WPS4207

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POST-CONFLICT TRANSITIONS WORKING PAPER NO. 11

Short-Term and Long-Term Effects of United Nations Peace Operations

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Abstract

Earlier studies have shown that United Nations peace operations make a positive contribution to peacebuilding efforts after civil wars. But do these effects carry over to the period after the peacekeepers leave? And how do the effects of UN peace operations interact with other determinants of peacebuilding in the long run? We address these questions using a revised version of the Doyle and Sambanis dataset and by applying different estimation methods to estimate the short-term and long-term effects of UN peace missions. We find that UN missions have robust, positive effects on peacebuilding in the short term. UN missions can help parties implement peace agreements, but the UN cannot fight wars and UN operations contribute more to the quality of the peace, where peace is based on participation, than to the longevity of the peace, where peace is simply the absence of war. The effects of UN missions are also felt in the long run, but they dissipate over time. What is missing in UN peacebuilding is a strategy to foster the self-sustaining economic growth that could connect increased participation with sustainable peace.

1. Introduction

In the December 2000 issue of the *American Political Science Review*, we published an analysis of the determinants of peacebuilding after civil war.[1] Our primary focus was to assess the impact of United Nations (UN) peace operations and we argued that UN operations made a positive difference, controlling for levels of war-related hostility and pre- and post-war levels of local capacities for development. We proposed a simple model in which post-conflict outcomes are determined by these three dimensions – hostility, local capacities, and international capacities measured primarily by the presence and mandate of a UN mission.

In this paper, we build on our earlier analysis in a number of ways: We propose new conceptualizations of peacebuilding success that are more consistent with the policy community's understanding of peace: successful peacebuilding is the achievement of *self-sustaining* peace; and we look beyond the short-term effects of UN missions to consider if countries that have had UN assistance do better than others over the long run. We analyze the UN's effects on a complex definition of peace that includes a modest measure of democratization, but also analyze the determinants of peace duration defined simply as the absence of war. Finally, we use new estimation strategies that establish the robustness of our results.

We present several new results. One result becomes apparent when we analyze selection issues in the assignment of UN missions and mandates: some of the factors that might lead the parties to the negotiation table also make the implementation of peace agreements more difficult. In countries with deep hostilities (after long and bloody wars) factions are more likely to sign a treaty as they realize that they cannot win a military victory, but trust is so low and local capacities so depleted that it is in precisely such complicated peacebuilding ecologies that the parties need the UN's assistance to implement a settlement. The UN can help by providing assurances, policing, and technical assistance in such cases.

Other new results refer to the long-term effects of UN missions. We find that the UN is not good at fighting wars and that the UN's effects are felt more with respect to

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[1] Doyle and Sambanis (2000). Henceforth cited as DS2000.

participatory peace than simply the absence of war. UN missions can have lasting effects

if they help keep the peace in the early stages of the peace process, when risks of a return

to war are greatest, and if they help build institutions for self-sustaining peace. But the

effects of UN missions diminish after a few years. Over time, economic growth and

development are the critical determinants of a low risk of a return to civil war.

This paper is organized as follows. In section 2, we explain how we

conceptualize peacebuilding success. In section 3, we present the model, data, and

results on the UN's effects on short-term peacebuilding outcomes. In section 4, we

discuss complications that arise from the non-random assignment of UN missions. In

section 5, we turn to the long-term analysis and in section 6, we conclude with a

discussion of the policy implications of our analysis. Throughout the paper, we refer to

many additional results that are presented in two supplements that are posted on the web.[2]

2. What Constitutes a Peacebuilding Success?

Peace can be thought of as a continuum, ranging from no peace (war) to negative

peace (absence of war) to social harmony (cf. Boulding 1964). Social harmony is an

elusive goal for most societies. What standard of peace should be the goal for societies

emerging from civil war? We argue that negative peace (absence of war) does not reflect

what is needed for peace to be self-sustaining in troubled societies. In our earlier

research (DS2000) we proposed a "lenient" and a "strict" standard for peacebuilding

success. Lenient success required an end to the war, no significant residual violence, and

undivided sovereignty. Strict success required all three of these conditions plus a

minimum standard of political openness (peacebuilding is inconsistent with extreme

authoritarianism). The lenient standard is the basis for what we call sovereign peace and

the strict standard is our measure of participatory peace. We analyze the determinants of

both negative peace and participatory peace and argue that all of the components of the

participatory peace measure help set the foundation of self-sustaining peace.

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[2] The supplements are here: http://pantheon.yale.edu/~ns237/index/research.html#Peace.
Upon publication of the article, we will also post all data files, do-files and coding notes.

Participatory peace is meaningful if it can be sustained after the peacekeepers leave. This corresponds with extensive discussions in the United Nations Security Council in which "sustainable peace" was proposed as the ultimate purpose of all peace operations and sustainability was defined as the capacity for a sovereign state to resolve the natural conflicts to which all societies are prone by means other than war (S/2001/394). "Peace-building," the report noted, "is an attempt, after a peace has been negotiated or imposed, to address the sources of present hostility and build local capacities for conflict resolution." Thus, for example, few observers think that peace has been successfully built in Kosovo today, even though Kosovo is not at war. NATO forces militarily separate the resident Kosovars and Serbs and deter both a potential attack from Belgrade to re-unify the breakaway province and a potential declaration of formal independence by the Kosovars.

We therefore evaluate peacebuilding outcomes while taking into consideration the fact that a peace that lasts without external assistance is more "true" than one that requires the UN to hold the country together. But not all civil wars have had a UN peace operation and, among those that did, the UN sometimes departed soon after the end of the war and other times stayed on for several years to manage a fragile peace process. This leads to a difficult determination of just when the war really ended as sometimes UN reconstruction and peacebuilding mandates require a presence several years beyond the end of the armed conflict. Whereas it is easier to evaluate all peace processes at the same point (say two or five years after the cessation of major hostilities), this is inconsistent with the concept of self-sustaining peace that is the goal of UN intervention.

