about the causal effect of UN intervention based on a comparison of these “most similar” cases. The idea is to compare cases where all other causal variables are as similar as possible so that any difference between the cases can be attributed to the treatment. In this way matching comports with tried-and-true techniques of comparative political research. Matching effectively employs Mill's method of difference and Przeworski and Teune (1970)'s most similar systems design. The innovation is that matching uses a relatively large  $n$  compared to most comparative case

study research and it utilizes a rigorous set of criteria by which to judge if cases are similar.

Matching has the further virtue that the analysis is completely transparent (we list the cases we compare in Tables 4 and 5) and inferences are based entirely on the data. None of the results flow from arcane functional form assumptions or implausible arguments about “valid instruments.” Matching makes inferences about the effects of UN intervention from the types of cases where it actually intervenes and not from extrapolations based on assumptions about what would have occurred in cases that have never been observed.

Although our method cannot control for unobserved variables that might affect both war outcomes and UN intervention the way the Heckman approach can (assuming that the researcher has a valid instrument and knows the proper functional form of the data generating process), our own view is that we have sound theoretical reasons for thinking that a valid instrument does not exist and furthermore that we, as political scientists, have a much better grasp of potential confounding covariates (variables that are correlated with both war outcomes and UN intervention) on which to match than we do of the precise functional form of the relationship between civil war, UN intervention and these confounding covariates. These considerations recommend a matching rather than a Heckman approach.

We divide our sample into two sub-samples: a post-conflict sample and in-war sample. In our post-conflict sample, the dependent variable is the duration of the peace period following a civil conflict and in our in-war sample the dependent variable is war duration. We limit our analysis to the post-Cold War period because the end of the Cold War represents a major structural break in the data generating processes for war and peace durations and for UN interventions. We establish the causal efficacy of UN intervention (or lack thereof depending on the samples) by employing techniques that match “treated” observations with similar “control” cases. In that way we are able to establish that the effect on peace (if any) was

truly due to the presence of an operation and not to other extraneous factors because we only compare cases where those extraneous factors are quite similar, and therefore could not be an explanation for the observed variation.

Our estimates suggest that failing to correct for the non-random assignment of the treatment leads to biased causal inferences about the effects of UN intervention in both post-civil-conflict settings and ongoing civil wars. In post-conflict settings UN intervention appears to significantly reduce the hazard of war occurrence and failing to control for nonrandom assignment would lead one to underestimate that effect. In in-war settings UN interventions appear to have no effect on the likelihood that the war will end and failing to correct for nonrandom assignment would lead one to conclude that UN intervention was quite efficacious at ending wars.

## 2 Previous Evaluations of UN Interventions

We first review the previous large  $n$  quantitative evaluations of interventions, which we divide into two categories—those that evaluate UN decisions to deploy missions and those that evaluate the performance of missions once deployed.

Regarding the studies in the first category, Gilligan and Stedman (2003) and Fortna (2004) evaluate whether missions are deployed to the appropriate civil wars according to some set of normative criteria, and they find that the record of Security Council deployment decisions is something of a mixed bag. The UN seems to deploy missions to those civil wars that suffer the most casualties. However there appears to be some regional bias in the way the missions are deployed—Africa and Asia appear to be less likely to receive a UN mission than similar civil wars in other areas. These articles have a very important implication for studies

in the second category, namely they show that UN interventions are not randomly assigned. These two articles indicate that the cases in which the UN intervenes have clearly identifiable differences from cases where the UN does not intervene. As such we must worry about the effects of nonrandom assignment when making causal inferences about UN interventions.

In the second category Doyle and Sambanis (2000, 2006) judge a mission a success if three criteria are all met: a) the war ends and does not resume for some specified period of time b) residual violence does not resume within the same period of time and c) there is some democratization within that time period. They find that, for the most part, UN operations are positively correlated with their measures of peace building success. Another important contribution to the evaluation of the effectiveness of UN peacekeeping is provided by Fortna (2004).<sup>2</sup> She finds that UN missions prolong ceasefires.

Recently King and Zeng (2006*a*) have argued that Doyle and Sambanis (2000)'s results are highly "model dependent." In other words the estimated causal effect of UN intervention changes dramatically with minor modifications to the specification. They show that simply adding an interactive term between conflict duration and the presence of a UN operation altered the predictions of the model substantially. Presumably the same general criticism also applies to Fortna (2004).

Model dependence arises from "extreme counterfactuals," that is faulty counterfactual inferences from comparing treated and control samples that are too disjoint. For example as shown in Figure 1 there are no cases of post-conflict settings where the UN intervened and in which the previous war had log cumulative battle deaths of less than roughly five whereas there are many cases where the UN failed to intervene in that category. Thus predictions

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<sup>2</sup>Although not exclusively a study of UN intervention, Hartzell, Hoddie and Rothchild (2001) find that peace agreements last longer if there is a third-party intervention and in some of their sample the UN plays that role.

about what would have happened had the UN intervened following a conflict with relatively low casualties are largely extrapolations from the data. With no data on which to base such inferences their causal claims were based on modeling assumptions rather than the data.

Had UN interventions been randomly assigned and had there been a sufficiently large number of them the extreme counterfactual problem would (probabilistically) disappear because the characteristics of the sample in which the UN intervened would have been similar to the sample where it did not intervene. For example if UN interventions were randomly assigned some cases with low casualties would have probably received a UN intervention. By addressing the nonrandom assignment problem the matching techniques we use in this paper correct King and Zeng (2006*a*)’s extreme-counterfactual and model dependence problem by insuring that the treated and the control groups are similar.

A second contribution of the paper is its treatment of ongoing wars. Fortna (2004)’s model was by design a model of post-conflict settings and as such does not address the effect of UN intervention while a war was still ongoing. Doyle and Sambanis (2000) examined seven cases of ongoing conflicts in their analysis; however they pooled them with civil wars that had ended. The data generating process of ongoing wars is quite likely different from that for wars that have ended however. Indeed the civil war literature implicitly recognizes that pooling is inappropriate by treating “war onset” differently from “war duration.”<sup>3</sup> Therefore we allow the data generating process of the duration of the post conflict period to be different from that of war duration.

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<sup>3</sup>In the former category see Fearon and Laitin (2003), Fortna (2004), Collier and Hoeffler (2004) Miguel, Satyanath and Sergenti (2004) and Hegre and Sambanis (2006) among others. In the latter category see Balch-Lindsay and Enterline (2000), Regan (2002) Fearon (2004) and Hegre (2004) among others. Elbadawi and Sambanis (2002) combine both approaches.

### 3 Method

In evaluating the effect of UN missions in prolonging periods of peace or shortening periods of war it is useful to make an analogy to a laboratory experiment. In a laboratory setting the researcher is careful that the treatment she wishes to study is randomly assigned. In that way she can ensure that any difference in the outcome between the treated and untreated cases are due to the treatment and not some other different characteristics of the treated and the control groups. By virtue of randomizing the treatment, any such characteristics will not be correlated with whether the case received the treatment. By analogy we can think of UN intervention as the treatment. The duration of peace or war is the affected outcome. Unfortunately the treatment in this case is not randomly assigned. As such the researcher must be careful to make sure that any differences between the treated and control groups are in fact due to the treatment and not to some other characteristic that was common to the treated group but not the control group. For example, if, as shown in Figure 1, the post-conflict periods in which the UN intervenes are characterized by higher levels of cumulative battle deaths from the previous war and higher levels of cumulative battle deaths are associated with shorter periods of peace, then not controlling for the confounding influence of cumulative battle deaths will bias the estimated causal effect of UN intervention.