There is no perfect way around this problem, but one way is to evaluate peacebuilding outcomes two years after the peace process starts, redefining the end to the war to include the peacekeepers' departure. Thus, a peace treaty, military victory, or the completion of a UN operation each marks the start of the peacebuilding process.[3]

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[3] Civil wars are defined as large armed conflicts between the government of a sovereign state and domestic challengers able to cause significant destruction in reciprocal violence. A more detailed definition and coding rules are given in Sambanis (2004). If a peace treaty is signed, followed by 6 months of no violence, we code an end to the war, as we do when there is a military victory or if the violence drops under the threshold of deaths that we use to code the start of a war (see Sambanis 2004 for details).

There is no *apriori* clear relationship between different war outcomes and peace processes of different lengths: wars that end in peace treaties typically last longer than wars that end in victories, but treaties may clear the way for a faster transition to peace. UN peace operations differ in their length and mandate. There is also no *apriori* clear relationship between UN peace missions of different durations and the probability of peacebuilding success: both long and short UN missions could potentially be successful, depending on underlying conditions. A long UN mission may indicate that the peacebuilding ecology is more difficult (hence the need for the UN to stay longer) and the UN obviously has more opportunities to fail the longer it stays. But the opposite can also be true: the longer the peace is kept, the more likely it is that a stable peace will be built partly because of the contribution of the UN mission. In all cases, we seek to evaluate the effect of the "therapy" on the eventual health of the "patient" country, two years after the therapy is complete. Treaties and victories are "therapies" with relatively obvious dates. A UN peace "therapy" ends when the military forces are withdrawn.

Thus, we code participatory peacebuilding success as a binary dependent variable, coded 1 if all the components of participatory peace are satisfied two years after the end of the war (including the departure of the peacekeepers). If war resumes before then, if sovereignty is divided, if there is significant residual violence, or if the polity score is below our threshold, we code a 0 (peace failure).[4] We code failures while the UN is still there – just no successes. This sets a high bar for success and we lose several cases of successful peace*keeping* – i.e. cases where there is participatory peace but the UN is still in place, hence the peace process is still unfinished.[5]

Our dataset includes peace processes after 145 civil wars from 1945 until the end of 1999. Each post-civil war transition is an observation for our statistical analysis. We exclude wars that were ongoing at the end of our analysis period (December 31, 1999) and/or cases where there was no significant peace process prior to that point. If a peace

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[4] We code a peacebuilding failure if the country's polity score is under 3 on a 20-point scale. This captures extreme autocracies and the most repressive of regimes. Such regimes are not considered successes by our peacebuilding definition, which excludes the "peaces of the grave" (the enemy is all dead or in prison).

[5] Results using the DS2000 definition, which measures outcomes at the 2-year or 5-year mark regardless of whether or not the UN is still present are somewhat stronger.

process started and failed immediately, then we code a peace failure in the first month of the peace process.[6] This leaves us with 121 cases for the statistical analysis.[7] We code 84 failures and 37 successes of *participatory peace*.

3. The Model and Results on Short-Term Peacebuilding Success

Our main hypothesis is that United Nations peace operations should contribute positively to the peace, controlling for other relevant factors. Peacebuilding outcomes are shaped by three dimensions –local capacities, postwar hostility, and international capacities. We use different measures for each dimension and conduct extensive sensitivity analyses to check the robustness of our results. We describe the explanatory variables here briefly and refer readers to Doyle and Sambanis (2006) for further discussion of the theoretical significance of these variables.

Hostility is measured by the *log of number of deaths and displacements* (*Logcost*), the *number of factions* (*Factnum*), the signing of a peace treaty (*Treaty*), the type of war (*Wartype*), distinguishing ethno-religious wars from all others, the level of ethnic fractionalization (*Ef*), and the *log of war duration* in months (*Lnwardur*).[8] Greater hostility (ethno-religious war, high ethnic fractionalization, no peace treaty, many factions, long wars, and high numbers of deaths and displacements) should make peacebuilding success less likely.

We measure local capacities with country-level indicators of socio-economic development. The overall level of development is measured by *electricity consumption per capita (Idev1)* and by the *annual rate of change in real per capita income (Gdpgro)* as well as by *real per capita income (Gdpen).*[9] Higher levels of economic development

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[6] In some cases, the UN sends troops before the war ends or already has troops on the ground when a new war starts. In such cases, if the war does not end within the two-year period, we code a peacebuilding failure. An example is the UN's missions in Angola (a new war start is coded in 1992 and 1997 while the UN is there). Results for UN missions are much stronger if these cases are dropped. Cases of enforcement missions present special challenges. In the long run analysis, we code a peace failure in the first month of deployment if the violence intensifies after UN involvement (e.g. Somalia).

[7] We lose two observations due to missing data.

[8] Summary statistics for all variables are presented in the supplement.

[9] We measure electricity consumption and real income the year before the war start since the hostility variables would have a direct effect on these measures after the war. Real

should increase the likelihood of peacebuilding success. We also measure local capacities by the degree of an economy's dependence on natural resources. Heavy reliance on natural resources reduces the likelihood of peacebuilding success as it makes the economy more susceptible to external price shocks and because resource-rich economies have been associated with corrupt political institutions. We use two measures: our first measure is *primary commodity exports as a percent of GDP* (*Isxp2*); our second measure is *oil export-dependence* (*Oil*), a binary variable coded 1 if a country's fuel exports make up more than 33% of its total merchandise exports. Local capacities are measures of institutional quality and of the economic opportunity costs of returning to war: higher capacities imply higher opportunity costs and better institutions, hence a better chance of building peace.

Deficiencies in local capacities and intense hostility can be offset by international capacities. Our key measures of international capacities are the presence and mandate of UN peace operations. Mandates measure the mission's strength, its technical and military capabilities, and the level of international commitment. Mandates are classified into *observer missions* (in which civilian officials or military officers monitor a truce or treaty); *traditional peacekeeping* (in which formed military units monitor a truce or treaty); *multidimensional peacekeeping* (in which a peace treaty authorizes international civilian officials and military units to help build or rebuild political, economic and social institutions); and *enforcement missions* with or without *transitional administration* (in which, in the absence of consent, international military forces intervene to impose peace).

We capture all UN missions with a binary variable (*any UN intervention*). We use a categorical variable (*unops*) to separate facilitative missions that basically offer monitoring and reporting (*observer* and *traditional PKOs*) from transformational UN missions that have a more intrusive mandate (*multidimensional* and *enforcement*). *Transformational UN* missions seek to transform a conflict rather than merely contain it. Finally, we distinguish consent-based missions authorized under Chapter VI of the UN Charter from enforcement missions (*Chapter 6*). These different ways to code UN

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income growth may have the same problem, but it has tremendous variability in post-civil war countries and it seems much more responsive as a measure of postwar developments.

involvement allow us to develop a nuanced argument about the conditions under which the UN can help build self-sustaining peace.

We code UN mandates based on a close reading of each mission's operational guidelines, status of forces agreements (where those were available), and a review of UN documents that indicated how much of the mandate was actually implemented.[10] We focus on UN operations because they are the predominant form of multilateral peace operation since 1945.[11]

Another obvious measure of international capacities is foreign economic assistance. Measuring the amount of economic assistance available to all countries from all sources (bilateral, NGO, multilateral) was not possible.[12] Thus, we measured instead the amount of *net current transfers per capita* to the balance of payments of the country.

The results presented here are robust to extensive specification tests: we controlled for *per capita size of government military* as a measure of the government's capacity to deter any external intervention; *Cold War conflicts* as a measure of systemic constraints to UN intervention; region-specific effects;[13] and time-trends in the data, which we capture with a variable denoting the *decade during which the war started*.

Short-Term Participatory Peace

The participatory peace model two years after the war's end is estimated using logistic regression, clustering observations from the same country. The results are presented in Models A and B, Table 1. All explanatory variables are significant and have

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[10] The results are robust to recoding ambiguous cases (Additional Results pages 22-24).
[11] We have also researched *non-UN peace operations*, but did not find evidence that they are effective (see "Additional Results", section 5). Controlling for the presence of non-UN missions does not affect the results. See Shimizu and Sandler (2002) for an analysis of burden-sharing in both UN and non-UN peace operations. They show that there is a public good component to peacekeeping as evidenced by the pattern of contributions, which tend to be disproportionately shared by large countries.
[12] For the period after 1970, we found data on *effective development assistance* as a percent of GDP. Results using this measure are "Additional Results," page 15.
[13] We did not find any regional differences in peacebuilding outcomes, but we did find that the UN is less likely to intervene in Asia and more in Europe or Africa. Thus, we used geographic region as an instrumental variable in two-stage least squares model that treats UN intervention as an endogenous regressor. UN intervention was still positive and significant in those models (supplement, pp 45-49; additional results, pp. 78-93).

the expected relationship with peacebuilding success.[14] In Model A, we control for the presence of *transformational UN* missions only and in Model B, we control for *any UN intervention*. These effects persist if we use an ordinal measure of peacebuilding success, which distinguishes "mixed" outcomes from unquestionable successes.[15]

One technical detail is that our definition of self-sustaining peace creates a *left-censoring* problem in a few cases. Since we require UN missions to leave before we code peacebuilding outcomes, in some cases we code outcomes more than two years after the end of the armed conflict. We discuss this issue at length in our supplement and show that it does not affect the results.[16] There are only 10 cases that are coded with such a time lag. One way to get at the fact that in those ten cases the UN had more time to implement its mandate is to control for the duration of all peace missions. Adding this control to the model and interacting it with UN mandates makes *transformational UN* missions less significant, though a joint significance test with mission duration overwhelmingly rejects the null hypothesis of no effect.[17] Other results are unaffected.

Using our core model and controlling for the type of UN mandate with our categorical variable *Unops*, we compute changes in the probability of peacebuilding success as a result of changes in the values of the explanatory variables while holding other variables at their means (for continuous variables) or medians (for binary or categorical variables). By going from facilitative peacekeeping (observer missions or traditional peacekeeping) to transformative peacekeeping (multidimensional, missions, enforcement, or transitional administration) the probability of peacebuilding success increases by 36% (the confidence interval ranges from 9% to 55%).

Our analysis shows that UN missions have a large, significant, positive effect on peacebuilding. This is true for *any UN intervention* and even more so for the more

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[14] In the supplement, we present bootstrapped estimates with bias-corrected standard errors and some variables are less robust (especially electricity consumption). We use three ways to measure UN intervention and all three are statistically significant. See supplement for more details.

[15] See Additional Results, pp. 17-18 and 38-39, and supplement, pp.14-16.

[16] See supplement, pages 7-11.

[17] These results are shown in the supplement. The p-value for the joint t-test is 0.01. The correlation between *transformationalUN* and its interaction with mission duration is 83%. Controlling for the duration of all UN missions makes the coefficient of *transformational UN* significant at the .001 level.

intrusive and *transformational* mandates. This result is robust to an array of specification tests and alternative estimation methods.[18]

Getting the mandate right is critically important. It is not sufficient for the UN to send large numbers of troops to the field, if those troops are not given the rules of engagement and mandate to make peace. The *number of peacekeeping troops* alone is not a good predictor of peacebuilding success.[19] We show in our supplement that there is no statistically significant difference in the *number of peacekeeping troops per square kilometer* in transformational missions as compared to facilitative missions. Thus, it is not the case that transformational peacekeeping works better because there is more concentrated force.[20] This also indicates that the Security Council often under-funds and under-resources transformational missions since they should, on average have more troops to deal with more difficult peacebuilding ecologies.