The prevailing methodology to address these problems is to add a variable to a parametric estimator (ordinary least squares regression, probit, Heckman regression etc.) to control for that variable's confounding influence. But, as King and Zeng (2006*a,b*) and Ho et al. (2005) argue, if the distributions of the confounding factors do not overlap sufficiently—in our case, for example, the distribution of the log of the cumulative battle deaths is farther to the right for those cases in which the UN intervened than it was for the cases where the UN did not intervene—then the causal inferences are sensitive to the unverifiable specification

assumptions of the parametric model.

To improve the reliability of one’s causal inferences, Ho et al. (2005) recommend pre-processing one’s data using matching techniques in which treated cases are matched with similar untreated cases. Matching creates a sample of the data in which the difference between the treated distribution and the non-treated distribution is reduced. When matching is perfect and the distributions match exactly a simple difference in means of the treated and the untreated is sufficient to obtain the causal effect of the treatment. But in most cases matching is not perfect. In order to adjust for any remaining imbalance (and in our case to account for right-censoring) one should then use the same parametric model one would have applied to the full data set on the matched sub-sample. This, as Ho et al. (2005) describe, greatly reduces the role of functional form and specification assumptions of the parametric model, leading to more reliable causal inferences. This method is “double robust:” if matching is not complete but the parametric model is properly specified, or if the model is not properly specified but the matching is adequate, causal estimates will be consistent.

With each of our samples, we first identify the confounding factors. Confounding factors are those variables that (1) influence the dependent variable conditional on treatment, (2) are correlated with the treatment variable, and (3) are causally prior to treatment. Thus, for example, the growth rate of a country after a UN intervention is not a confounding factor even though it does influence the likelihood of peace<sup>4</sup> and is correlated with a UN presence, because it is not causally prior to a UN intervention and therefore the intervention may be creating the circumstances for higher growth. Including growth after a UN intervention in our analysis would lead to post-treatment bias, incorrectly controlling for the consequences of treatment, thus biasing the overall causal effect (Rosenbaum, 2002; King and Zeng, 2006*a,b*). Therefore, when selecting our confounding factors, we must be careful that they meet all

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<sup>4</sup>See Collier and Hoeffler (1998, 2004) and Miguel, Satyanath and Sergenti (2004)

three requirements outlined above.

Once we have developed a list of confounding factors, we use one-to-one nearest neighbor matching with replacement to obtain matches from all of our control (nonintervention) cases for each of our treatment (intervention) cases.<sup>5</sup> One-to-one nearest neighbor matching was preformed with GenMatch (Sekhon, 2006*b*), which uses a genetic search algorithm to determine the weight each confounding factor should receive in order to obtain optimal balance (Sekhon, 2006*b*; Sekhon and Mebane, 1998). Once we obtained matches, we analyzed the matched data with a Cox proportional hazards model, which corrects for any right censoring and remaining imbalance.<sup>6</sup> Last, we present results of the Cox regression on the full (unmatched) sample to compare the causal effects estimated by the different procedures.

By necessity we estimate the average treatment effect on the treated (ATT). That is, we answer the question: how well did the UN do in the range of cases where it actually intervened and, if the UN were to intervene in a similar case, how well would we expect it to perform? We cannot answer the question: how well would the UN have done had it intervened in a type of situation where it has never intervened (typically called the average treatment effect on the control or ATC). We do not have treated cases to match to all of our control cases and so we cannot answer that question. Like drugs and surgical procedures, UN interventions are designed for certain cases and not for others. For example, it is rare that the UN intervenes during a coup because they are typically over in a matter of days and do not provide the international community enough time to put a mission in place. In addition, UN missions tend not to be found in large countries, as it is harder for the UN to manage such a task. Thus, asking how well the UN would do if it were to intervene during a coup in a large

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<sup>5</sup>We also used two-to-one matching as well. See below.

<sup>6</sup>We also ran the same specification with different parametric models (exponential, Weibull, and lognormal) and the results were substantively the same. We prefer the Cox procedure because it is semi-parametric and therefore less dependent on any particular parametric assumption.

country is not realistic because the UN does not intervene in such cases and so we have no data from which to make such inferences. We estimate how well the UN performs in the type of cases where it actually intervenes.

## 4 The Effect of UN Intervention in Prolonging Peace

### 4.1 Sample Definition

We begin with an analysis of the post-conflict sample. A fuller description of the data, the data itself and our estimation code are available at [www. ....](http://www. ....) We adopt a country-level unit of analysis and our dependent variable is the number of months a given country was at peace following a civil conflict according to Gleditsch et al. (2002). We limit our attention to the post-Cold War period and our durations are right-censored at December 2003.

Our treatment variable, a UN intervention in a country during a particular month, is a dichotomous variable equal to 1 if the UN was present at any point during a period of peace, and 0 otherwise. We recognize the shortcoming of using a dichotomous measure of the treatment. UN missions come in many different sizes with different mandates. Unfortunately we simply do not have enough cases of UN intervention to take these nuances into account reliably. The number of UN interventions in our sample (nineteen and sixteen for the post-conflict and in-war samples respectively) is already small. Dividing UN interventions into more fine-grained categories would reduce our sample size for each type of treatment even further and make reliable inference impossible.

## 4.2 Confounding Factors

Our confounding factors and reasons for including them are as follows.

**Log (Cumulative Battle Deaths) from Last War** Battle deaths is a confounding factor because it is negatively correlated with the duration of the subsequent peace (Fortna, 2004) and positively correlated with the probability that the UN will intervene (Gilligan and Stedman, 2003). Indeed the “before-matching” balance statistics in Table 1 show that the cumulative battle deaths from the previous war of the treated group are significantly greater than those of the control group, and the Kolmogorov-Smirnov (K-S) test indicates that the two distributions are very different with a  $p$ -value of zero. The battle death data come from Lacina and Gleditsch (2005).

**Duration of Last War.** The duration of the previous war is positively correlated with the duration of the subsequent peace (Hartzell, Hoddie and Rothchild, 2001; Fortna, 2004) and has been shown to be positively correlated with the probability that the UN will intervene (Gilligan and Stedman, 2003). The before-matching balance statistics in Table 1 corroborate this finding—the UN tends to become involved following wars of longer duration. The K-S test indicates that the two distributions are indeed significantly different.

**Ethnic Fractionalization.** Some authors have argued that ethnic fractionalization affects civil war onset and therefore would be negatively correlated with the duration of the peace period, although this finding is not robust (Fearon and Laitin (2003) which is the source of our ethnicity measure). No previous studies have linked ethnic fractionalization to likelihood of UN intervention and indeed the before-matching balance statistics in Table 1 indicate that there is no difference between the treated and control groups

on this dimension. Still we include the variable as a confounding covariate to avoid any omitted variable bias.

**Log (Population Size)** Collier and Hoeffler (1998, 2004) found that countries with large populations are more prone to civil war. As shown in Table 1 populations of countries in which the UN intervened were smaller than the countries in which they did not intervene and both the *t*-test on the difference of means and the K-S test on the difference of the distributions indicates that this difference is highly significant. Our population data come from the World Bank World Development Indicators, 2005

**Log (Mountainous)** Fearon and Laitin (2003) (our source for this measure) found that countries with more mountainous terrain are more likely to have outbreaks of civil war. Therefore mountainous terrain is negatively correlated with the duration of the peace period. As shown in Table 1 the UN also appears to intervene in more mountainous countries. The K-S test indicates that the distributions of treated and control groups are significantly different at the 0.03 level.

**Log (Military Personnel)** Gilligan and Stedman (2003) found that the size of a country's government army was negatively correlated with the likelihood that the UN would intervene in that country. Furthermore periods of peace should be longer in such states because large government armies should deter would-be rebels. We were unable to establish this hypothesized correlation, however, probably because the variable is endogenous to the threat of civil war—governments have large armies because their countries are more civil-war prone. Therefore we include the variable because we have theoretical reasons for doing so and to err on the side of caution in taking account of possible confounding factors. Our measure of military personnel comes from David, Bremer and Stuckey (1972); Singer (1987)

**Democracy before Last War** Hartzell, Hoddie and Rothchild (2001) suggest that democ-

racies are less prone to civil war, so democracy score should be positively correlated with duration of peace. Andersson (2000) suggests that the UN is more likely to intervene in democracies. Using the Polity2 measure from the last month before the civil war started, our before-matching balance statistics in Table 1 indicate that the treated and control groups may be different, albeit in the opposite direction of Andersson’s hypothesis— countries in which the UN intervened appear to be less democratic than those in which the UN did not intervene.