Moreover, the effects of *peacekeeping troops per square km* on the probability of participatory peace success are negative (though non-significant).[21] This might seem jarring, but it is actually consistent with our theory. A large troop deployment with a weak mandate is a sure sign of lack of commitment by the Security Council and creates an impediment for effective intervention. This result is influenced by one observation – Rwanda— where we had a large troop deployment (in per capita terms) with no authority to intervene to stop the violence. Large numbers of troops per capita in monitoring missions (*observer missions* and *traditional PKOs*) actually *reduce* the chance of peacebuilding success (examples are Cyprus, Lebanon, Rwanda). Such deployments are inefficient and potentially counter-productive. The large troop deployment with a narrow mandate in monitoring operations indicates, on the one hand, that the Security Council recognizes the severity of the conflict and, on the other hand, that it is unwilling or unable to give those troops an adequate mandate to resolve the conflict. Thus, there is often a mismatch between the problem and the treatment – better targeted mandates should improve the effectiveness of UN missions.

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[18] See "Supplement" and "Additional Results" documents.
[19] See "additional results," section 4.
[20] A comparison of the means cannot reject the null hypothesis of no difference p = .94).
[21] See Additional Results, Table S4.1 for the results that we refer to in this paragraph.

These results refer to our measure of participatory peace. But other scholars favor more restrictive definitions of peace, so in our supplement, we unpack our complex peacebuilding measure and analyze each of its components. We show that our model does not explain sovereign peace as well as it does participatory peace. Sovereign peace is more robustly associated with income growth. By contrast, we find clear evidence that transformational UN missions are more important than income growth for postwar democratization and the other components of peacebuiding.[22] Different parts of the model are better at explaining different components of peacebuilding. The model in general does not do very well in explaining war resumption two years after the war with the exception of local capacity variables: higher income reduces the risk of a new war, as do lower dependence on natural resources and less fractionalization of the society. However, a more developed and rapidly growing economy with lower dependence on natural resources is not less likely to experience divided sovereignty after civil war. Ethnic wars, by contrast, are much more likely to have peacebuilding failure due to persisting claims over sovereignty. High levels of hostility are particularly damaging with respect to higher-order, positive peace and are also more likely to lead to persistent divisions in state sovereignty. Treaties are also more important for the design of participatory peace and are generally not significant in ensuring that sovereignty will be undivided or that war and other large-scale violence will recur. UN missions are not very effective in preventing a resumption of full-scale war in the short run, but they are helpful in preventing peace failures that result from persistent divisions in sovereignty, minor armed conflict, or a failure of political institutions.[23]

We return to the analysis of negative peace (absence of war) when we analyze the effects of UN missions in the long-term. Next, we discuss how the UN's decision to intervene in some cases but not others might complicate our assessment of the effects of UN missions on short-run participatory peace.

4. Where Does the UN Intervene?

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[22] See Supplement, Table 11.
[23] For results, see Tables 5 and 6 in the supplement.

The fact that the UN does not randomly assign its missions should be reassuring to its members and interested observers, but it complicates the evaluation of the impact of UN missions. We have used three strategies to account for selection effects in the UN's decision to intervene. First, we use our knowledge of the institutional structure of the UN to consider the plausibility of arguments that the UN is selecting easy cases and to gauge the likely direction of bias in statistical estimates. Second, we use propensity score matching to estimate the effects of UN missions by comparing cases that are similar as we can find in our dataset (similarity is judged on the basis of the variables included in the model). Third, we use instrumental variables and Heckman selection models to account for possible selection on unobservables.

Accounting for the Logic of UN Intervention

Underlying the logit regression results is an assumption that that all right-hand-side (RHS) variables are exogenous. Formally this means that, though RHS variables may be correlated with other variables in the model, they are not caused by peacebuilding outcomes. This is a reasonable assumption since we code peacebuilding outcomes years after the periods which correspond to the values of the RHS variables. RHS variables must also be causally independent from each other or, if there are significant correlations, we must control for all explanatory variables that are correlated both with the RHS and with peacebuilding outcomes. This is not easy to establish and it is always possible that some variables have been omitted (though we have not been able to think of any such *exogenous* variables).

Critics might argue not only that some variables have been omitted, but also that there is logic to the UN's decision to intervene: perhaps the UN picks "easy" cases to maximize its chances of success. The opposite may also be true: the UN may go where things are really bad and its help is needed the most. Either way, cases with a UN operation may look very different from cases with no UN operation. This sort of selection can affect the estimates from our regressions, so we account for selection in a number of ways. We explain how we account for selection on observables later and focus first on the possibility that the error terms in the peacebuilding model might be correlated with

the error terms in an equation that determines if the UN will intervene (because a variable that is significant for both might have been omitted from the model).

To avoid bias in regression estimates, we can estimate a two-stage model with a "choice" equation explaining whether or not the UN will intervene and an outcome equation that is the same as our original peacebuilding model. Such a model is identified if we find an instrumental variable that explains UN intervention without also explaining the peacebuilding outcome.[24] We chose instruments among variables with a theoretically plausible relationship with UN intervention after establishing that they were not significantly correlated with peacebuilding outcomes. We used an indicator variable for whether or not the country used to be a colony of either Britain or France, as one might expect that intervention would be more frequent in countries where permanent members of the Security Council have political, cultural, or other interests. We also used the size of the government military in the civil war country, expecting that intervention might be less frequent in militarily strong countries, as the costs to the UN of a potential military confrontation would be significant.[25] We used a dummy variable indicating Cold War conflicts since the UN was much less likely to intervene during the Cold War due to the difficulty of reaching consensus in the Security Council. And we used dummy variables for different geographical regions, expecting that some regions (e.g. Europe or Latin America) would be more likely to get attention in the Security Council than others (e.g. Asia, or Africa).

None of these instruments are perfect and it is possible to think of conditions that violate exclusion restrictions. In the vast majority of cases, we could not reject the exogeneity assumption and instrumental variables estimation using these instruments produced results that were consistent with our earlier analysis.[26] Even though some of these instruments are significantly correlated with UN intervention, the R^2 in first-stage

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[24] The covariance between the instrument and the disturbance term in the outcome equation must be zero, while the covariance between the instrument and the treatment variable (UN intervention) must be different from zero.

[25] The absolute size of the military is more relevant than the per capita measure. The size of the military may potentially also affect peacebuilding, making this a poor instrument. But we find no significant correlation between military size and peacebuilding outcomes.

[26] See Additional Results, section 10 for a full presentation of the results and see our supplement (pages 45-49) for more discussion.

regressions was small and it is well-known that weak instruments can affect the size of the estimates in the second stage regressions. It was not possible to find better instrumental variables, so we cannot claim to have addressed this problem fully.

Since we cannot claim that the magnitude of the effect of UN intervention is estimated accurately, we try to gauge the direction of bias. One variable that some suggest might explain both the UN's intervention and peacebuilding outcomes is the degree of interest by major powers. The effect variable of this variable, however, should be captured by the model: the fact that the UN is there means major powers in the Security Council are interested and the degree of interest might be reflected in the mandate (more intrusive mandate implying greater interest) or in the number of troops sent (more troops implying more interest). Controlling for mandate and troops does not change the results of the analysis (see results on troops in the supplement). Similarly, the effect of UN missions remains strong when we control for mountainous terrain, political instability, and other variables that are usually included as regressors in models of civil war onset (see supplement for these results).

The direction of bias can be determined by thinking about the likely effect of the omitted variable(s) on peacebuilding outcomes and about the likely sign of the regression coefficient of that same variable on UN intervention. While a precise calculation would require us to know the variance-covariance matrix of these variables, if both these relationships are positive, then the bias will over-estimate the effects of UN missions; whereas if one or both of these coefficients is negative, the bias will be negative, leading us to under-report the effects of UN missions. Any omitted variables should help explain why the UN goes where it does go and results from first stage regressions reveal that the UN actually picks hard cases to intervene. Even casual observation makes this abundantly clear: an organization concerned about its success rate would not have intervened in the quagmire of the Congo – a vast under-developed country with deep hostility, no democratic traditions and several uncooperative factions. Thus, the effect of any omitted variables that explain why the UN goes to difficult cases would be positive on the probability of UN intervention and negative on the probability of peacebuilding success, with an overall negative bias on the estimates. Consistent with this logic, we

expect that bias from unobserved factors should work against our finding of a positive effect for UN missions and the true effect should be higher than our estimates.

Finally, we argue that UN missions could be treated as exogenous variables due to the particular institutional organization of the UN. The UN's complex decision-making process suggests that there is no simple utility-maximizing logic underlying the UN's decision to send peacekeepers to different countries. Bargaining inside the UN is too complex to respond in a straight-forward manner to a particular logic of intervention. The interests of the Security Council, the Secretariat, and the General Assembly are rarely aligned in such a way as to produce a unifying logic for the deployment of blue helmets. This explains why there is so much variation in the fit between the underlying conflict conditions and the mandate and resources given to UN peacekeeping missions (see our earlier discussion). In some cases (e.g. El Salvador, Namibia), the fit is good and the UN responds in a unified way to the challenges it faces. In other cases (Rwanda), the UN mission barely hides the major powers' indifference. There is also a large amount of agency slack in the field – review of several cases of peacebuilding suggests that the same mandate can be interpreted either as a ceiling or as a floor by different force commanders or by the same commander under different conditions in the same country.[27] Leadership decisions in the field are not easily anticipated and can lead to adaptations of the mandate over time in ways that does not reflect directly the preferences of the initial guidelines of the Security Council. The actual impact of UN operations has as much to do with how the mandate is implemented as it does with what mandate is given in the first place and the agency slack available to field commanders is the source of the independent institutional impact of UN peacekeeping.

Semi-Parametric Estimation via Propensity Score Matching

The non-random assignment of UN peace operations can generate another source of bias even if we assume that there are no omitted variables. Selection on observables implies that cases with UN missions may be different than cases without UN missions if we just compare cases across some or all of the variables included in our model. The UN may be less effective in wars with many deaths and displacements, for example. We

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[27] See our discussion of the case of Cyprus as an example in Doyle and Sambanis (2006).

discuss the conditional effects of UN missions in our supplement and do not find evidence that the model fits differently in different ranges of the data.[28] Even with a more interactive model, UN missions have a statistically significant effect.

But we have already mentioned that the UN is more likely to send troops to difficult cases. This is verified by sorting cases with and without UN missions and comparing means of some of the covariates in the two groups. This type of selection can be dealt with propensity score matching methods. The propensity score is an estimate of the probability that a UN mission will be assigned to a peace process. We get such an estimate using all other variables in the model and then divide the data in two groups –the treatment group (with UN missions) and the control group (no UN)— and compare outcomes in cases with similar propensity scores. Since all other covariates are used to estimate the propensity score, differences in outcomes are attributed to the "treatment" (the UN operation). Matching estimation assumes that all the right variables have been identified and are exogenous and there can be several ways to balance the two groups, and different balancing rules can affect the results. The results we report here are based on widely used programs for matching (see supplement for a more detailed discussion).[29] We report average treatment effects on the treated (ATT) for *transformational and* any *UN intervention* using four matching methods.

Out of 119 observations with non-missing observations, there are 56 cases in the range of common support for the model including *transformational UN missions*. The average treatment effect is substantial and stable, ranging from .46 to .75 and it is statistically significant using all four matching methods. However, nearest neighbor matching yields only a few matches (there are 8 treated and 5 control observations on common support), so parameter estimates will necessarily be more uncertain. The effects of *any UN intervention* are also strong (the ATT ranges from .26 to .42). The range of common support is now larger and includes 89 observations. We find similarly strong results for UN missions using other matching estimators.[30] In our supplement, we

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[28] Supplement, pages 36-40; Additional Results, section 9.