**Regional Controls** Gilligan and Stedman (2003) and Fortna (2004) found that the probability of a UN mission being allocated varied by region. Survival-time analysis (not shown) also indicated that duration of peace varied by region. Our regional categories were Latin America, Asia, North Africa/Middle East, North Africa/Middle East, Sub-Saharan Africa and Eastern Europe.<sup>7</sup>

### 4.3 Matching, Balance, and Proportional Hazard Results

We now turn to the creation of our matched sub-sample. We generated matched observations using one-to-one matching with replacement.<sup>8</sup> These matched cases are listed in Table 4. Table 1 presents “before” and “after” balance statistics. There are several methods for describing the balance of a sample (see Imai, King and Stuart (2006); Sekhon (2006*a*)). In what follows, we look at three standard indicators of balance: the difference in means, the

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<sup>7</sup>In addition to the variables listed above we also matched on whether the rebels in the previous war were contraband funded, whether the last war was a “sons-of-the-soil” war and whether it was a coup or revolution (see the next section for a fuller explanation of the variables). These variables were taken from Fearon (2004). Including these variables did not change our substantive findings. We chose not to include those variables in the results we present here for two reasons. First missing values for these measures reduced our sample size by about ten percent. Second we had less faith in the applicability of these measures to our post-conflict sample. Fearon (2004), our source for the variables, used them to explain war duration. As such they were measures for specific wars and do not necessarily carry over into the the post-conflict period.

<sup>8</sup>We also performed our analysis using two-to-one matching. The results of the subsequent analysis were not substantively modified.

$p$ -values from a  $t$ -test on the difference of means, and where possible, the  $p$ -values from a K-S test of similar distributions. Following the recommendations of Imai, King and Stuart (2006) we also include the ratio of the variances of the treated and control samples and the mean standardized difference from the QQ plot.<sup>9</sup>

The after-matching statistics presented in Table 1 indicate that we have achieved excellent balance. The  $p$ -values on all  $t$ -tests are above 0.2. All  $p$ -values on K-S tests are above 0.2 except for duration of the last war, and even in that case the  $p$ -value is quite good at 0.186 and the the QQ statistic shows marked improvement.

The traditional method for calculating the causal effect of a treatment is to take a difference in means. However, our dependent variable is the duration of a period of peace and many of our cases our right censored, so we employ a duration model instead to control for the right censoring and any remaining imbalance with the confounding factors. The top panel of Table 3 presents the causal effect of UN intervention estimated by a Cox regression on the unmatched and matched samples. The Cox regression results are presented in the relative hazard metric. In addition to the dummy variable for UN intervention (the treatment) we also included all non-regional variables listed in Table 1 and the Sub-Saharan regional variable as controls in our Cox estimation, but we only present the coefficients for the treatment variable in Table 3.<sup>10</sup>

According to the estimates from the Cox regression on the matched sample, a UN presence results in the reduction of the hazard rate of transition to war by over 90 percent or by a factor of 0.089. The comparable estimate from the unmatched sample is that UN intervention reduces the hazard rate of transition to war by a factor of 0.263, which indicates that

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<sup>9</sup>QQ plots are available in the web appendix.

<sup>10</sup>The confounding variables are included in the Cox estimation only to control for any remaining imbalance. We do not present those coefficients because they are substantively meaningless since we have matched on those variables.

estimates of the effect of UN intervention, when they do not correct for the nonrandom assignment of the UN intervention are biased. They underestimate the effectiveness of UN intervention in prolonging post-conflict peace.

Figure 2 illustrates the monthly probability of transition from peace to war when the UN has intervened and when it has not as estimated by the Cox model. The effects of a UN intervention are quite striking. For example after 25 months of peace the estimated probability of transition to war is over one percent when the UN does not intervene while that probability is about one-tenth of a percent when the UN has intervened. The probability of transition from peace to war is small in either case but the UN clearly has a large impact, and while these percentages may appear to be small it is important to remember that they are *monthly* probabilities so that over the course of a year or more the difference can be quite substantial.

In summary, our results from the post-conflict sample clearly indicate UN intervention significantly reduces the proportional hazard of returning to war. Even more important, our results show that it is crucial when evaluating the record of UN missions in post-civil-war settings to take into account the nonrandom assignment of UN interventions. Not correcting for this problem produced underestimates of the effectiveness of UN missions in post-conflict settings.

## 5 The Effect of UN Intervention on Shortening Wars

### 5.1 Sample Definition

Our dependent variable in this section is the duration of war. As before we focus on the post-Cold War period and our dependent variable is right censored in December 2003. One important feature of the in-war sample compared to the post conflict sample is that the timing of UN interventions is more critical. With the post-conflict cases it was reasonable to ignore the differences in the timing of the UN interventions because most of them occurred in the first month of the peace period. With the in-war cases it is the exception that the UN is present from the start of the war.<sup>11</sup> Hence, to properly examine the effect of the UN, we cannot use a time-invariant UN intervention variable as we did with the post-conflict sample. Doing so would substantially underestimate the effectiveness of the UN mission, because a war could have been going on for many years before the UN intervened, and then, after the UN intervened, stop after a few months. The relevant metric is instead “how long does the war last once the UN has intervened?” Regan (2002) also highlights this issue, which is why he argues for the use of a duration model with time-varying covariates. For the same reason, we estimate a time-varying covariates model on the unmatched data for our in-war sample.

When we turn to our matched sample, however, a time-varying-covariates approach causes concern about post-treatment bias. For example, after a UN intervention, the number of yearly battle deaths could go down, which might affect the outcome of the war. Controlling for this factor after the UN intervenes will bias the estimated effect of the UN, but not controlling for it before the UN would fail to address the nonrandom assignment of the UN treatment. Our solution is to transform our time-varying data set by taking “snapshots”

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<sup>11</sup>This happened only in Rwanda and Tajikistan where a UN intervention was already in place for other reasons when civil war erupted.

of the data for different durations of the war. In cases with a UN intervention we take a snapshot at the month that the UN intervened, freezing the time-varying variables at their values in that month and match on the variables at those values. Furthermore duration of the war is one of our confounding covariates. Thus it would be inappropriate to compare the effect of a UN intervention in, say, the fortieth month of a war, with a similar war in, say, its second month. Thus we need to take snapshots of each non-UN case at different durations of the war in order to match the treated UN cases appropriately. In other words we match on the duration of the war and on the level of the particular time-varying covariates at the time the UN intervened. We then examine the effect of the entire period from the time after the UN intervened, even if the UN left after a few years (as it did in Somalia, Angola, and Cambodia). The difference from the post-conflict sample is that we only start to examine the in-war case once the UN has intervened, not before. Doing so gives us the correct counterfactual: how much longer would the country have remained in a state of war had the UN not intervened?

## 5.2 Confounding Factors

We now present the confounding variables on which we matched in our in-war sample and our reasons for doing so.

**Non-UN Third Party Interventions** Regan (2002) shows that international interventions increase the duration of war. Furthermore, our before-matching balance statistics, shown in Table 2, indicate that proportionally more of the treated group experienced a non-UN third party intervention than did the control group.

**Log (Battle Deaths)** As in the post conflict sample high battle deaths may both reduce the hazard rate of peace and increase the likelihood that the UN will intervene. Indeed

the before-matching statistics in Table 2 indicate that the average battle deaths of the treated group are higher than that of the control group and the K-S test indicates that the distribution of this variable is very different across those two groups.<sup>12</sup> Note that this is time-varying measure for the ongoing war in contrast to the measure we used in the post-conflict sample.