[29] We used Becker and Ichino's (2002) program in stata for matching estimation. The seed number used for sampling observations in the bootstrap was 123456789.

[30] Additional Results, sections 11 and 12. For a fuller discussion and several sources on matching. For a more extensive discussion, see our comment on Gary King and Langche

discuss matching estimates of UN peacekeeping in greater detail and also highlight some possible problems with the application of this method to our data.

Selection on the Signing of a Peace Treaty

A point that deserves special attention is that peace treaties are usually necessary for certain UN mandates, especially multidimensional ones. Treaties and UN missions work in concert, enhancing the chances for peace.[31] But since treaties are sometimes necessary for the UN to send troops, we estimated the effects of UN operations on participatory peace while selecting on treaty and on war termination.[32] An interesting new result is that some variables that lead the parties to sign a peace treaty also make the implementation of the peace harder. Longer wars and high levels of deaths and displacements increase are two examples. This is consistent with rational choice models of war, since a long and bloody war should resolve uncertainty over the parties' relative resolve and capabilities, helping them realize the benefit of a settlement. But these variables have a negative coefficient in the participatory peace equation, indicating the peace implementation is actually harder after long and bloody wars, consistent with our hostility hypothesis. Thus, we argue that it is precisely in these circumstances that the UN is useful: it can help parties implement the peace when peace is in their interest, despite a high degree of hostility.

5. How Durable Is the Peace?

The analysis so far has demonstrated that UN missions have a positive impact in the short term. Imposing a 2-year or 5-year cutoff point is not wrong and our two-year results also hold for a 5-year period. Policymakers are interested in seeing if policy interventions have an impact in a reasonable amount of time. We ask a different question in the short-run analysis (has UN intervention paid off after two or five years?) than we would in a long-run analysis (does UN intervention contribute to long peace durations?). We addressed the former question thus far and now we turn to the latter.

Zeng's application of matching to estimate the effects of peacekeeping: http://pantheon.yale.edu/~ns237/index/research/KZ_Response.pdf (pages 24-36).
[31] See Additional Results, Table S9.1.
[32] See supplement (Table 10, page 52) for the results and more discussion.

In some cases, the peace might fail soon after the two-year cutoff point and as with many interventions, we would expect the benefits of UN missions to be concentrated in the short run. In the long run, new challenges develop that can undermine the peace that may be unrelated to the previous war or to the peace process. But countries that have had some UN assistance during their peace processes should have a better chance at achieving self-sustaining peace, largely because of the much needed help they receive in the critical first years of their transition from war.

Survival analysis can be used to analyze the duration of the peace in the longer term. Survival models estimate the "hazard" (or risk) of peace failure at time t given that failure has not occurred until then and it can account for right-censoring (the fact that the peace has not failed up to the end of analysis time does not mean that it cannot fail afterwards).[33] The dependent variable (*peace duration*) is coded as a continuous variable in months, counting from the start of the peace process until the peace fails or up to a censoring point, at the end of December 1999. Peace failure implies that a new civil war starts in the country. This is a departure from the complex definition of participatory peace used up to this point. We now focus on war recurrence (we discuss the duration of participatory peace in our supplements). We begin with the same single-record single-failure dataset used this far and then switch to a time-series cross-section (TSCS) version of the data. The model must by necessity be somewhat different for the TSCS data analysis since we could not code time-series for all the variables from the original model.

War Recurrence as Peacebuilding Failure

Our analysis of the long-term effects of UN missions focuses on the negative definition of peace as the absence of war. This is a fairly common standard in the literature (cf. Fortna 2004) that we use because our measure of participatory peace cannot be coded accurately as a continuous variable. There are no reliable data on levels of residual violence over time and levels of political openness can vary over time, so it is theoretically possible that some cases would "fail" according to our participatory peace measure, then "succeed" a few years later, and then "fail" again if their polity score dipped below our threshold. Coding undivided sovereignty is also tricky without some

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[33] For a methodological discussion, see Box-Steffensmeier and Jones (2003).

sort of deadline: since peace processes often are designed to resolve persisting problems of divided sovereignty gradually, we would have to decide how we would treat cases where sovereignty was still divided a few months into the peace process, but there was gradual improvement until there was undivided sovereignty a year or two into the peace process. These considerations illustrate the usefulness of a short-run cutoff point that allows us to ask: has the UN missions helped resolve these issues in a given time frame? The supplement presents survival analysis estimates that use a definition that is consistent with the participatory peace measure, and we explain the judgment calls that we made in our coding rules. The results are highly consistent with the results of the short-term participatory peace analysis. But since the long-term measure of war recurrence is both conceptually and empirically clearer, we focus on war recurrence here.

In the single-record, single-failure dataset, out of 138 "subjects" (peace processes), we have 73 failures (*peacend*) with mean peace duration of 53 months and the longest peace duration in the dataset is 634 months. Failures cannot occur at time t = 0, but there are several failures of the peace in the first month. Our main concern is to explore the impact of UN operations on war recurrence.

We estimate the core model from Table 1 using a Cox Proportional Hazards (PH) model. Given its greater versatility, the Cox model is a better initial choice than the more frequently used Weibull model or other parametric hazard models. The Cox model gives the hazard rate for the i'th individual as $h_i(t) = h_0(t) \exp(\beta'\mathbf{x})$, where h_0 is the baseline hazard rate and $\beta'\mathbf{x}$ are the estimated coefficients and covariates. It assumes a proportional hazard rate and utilizes the ordered duration times to derive estimates for the regression covariates. We test the proportional hazard assumption and, if it is not satisfied, we shift to another model, such as the Weibull model, which is appropriate if the hazard rate is monotonically increasing or decreasing.