**Coup/Revolution** This is an indicator variable for whether the war was a coup or revolution. Fearon (2004), who is our source for these data, found that such wars are shorter in duration. Our before-matching statistics indicate that it is unlikely that our treated and control groups are different along this dimension but we match on it to avoid omitted variable bias.

**Sons of the Soil** This is an indicator variable for whether the war was a “sons-of-the-soil” conflict defined as “land conflict between a peripheral ethnic minority and state-supported migrants of a dominant ethnic group.” This measure is from Fearon (2004) who found that such wars lasted longer than other types of wars. Our before-matching balance statistics indicate that those conflicts that receive a UN mission are significantly less likely to be “sons-of-the-soil” conflict than those conflicts that do not receive a UN mission.

**Rebels Contraband Funded** This is an indicator variable for whether the rebel group in the war was financed by trade in contraband (drugs, diamonds, etc.). Such funding has been shown to increase the duration of the war by Fearon (2004) who is our source for these data. Our before-matching balance statistics do not give great cause for concern but multivariate probit estimates (not shown) indicated the UN was significantly more likely to intervene in conflicts where the rebels were contraband funded and so we

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<sup>12</sup>Lacina and Gleditsch (2005) provide yearly battle death data. We divide their yearly figure evenly over the number of months a country was at war in the respective year.

include this variable as a possible confounding factor.

**Ethnic Fractionalization.** By some accounts ethnic fractionalization prolongs war. Furthermore, unlike in the post-conflict sample there is a strong indication from the K-S test in Table 2 that those conflicts in the in-war sample that received a UN operation had lower levels of ethnic heterogeneity than those conflicts that did not.

**Log (Population Size)** Similar to the post conflict sample this variable is positively correlated with the duration of war and negatively correlated with the likelihood of UN intervention. The  $t$ -test shows that the treated cases had significantly smaller populations than the untreated cases and the K-S test indicates that the distributions are quite different.

**Log (Mountainous)** Following Fearon and Laitin (2003) wars may be prolonged in countries with more mountainous terrain and the cases in which the UN became involved were less mountainous on average as indicated by the before-matching balance statistics in Table 2. The K-S test reveals that the distributions of the mountainous variable were very different in the treated and the control groups.

**Log (Military Personnel)** As in the post conflict sample, UN interventions are less prevalent in countries with large armies and we have theoretical reasons for thinking that a large military is correlated with the likelihood that a war will end. The former point is corroborated by the before-matching balance statistics in Table 2. The average number of military personnel in those countries into which the UN intervened was significantly smaller than in countries where the UN did not intervene.

**Regional Controls** The before-matching balance statistics in Table 2 indicate that treated cases are significantly less likely to be in Asia and North Africa/Middle East than control cases are. Survival time analysis (not shown) also suggested that length of war var-

ied by region. Our regional categories were Latin America, Asia, North Africa/Middle East, Sub-Saharan Africa and Eastern Europe.

### 5.3 Matching, Balance, and Proportional Hazard Results

We generated matched observations using one-to-one matching with replacement. As a robustness check we also used two-to-one matching and it did not change the balance statistics or the results appreciably. Our matches for this sample are listed in Table 5. Table 2 presents before- and after-matching balance statistics. The after-matching statistics indicate that we have achieved excellent balance. The  $p$ -values on all K-S statistics are well above 0.6 with the exception of log population size and log military personnel. Even in these two exceptions the  $p$ -values on the K-S tests are respectable at 0.18 and 0.174 respectively and the QQ statistics show marked improvement as a result of our matching.

In the bottom half of Table 3, we present the estimated causal effect of UN interventions on the unmatched and matched samples.<sup>13</sup> The importance of controlling for nonrandom assignment is clear. In estimates from the unmatched sample the Cox regression coefficient on the UN intervention variable suggests that the proportional hazard of a country transitioning out of war and into peace is over two and a half times greater in cases where the UN intervened. However within the matched sample a UN presence has no effect. The hazard rate actually decreased by a factor of 0.747, and the effect is not statistically significant.<sup>14</sup>

Failing to correct for nonrandom assignment of UN operations in the in-war sample would lead researchers to overestimate the causal effect of the UN in shortening civil wars. Thus

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<sup>13</sup>The size of the unmatched sample in the in-war analysis is much larger than that of the post-conflict sample because of the time-varying approach we adopted in the in-war samples.

<sup>14</sup>As in the post-conflict sample these are multivariate estimates. In addition to the UN treatment variable we included the non-regional variables and the Eastern Europe and Asia regional dummy variables. We do not report those coefficients because we have matched on those variables.

while the substantive conclusion of our in-war sample is quite different from the one we reached in the post conflict sample the methodological conclusion is the same—it is crucial that researchers control for the nonrandom assignment when evaluating UN interventions. We might also point out that the different causal effects of UN operations in the post-conflict and in-war samples provides further evidence of the inappropriateness of pooling those two samples.

## 6 Conclusion

Do UN interventions *cause* peace in civil wars? In answering this question it is crucial to take into account the fact that UN missions are not randomly assigned to civil conflicts. Was UN intervention truly causing peace or was the peace due to other factors that either by chance or design characterized the conflicts into which the UN intervened? We have addressed the nonrandom assignment of UN missions using matching techniques. Our results suggest that the UN truly has had an important independent causal effect in prolonging periods of peace and that that effect is even larger than would be estimated had we not corrected for the nonrandom assignment of UN missions. In our in-war sample we could find no causal effect from UN interventions in shortening the war. Controlling for nonrandom assignment was equally important for this result because our estimates when we did not correct for it indicated that UN operations were effective in helping countries transition out of war and into peace.

Previous quantitative empirical research that has argued that UN interventions foster peace has been questioned by recent methodological critiques because it was based on comparisons to “extreme counterfactuals” and therefore its conclusions are highly “model dependent.” Matching techniques have allowed us to address this issue and get more reliable estimates of

the causal effect of UN interventions in civil wars.

Our aims in writing this paper have been twofold. We have sought to introduce to a wider audience a different method that avoids in a straightforward way the twin pitfalls of model dependence arising from the use of extreme counterfactuals and the need for an instrument when addressing the nonrandom selection problem. Second, by employing these methods, we have hoped to provide a more robust set of findings about the effectiveness of UN intervention. The implications of our findings for policy and the development of UN doctrine are clear. The UN is quite good at peacekeeping. It is not good at war fighting. While this is a point that has been raised by others, we reach this conclusion based on a more robust analysis of the causal effect (or lack thereof) of UN intervention and therefore we can have much more confidence in the conclusion.