Model 1 (Table 3) is the core model, with added controls for real per capita income, the rate of growth of real income at the end of the war, and the level of ethnic fractionalization. These three variables did not have a significant association to participatory peace in the short run, so they were not included in previous Tables, but since we are now using a different concept of peacebuilding and since there are several arguments in the literature linking them to civil war onset, we add them to the model.

UN intervention is significant (p=0.039) level and reduces the risk of peace failure by about 50%.[34] But, the strongest result, consistent with much of the literature on civil war onset, is that local capacities are critical in determining proneness to a new war outbreak. Local capacity variables now take away some of the effect of the hostility variables (only deaths and displacements is significant and this is not very robust). Countries with higher levels of income, fast-growing postwar economies, and lower dependence on natural resource exports are far more likely to experience longer peace durations despite the negative effects of postwar hostility. The effects of variables like the number of factions or the nature of the war (was it ethnic or not?) are non-significant as we might expect any impact that they have to be limited to the immediate postwar period.[35] High fractionalization has a significant (p=0.007) negative effect on peace duration, which lends support to our hypothesis about the difficulties of achieving long-lasting peace in fractionalized countries after civil war.[36]

The results on local capacities are quite robust to small specification changes. By contrast, the effects of UN intervention are less robust. One problem is that UN enforcement missions seem to have negative effects on peace duration, probably due to the very challenging circumstances in which they are employed and because they are designed to end wars and not build long-lasting peace. Thus, in Model 2, we used a variable that identifies only consent-based UN missions (dropping enforcement missions) and found consent-based missions to be highly significant (p=0.044) and positive with respect to peace duration (the odds ratio in Model do for Chapter VI missions is under 1, reducing the risk of peace failure by about 50%). This effect is not reduced when we control for the outcome to the previous war, and we find that both negotiated settlements

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[34] This effect is robust to controlling for non-UN missions.

[35] We drop *treaty* from the model on the basis of a likelihood ratio test. A test of the proportional hazards (PH) assumption for this model cannot reject the null (p = 0.13). Re-estimating the model assuming a Weibull distribution improves the results slightly, as does estimating the model using Cox regression with interactions with the log of time for two variables that come close to failing the PH test (see supplement).

[36] Models of civil war duration also find a significant association between greater fractionalization and longer duration (Fearon 2004) and the effect may be nonlinear (Collier, Hoeffler, and Soderbom 2004). This is in contrast to models of war onset. Thus, consistent with the nationalism literature, ethnic differences seem to be "activated" to support mobilization for violence once conflict levels are high (during and after war).

and military victories lead to longer peace durations than less decisive outcomes, such as truces or military stalemates. In Model 2, we also replaced the generally non-significant *electricity consumption per capita* variable with the *log of per capita real income* measured before the start of the war. Income is positively associated with longer peace durations (p=0.054) and this is consistent with the results from the larger literature on civil war onset. But a test of the Proportional Hazard assumption now shows that the PH assumption is not satisfied in Model 2.[37] We re-estimated the model using Weibull regression (Model 3), which produced substantively similar results and showed that the risk of war recurrence has negative duration dependence (i.e. peace becomes more stable as time passes). To account for the fact that exposure to the risk of peace failure is higher in countries where the war ended early in our analysis period, we added controls for the decade during which the peace process started (Model 4). This marginally satisfied the PH assumption underlying the Cox model and improved some of our results and made *deaths and displacements* highly significant (p=0.008).[38]

These results highlight the importance of including economic rehabilitation in a peacebuilding mandate and dovetail with recent findings in the literature on civil war that demonstrate the power of these variables in influencing the risk of war onset. Scholars of civil war use income per capita as a measure of state strength and it has been shown that heavy dependence on oil results in authoritarian state structures. Thus, our results may suggest that postwar authoritarianism and state weakness increase the risk of war recurrence. We also found that consent-based UN missions have a positive impact, but their impact is overshadowed by local capacity variables. Thus, a war-prevention strategy for the UN in countries that are emerging from civil war should be to help build institutions that resist the corrupting pressures of resource-dependent economies and allow fast economic growth. The UN's impact in rebuilding institutions will be

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[37] We therefore re-estimated the model by adding time interactions for those variables that failed the test and the results improved. The hazard ratio for per capita income dropped substantially and was significant at the p=0.008 level. The results on UN consent-based missions were also stronger. See supplement.

[38] The χ^2 test of the PH assumption can only narrowly not be rejected (p-value = 0.055). The results are similar using a Weibull model (see supplement).

particularly important in ethnically divided societies which are at higher risk of a return to civil war.[39]

We now turn to an analysis of peace failure using the time-series cross-section version of our dataset.

Analysis of the Hazard of Peace Failure Using Time Varying Covariates (TVCs)

We can think of our dataset as a multiple-event, multiple-failure dataset with both ordered and unordered events (peace failures). The dependent variable is again months until peace failure. Since we often have multiple peace failures in a country and our cross-sectional unit is the peace process (*cnumb*), we add multiple records for each cross-section, making the use of clustering on country necessary, since multiple records of country-level variables will be the same across conflicts. Our unit of analysis is the peace process-year, not the country-year as is typical with binary time-series cross-sectional (BTSCS) datasets in international relations and comparative politics. We discuss the method in more detail in the supplement and present key results below.

Even though the model is now slightly different, we nevertheless include measures for all three concept variables – hostility, international and local capacities. We estimate a Cox PH model controlling for *lagged log of per capita GDP, lagged oil export-dependence,* the *log of deaths and displacements, ethnic war, signed treaty, level of ethnic fractionalization, population size* and our UN intervention variables.

The presence of any UN mission (*anyun*) is our focus. This is a binary variable coded 0 if there was no UN mission and 1 if there was a UN mission, starting at the year of initial deployment and coded as q until the peace either fails or analysis time runs out.