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Table 1: Balance Statistics, Post-Conflict Sample

Variable	Mean treated	Mean control	<i>t</i> -test <i>p</i> -value	K-S test <i>p</i> -value	Var. ratio (Tr/Co)	Mean Std. eQQ diff
Before Matching						
Log (Cumulative Battle Deaths)	8.982	6.647	0.001	0	0.582	0.226
Duration of Last War	80.526	50.279	0.141	0.006	0.908	0.192
Ethnic Fractionalization	0.492	0.565	0.299	0.122	1.154	0.097
Log Population Size	8.754	9.509	0.004	0	0.304	0.177
Log Mountainous	2.797	2.221	0.105	0.03	0.883	0.121
Log Military Personnel	3.254	3.874	0.081	0.032	0.495	0.137
Log GDP per Capita	6.588	6.56	0.921	0.698	1.160	0.051
Polity	-2.579	-0.838	0.194	0.24	0.661	0.088
Eastern Europe	0.368	0.147	0.082	-	1.929	0.111
Latin America	0.211	0.118	0.380	-	1.665	0.046
Asia	0	0.191	0	-	0	0.096
Sub-Saharan Africa	0.316	0.426	0.383	-	0.919	0.055
North Africa/Middle East	0.105	0.118	0.881	-	0.944	0.006
After Matching						
Log (Cumulative Battle Deaths)	8.982	8.373	0.218	0.218	1.213	0.111
Duration of Last War	80.526	66.842	0.254	0.186	0.790	0.109
Ethnic Fractionalization	0.492	0.407	0.214	0.258	0.894	0.082
Log Population Size	8.754	8.894	0.384	0.252	0.971	0.094
Log Mountainous	2.797	2.381	0.337	0.462	0.932	0.086
Log Military Personnel	3.254	3.462	0.231	0.686	1.224	0.077
Log GDP per Capita	6.588	6.518	0.810	0.752	0.779	0.075
Polity	-2.579	-2.263	0.775	0.856	1.059	0.045
Eastern Europe	0.368	0.368	1	-	1	0
Latin America	0.211	0.211	1	-	1	0
Asia	0	0	1	-	NA	0
Sub-Saharan Africa	0.316	0.368	0.318	-	0.929	0.026
North Africa/Middle East	0.105	0.053	0.318	-	1.889	0.026

Table 2: Balance Statistics, In-War Sample

Variable	Mean treated	Mean control	t-test p-value	K-S test p-value	Var. ratio (Tr/Co)	Mean Std. eQQ diff
Before Matching						
Non-UN 3rd-Party Intervention	0.438	0.101	0.019	-	2.895	0.168
Log (Battle Deaths)	6.941	6.613	0.49732	0	0.915	0.039
Coup/Revolution	0.0625	0.049	0.829	-	1.347	0.007
Sons-of-Soil War	0.188	0.413	0.042	-	0.670	0.113
Rebels Contraband Funded	0.375	0.267	0.402	-	1.277	0.054
War Duration	62.875	95.806	0.062	0	0.333	0.078
Ethnic Fractionalization	0.488	0.591	0.168	0	1.1276	0.103
Log (Population Size)	8.964	10.013	0.001	0	0.636	0.270
Log (Mountainous)	2.634	2.7347	0.78069	0	1.192	0.102
Log (Military Personnel)	3.092	4.386	0.008	0	1.415	0.181
Eastern Europe	0.375	0.057	0.022	-	4.649	0.159
Latin America	0.0625	0.111	0.450	-	0.630	0.024
Asia	0.125	0.315	0.541	-	0.541	0.095
Sub-Saharan Africa	0.375	0.395	0.878	-	1.046	0.010
North Africa/Middle East	0.625	0.122	0.359	-	0.582	0.030
After Matching						
Non-UN 3rd-Party Intervention	0.438	0.25	0.255	-	1.313	0.0934
Log (Battle Deaths)	6.941	7.108	0.782	0.628	1.032	0.082
Coup/Revolution	0.0625	0.0625	1	-	1	0
Sons-of-Soil War	0.188	0.188	1	-	1	0
Rebels Contraband Funded	0.375	0.375	1	-	1	0
War Duration	62.875	55.25	0.137	0.994	1.330	0.045
Ethnic Fractionalization	0.488	0.420	0.335	0.604	1.358	0.108
Log (Population Size)	8.964	9.156	0.275	0.18	1.854	0.125
Log (Mountainous)	2.634	2.4516	0.5144	0.876	0.880	0.077
Log (Military Personnel)	3.092	3.759	0.160	0.174	3.20	0.114
Eastern Europe	0.375	0.375	1	-	1	0
Latin America	0.0625	0.125	0.568	-	0.536	0.031
Asia	0.125	0.188	0.718	-	0.718	0.031
Sub-Saharan Africa	0.375	0.313	0.659	-	1.091	0.031
North Africa/Middle East	0.625	0	0.318	-	NA	0.031

Table 3: The Causal Effect of UN Intervention:  
 Cox Proportional Hazards Estimates  
 Both Samples pre- and post-matching

<b>Post-Conflict Sample</b>	
<b>Unmatched, <math>n=87</math></b>	<b>Matched, <math>n=38</math></b>
<b>Coefficient</b>	<b>Coefficient</b>
<b><math>t</math>-stat.</b>	<b><math>t</math>-stat.</b>
0.263	0.089
-2.35	-2.57
<b>In-War Sample</b>	
<b>Unmatched, <math>n=3999</math></b>	<b>Matched, <math>n=32</math></b>
2.620	0.747
1.85	-0.51

Table 4: Matched pairs, Post-Conflict Sample

<b>Treated, Start Date</b>	<b>Control, Start Date</b>
Haiti Jan 1992	Panama Nov 1989
Guatemala Jan 1996	Paraguay Mar 1989
El Salvador Jan 1992	Peru Jan 2000
Nicaragua Jan 1990	Paraguay Mar 1989
Croatia Jan 1994	Azerbaijan Aug 1994
Croatia Jan 1996	Azerbaijan Aug 1994
Serbia and Montenegro Jul 1999	Serbia and Montenegro Jan 1992
Bosnia and Herzegovina Jan 1996	Azerbaijan Aug 1994
Georgia Jan 1994	Moldova Aug 1992
Liberia Sep 1995	Guinea-Bissau June 1999
Sierra Leone Jan 2001	Burundi Jan 1993
Zaire Jan 2002	Zaire Jan 1998
Rwanda Jan 1995	Burundi Jan 1993
Mozambique Nov 1992	Somalia Jan 1997
Namibia Jan 1990	Chad Jan 1989
Morocco Jan 1990	Iraq Jan 1997
Lebanon Jan 1991	Azerbaijan Aug 1994
Tajikistan Jan 1997	Azerbaijan Aug 1994
Tajikistan Jan 1999	Niger Jan 1998

Table 5: Matched pairs, In-War Sample

<b>Treated, Month</b>	<b>Control, Month</b>
El Salvador Jul 1991	Mozambique Jul 1988
Croatia Feb 1992	Moldova Apr 1994
Croatia Jan 1995	Moldova Mar 1992
Bosnia and Herzegovina Jun 1992	Azerbaijan Feb 1992
Georgia Aug 1993	Azerbaijan Dec 1992
Liberia Sep 1993	Senegal Oct 2000
Sierra Leone Jul 1998	Nicaragua Mar 1989
Zaire Dec 1999	Zaire Oct 1997
Rwanda Jun 1993	Rwanda Feb 2000
Somalia Apr 1992	Sri Lanka Oct 1994
Angola Dec 1988	Afghanistan Oct 1992
Lebanon Jan 1988	Burundi Oct 2002
Tajikistan Dec 1994	Azerbaijan Feb 1994
Tajikistan Jan 1998	Azerbaijan Jan 1992
Cambodia Oct 1991	Nicaragua Nov 1989
Indonesia Oct 1999	Myanmar (Burma) Apr 2000

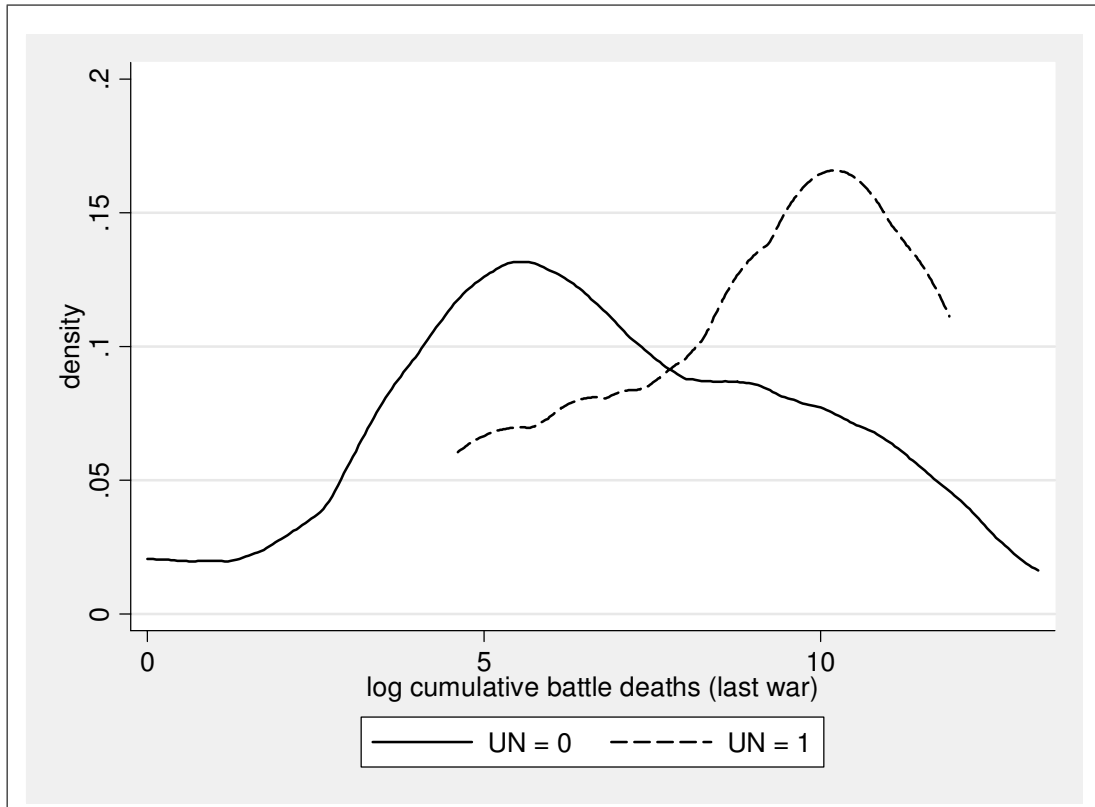


Figure 1: Kernel densities of log cumulative battle deaths in cases where the UN intervened (UN=1) and where it did not (UN=0)

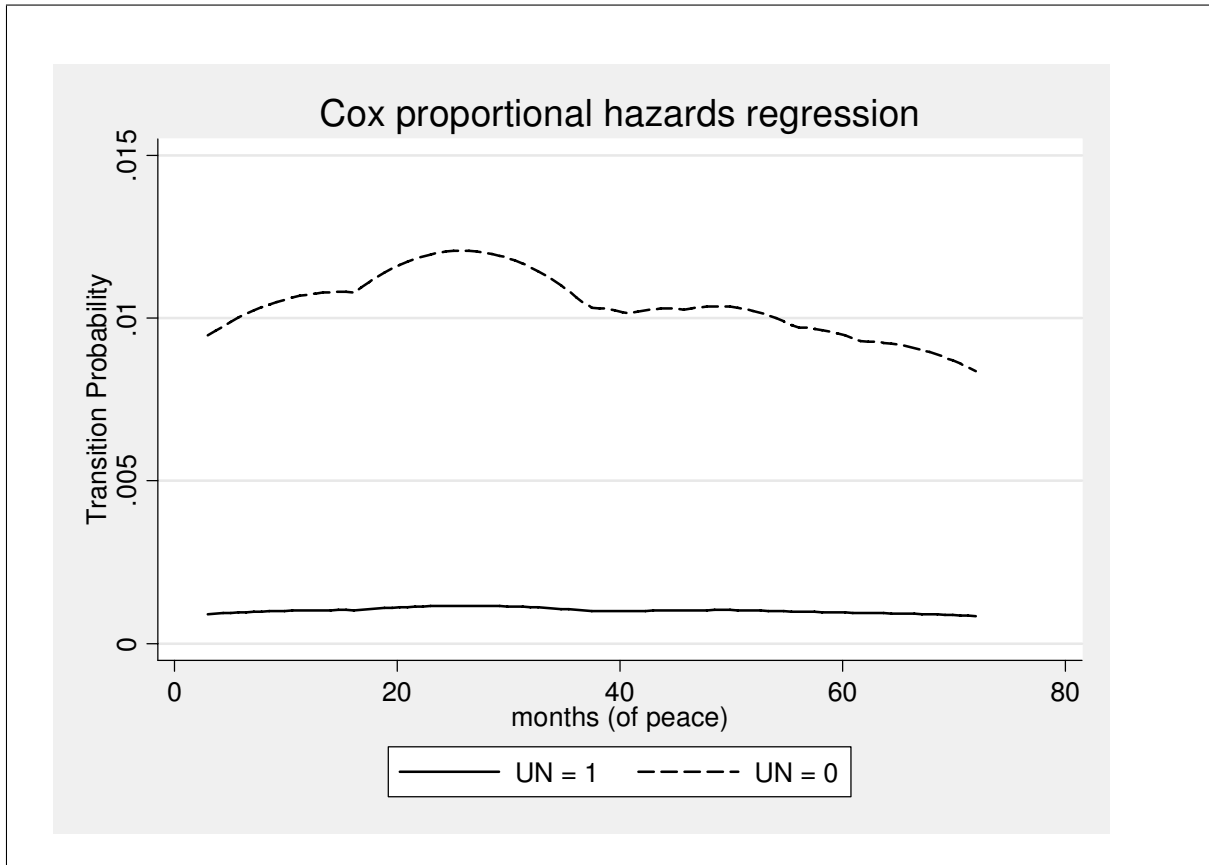


Figure 2: Probability of transitioning from peace to war estimated with a Cox proportional hazard model in a given month in cases where the UN intervened (UN=1) and where it did not (UN=0)

## 7 Web Appendix

**Note to Reviewers: This appendix is included for your information. If the paper is accepted it will be posted on the web and will not be included in the published manuscript.**

In this appendix we describe our data in somewhat greater detail. Descriptive statistics are presented in Table 6. QQ Plots for the post matched samples are shown in Figures 3 and 4.

### 7.1 Post-Conflict Sample

As mentioned in the main text our dependent variable is the duration of a period of peace following a civil war. We focus on the post-Cold War period; therefore we restrict our attention to periods of peace following the end of a civil war that existed between January 1988 and December 2003.<sup>15</sup> Our civil war data come from the Armed Conflict Dataset, Version 3.0. We use all Type 3 (Internal) and Type 4 (Internationalized Internal) armed conflicts listed in the “armedconflicts.xls” sheet.<sup>16</sup> In addition we have a full set of covariates only through December 2001 and so our sample excludes any peace periods that began after January 2002. A period of peace either ends when a civil war starts or is right censored at December 2003. In all, there were 87 such periods of which the UN intervened in 19.

We are using a country-level, not a war-level, measure of peace. We do not examine the length of peace after the end of a particular war; instead we examine the length of peace for an entire country. One complication is that some countries may experience several conflicts concurrently. These conflicts are often difficult to disentangle and are almost certainly not

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<sup>15</sup>We are aware, of course, that, strictly speaking, the Cold War was still on in 1988, however the sea change in peacekeeping that occurred with the end of the Cold War really began in 1988.

<sup>16</sup>located at <http://new.prio.no>

independent observations, which is why we have adopted the country-level approach for our dependent variable. However it does come at a cost. The clearest example of this cost is for Indonesia. During the 1990's, the Armed Conflict Dataset identifies at least two intermittent conflicts, one in East Timor and another in Aceh. Whereas the conflict in East Timor ended in 1998, the one in Aceh started in 1999. Thus, our measure is unable to pick up the post-conflict period of peace in East Timor before it became an independent country in May 2002, because by our country-level measure Indonesia remained in a state of conflict due to the conflict in Aceh. We cannot therefore evaluate the role of the UN Peacekeeping mission in prolonging peace in East Timor. Fortunately, these cases are rare. The UN mission to East Timor is the only UN mission excluded due to this issue.

Data for our treatment variable come from the United Nations Peacekeeping web site.<sup>17</sup> We examine all UN missions that address internal conflicts that ended in the period 1988 through 2002. Our sample excludes one on-going mission that was initiated before 1988, UNFICYP in Cyprus (initiated in 1964), because the post-conflict period began before January 1988, as well as any missions initiated after 2002, such as UNMIL in Liberia, UNMIS in Sudan or UNOB in Burundi. Table 7 lists all such UN missions. From these data, we created an indicator variable equal to 1 if the UN was present at any point during a period of peace, and 0 otherwise. In other words a post-conflict period either receives a UN treatment or it does not. We adopt this measure of the UN intervention because we wish to test if a UN intervention led to a lasting peace, that is peace even after UN troops left the country. Using a time-varying approach for UN intervention (that is coding UN intervention 1 only for the periods the UN was present) would only show if the UN intervention reduced the hazard rate of war while it was stationed in the country. Our measurement choice is not without its drawbacks however in that it essentially gives the UN intervention "credit" for the months

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<sup>17</sup><http://www.un.org/Depts/dpko/dpko/index.asp>

of peace before it intervened in the country. Fortunately this is not a major problem in practice. If the UN intervenes at all it almost always does so in the first month of peace. Of the 19 post-conflict UN interventions, the UN was present within the first month for 16 of them.

Our UN mission variable is a country-level variable so it does not measure the effect of any particular UN mission, e.g. UNMIH present in Haiti from September 1993 to June 1996. Instead we examine the entire effect of a continuous UN presence in a country during a particular period of peace, e.g. the total effect of the UN presence in Haiti under various different acronyms (UNMIH, UNSMIH, UNTMIH and MIPONUH) from September 1993 to March 2000. Based on our definitions of our sample and our dependent variable, we exclude three UN peacekeeping presences initiated between 1988 and 2002 from our subsequent analysis, those in Macedonia, the Central African Republic and in East Timor. We exclude the first two cases because they were neither post-conflict nor in-war interventions. We exclude the East Timor mission, first, for the reason discussed above—based on our definition of our dependent variable Indonesia does not belong in the post-conflict sample—and, second, after East Timor’s independence the conflict had to be excluded due to missing data.

## **7.2 In-War Sample**

For the in-war sample the source of our dependent variable and treatment variable are the same as discussed in the previous section. As in the post-conflict sample our dependent variable is right censored at December 2003, as this is the last date for which we have data on civil wars. In addition, we also include several variables coded by Fearon (2004). Fearon’s data set ends in 1999 and is missing several wars that are in the PRIO/Uppsala set because Fearon uses a higher battle deaths threshold for a conflict to qualify as a war. Therefore we

were forced to exclude some wars: all those that began after December 1999 and all those ongoing wars before December 1999 that were not in Fearon's dataset.

As a robustness check we also estimated our model excluding the variables that we obtained from Fearon's data set and found that our results are substantively equivalent using the larger sample that resulted from excluding those variables. We are most convinced by the results we present in this paper, however, because they control for confounding factors that Fearon found to be important, notably the availability of contraband to fund the rebel movement. Hence, we examine all conflicts that were identified by both the Armed Conflict Data set and Fearon (2004) and that were either on-going as of January 1988 or were initiated between January 1988 and December 1999. A period of war ends either when the country is no longer characterized by any civil conflict according to Fearon's data set or is right-censored in December 2003, the point at which the Armed Conflict Data set ends. In all, there were 69 such periods. The UN was present in 16 of them. As in the previous section we could not include UN missions begun after December 2002 because of data limitations of other covariates.

Table 6: Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
Post Conflict Sample, $n=87$				
UN Intervention	0.218	0.416	0	1
Log (Cumulative Battle Deaths)	7.157	3.118	0	13.229
Duration of the Last War	56.885	79.803	1	360
Ethnic Faractionalization	54.911	25.490	0.498	90.163
Log Population	9.344	1.370	6.363	12.139
Log(Mountainous)	2.347	1.387	0	4.421
Log(Military Personnel)	3.739	1.635	0.693	7.313
Log GDP per Capita	6.566	1.011	4.689	9.930
Polity 2	-1.218	5.700	-10	10
Eastern Europe	0.195	0.399	0	1
Latin America	0.138	0.347	0	1
Asia	0.149	0.359	0	1
Sub-Saharan Africa	0.402	0.493	0	1
North Africa/Middle East	0.115	0.321	0	1
In-war Sample, $n=3999$				
UN Intervention	0.136	0.343	0	1
Non-UN 3rd Party Intervention	0.126	0.332	0	1
Log(Battle Deaths)	6.749	1.754	0	10.780
Coup/Revolution	0.036	0.187	0	1
“Sons of the Soil”	0.387	0.487	0	1
Rebels Contraband Funded	0.418	0.493	0	1
Ethnic Fractionalization	55.036	26.606	0.498	90.163
Log(Population)	10.001	1.367	6.229	13.864
Log(Mountainous)	2.793	1.243	0	4.407
Log(Military Personnel)	4.384	1.472	0	7.244
Eastern Europe	0.068	0.251	0	1
Latin America	0.125	0.331	0	1
Asia	0.323	0.468	0	1
Sub-Saharan Africa	0.378	0.485	0	1
North Africa/Middle East	0.107	0.309	0	1

Table 7: UN Missions in Internal Conflicts 1988 - 2002

Country	Mission	Start Date	End Date	Post Conflict Sample	In War Sample
Lebanon	UNIFIL	Mar-78	-	1	1
Angola	UNAVEMI UNAVEMII UNAVEMIII MONUA	Dec-88 Jun-91 Feb-95 Jun-97	Jun-91 Feb-95 Jun-97 Feb-99		1
Namibia	UNTAG	Apr-89	Mar-90	1	
Central America (Nicaragua)	ONUCA	Nov-89	Jan-92	1	
Western Sahara (Morocco)	MINURSO	Apr-91	-	1	
El Salvador	ONUSAL	Jul-91	Apr-95	1	1
Cambodia	UNAMIC UNTAC	Oct-91 Mar-92	Mar-92 Sep-93		1
Croatia	UNPROFOR UNCRO UNTAES UNPSG	Feb-92 Mar-95 Jan-96 Jan-98	Mar-95 Jan-96 Dec-97 Oct-98	1  1	1 1
Somalia	UNOSOMI UNOSOMII	Apr-92 Mar-93	Mar-93 Mar-95		1
Mozambique	ONUMOZ	Dec-92	Dec-94	1	
Georgia	UNOMIG	Aug-93	-	1	1
Bosnia/Herzegovina	UNPROFOR UNMIBH	Jun-92 Dec-95	Mar-95 Dec-02	1	1
Liberia	UNOMIL	Sep-93	Sep-97	1	1
Haiti	UNMIH UNSMIH UNTMIH MIPONUH	Sep-93 Jun-96 Aug-97 Dec-97	Jun-96 Jul-97 Nov-97 Mar-00	1	
Rwanda	UNOMUR UNAMIR	Jun-93 Oct-93	Sep-94 Mar-96	1	1
Tajikistan	UNMOT	Dec-94	May-00	2	2
Guatemala	MINUGUA	Jan-97	May-97	1	
Sierra Leone	UNOMSIL UNAMSIL	Jul-98 Oct-99	Oct-99 Dec-05	1	1
Yugoslavia (Kosovo)	UNMIK	Jun-99	-	1	
Indonesia (East Timor)	UNTAET	Oct-99	May-02		1
D.R. of the Congo	MONUC	Dec-99	-	1	1
<b>Total:</b>				<b>19</b>	<b>16</b>

NOTE: UNFICYP is excluded because the war in Cyprus ended well before our sample period. MINURCA in the Central African Republic and UNPREDEP in Macedonia are excluded because there was no war in those countries prior to or during the UN involvement. UNMISSET in East Timor is excluded because of missing data. Tajikistan transitioned out and then back into war during the UN mission according to the PRIO/Uppsala data set which accounts for the two post-conflict and in-war periods recorded in the table.

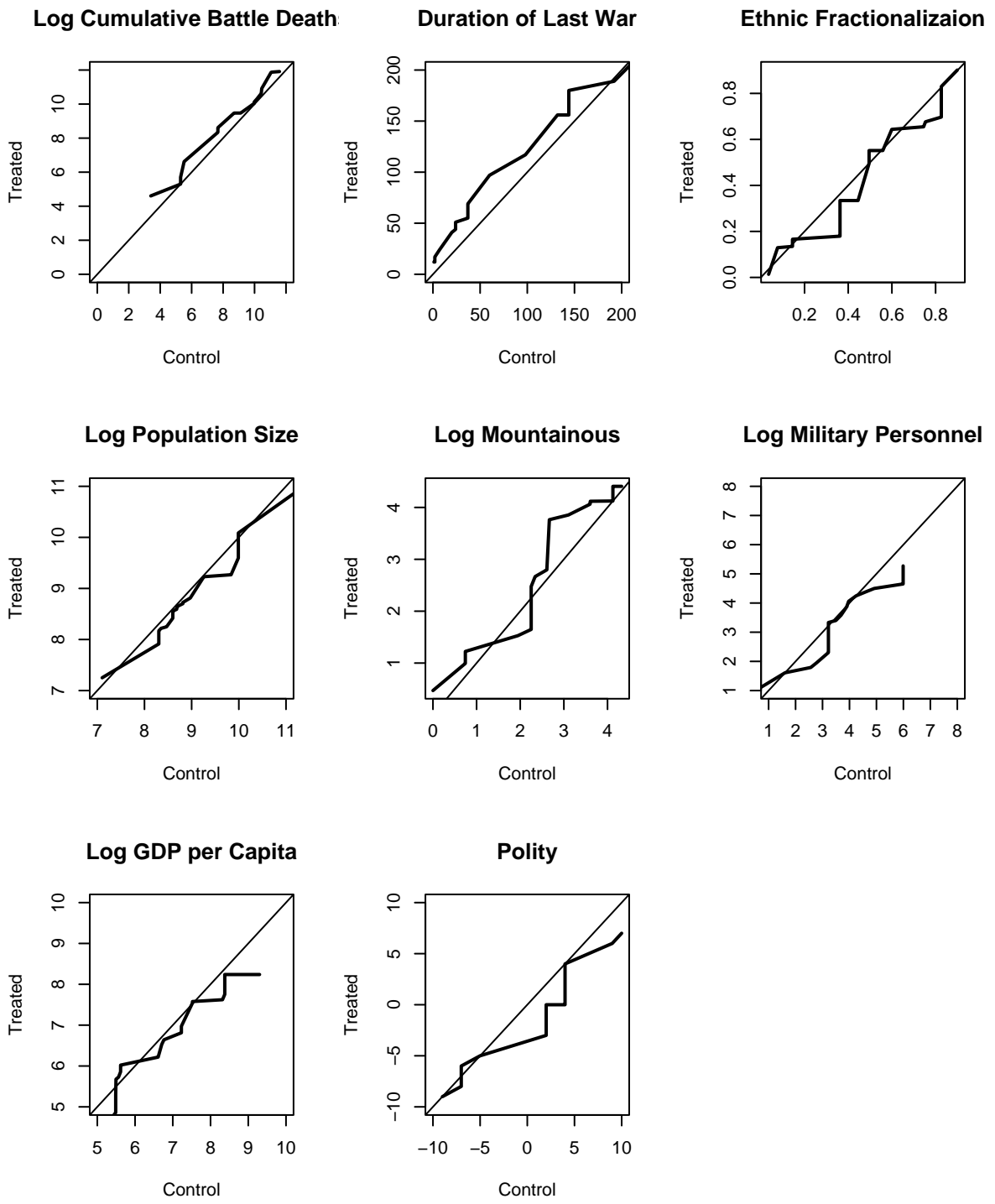


Figure 3: QQ Plots, post-conflict sample

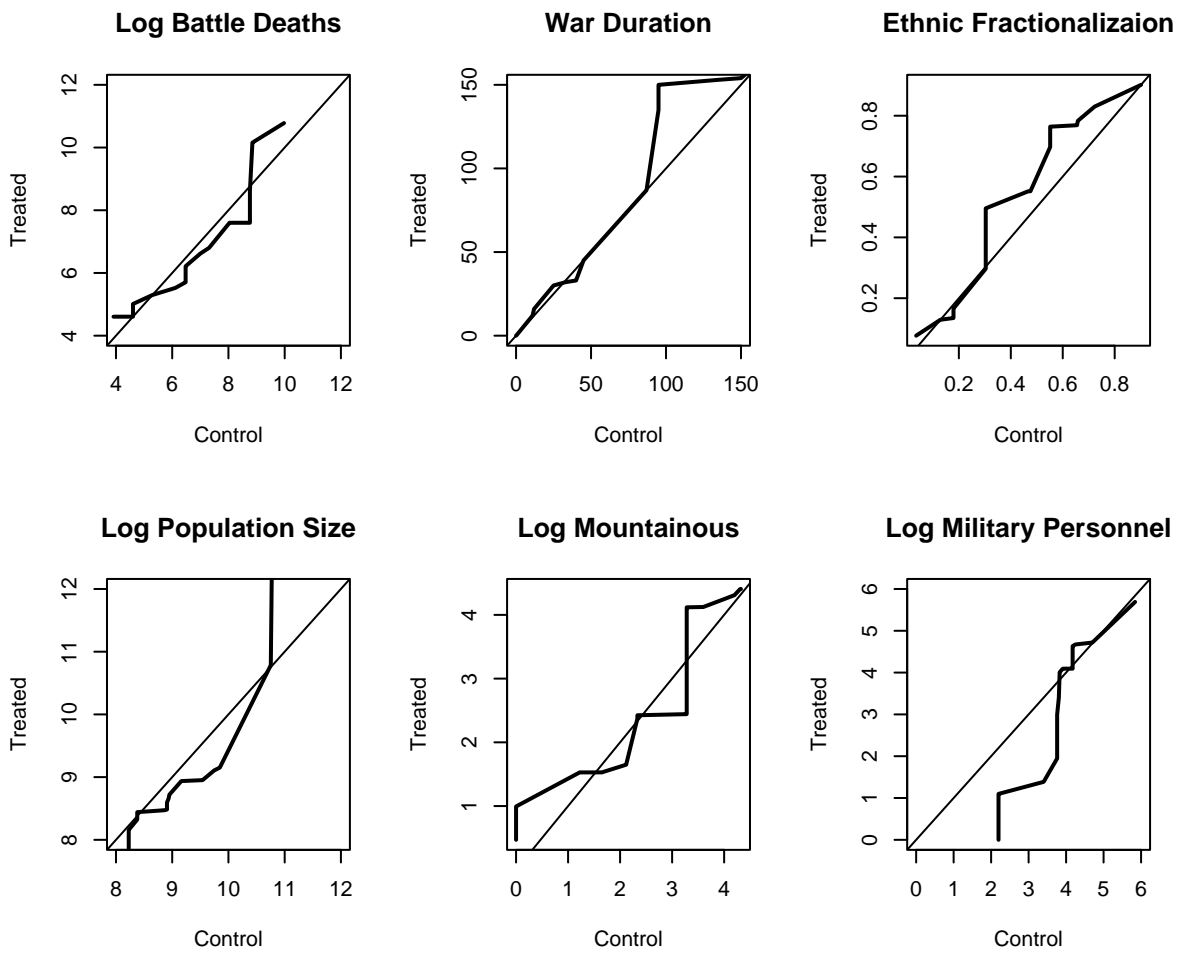


Figure 4: QQ Plots, in-war sample