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[39] In the supplement, we present several more robustness tests. We utilize different coding of peace duration, based on different assumptions about which cases of war resumption represent war "recurrence" and which are "new" wars. It is not always easy to make those distinctions. We also drop cases that some scholars might not characterize as civil wars, but rather as different forms of violence. The results are generally robust to those changes, though there are some differences when we use the alternative peace duration variable (see supplement). The logistic regression estimates are robust to all those coding changes.

This allows us to study the effect of UN missions even after the UN departs.[40] Thus, the variable *anyun* distinguishes those cases that had some form of UN assistance at some point in their peace transition from those cases that had no UN assistance. We can also use a different version, coding a "1" only after the UN departs and for all subsequent years. But here we again would lose many cases as we would have drop all observations with ongoing missions. The difference in the results between these two variables might reveal something about the difference in the effectiveness of UN missions in peace-*keeping* as compared to peace-*building*. We lag both versions one year. Table 4 presents the results.

Lagged per capita GDP in logs is very significant (p=0.005) and lowers the hazard of peace failure substantially, whereas higher ethnic fractionalization (p=0.01) and more deaths and displacements (p=0.03) significantly increase the risk that the peace will fail (Regression 1). The other variables in our model, oil export-dependence, signed treaty, population size and ethno-religious war do not have a significant effect.

UN missions are weakly significant (p-value=0.056), as was the case with the single-record, single-failure data. But this effect is more significant (p=0.025) if we exclude cases where the peace has lasted for 10 years or more in an informal "splitting" model (see supplement), a result which would indicate that the UN's effects are greater in the first few years of the peace process. An interpretation of this result that is consistent with our arguments is that the UN has actually become better at peacekeeping over time, since by dropping cases with peace durations longer than 10 years we are more likely to select against cases early in the data.[41] The significance of the UN variable also improves (p=0.026) if we run a stratified Cox model in Regression 2 (the variable *pstrata* identifies the number of civil wars in the country.

A theoretical argument can be made that the hazard of war recurrence should be decreasing with time spent at peace (see Collier and Hoeffler 2004). Thus, even though

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[40] Instead of this coding rule, we might code UN missions only for those years where they were actually present. But in this case, if the peace lasts beyond the departure of UN troops, the model would lead us to find a negative association between UN interventions and self-sustaining peace as an artifact of the coding (by construction, there would be no observations of self-sustaining peace coinciding with UN presence).

[41] We thank an anonymous reviewer for making this point.

we cannot reject the PH assumption, the Weibull model may be more appropriate and we estimate such a model in Regression 3. The substantive results remain unchanged and we do find negative duration dependence, confirming the intuition that the few years immediately following the end of the civil war have the highest risk of war recurrence. Given that the effects of UN peace missions have their greatest effect in the short run, this suggests both that their long-term effect on the durability of the peace would be due to successful peace implementation during the first few years of transition that can help set the stage for self-sustaining peace in later years.

We look at the impact of different types of UN mandates separately, but we find that *Multidimensional mandates* cannot be included in the regression because they predict peacebuilding success perfectly. We saw earlier that *enforcement* missions increase the risk of a new war, but there have been very few of those missions so the results will not be robust and there may be a selection effect here (war is often not over when enforcement missions are sent). Thus, we drop enforcement and focus on all consent-based operations in Regression 4, where we see that they have a significant effect (p=0.04) in reducing the risk of a new war.[42] We cannot push the analysis of the different mandates much further using these methods, given the small number of observations for each type of mandate.

Might it be the case that longer-lasting UN intervention reduces the risk of peace failure? We tested this hypothesis by creating an interaction between mandate type and the duration of UN missions (*pkodur*). In Regression 5, we find that longer-lasting *multidimensional* missions (*multitm*) do have such an effect, which is as we would expect, since rebuilding institutions and enhancing local capacities takes time (this suggests that a quick exit is not likely to be a good strategy on average). By contrast, longer-lasting *traditional peacekeeping* (*tradtm*) does not make a difference, consistent with the fact that such missions do monitoring rather than transform a conflict and may well merely be monitoring a political stalemate.

To explore further how lasting the influence of UN operations is, we used several time lags of UN intervention (*anyun*) and coded another version (*anyund*) that takes the value of 1 only *after the UN departs,* which should tell us more about the effect of UN

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[42] This result is robust to adding year dummies and controls for the geographical region.

intervention and self-sustaining peace. Using Weibull regression, we find that lagged UN interventions are weakly significant (Regression 6), but become non-significant after the second lag. It may not matter how a sustainable peace is achieved. But achieving it, whether by domestic or international means, does make the long-run difference. If we control for UN intervention only after the UN has left (Regression 7), there is no effect (though results are close to statistical significance with other methods).[43] The UN's positive contribution with respect to avoidance of war recurrence is limited to the short-term and primarily to the period while the UN is still present. However, this is because all mandates are lumped together in this variable, including enforcement. If we look separately at consent-based peace operations, then we find more lasting effects (p=0.068) even after these missions depart (Regression 8), though these effects become non-significant after the second lag (Regression 9).

6. Conclusion

This paper provides an empirical investigation into the impact of United Nations peace operations after civil war. We evaluate the effects of UN missions on self-sustaining peace both in the short term and in the long term. We argue that UN missions have robust positive effects on peacebuilding outcomes, particularly participatory peace. But peace interventions have effects mainly in the short run and the effects are stronger when the peacekeepers are there. This is reassuring since, the longer a peace lasts, the more stable it becomes, so interventions that shore up the peace in the immediate postwar period can have a lasting effect. The best predictor of participation and peace tomorrow is participation and peace today.

Economic factors drive the long-term prospects of peace in the average post-civil war country. This links our results to prevailing opinion in the quantitative literature on the causes of civil war. But we also find that long-term peace can be assisted by the presence of UN missions with a mandate to monitor and police cease-fires and rebuild institutions. Simply sending large numbers of troops will not solve the problem – they are often sent to the worst crises and need to have a well-crafted mandate. Economic assistance alone is also unlikely to do the trick – while economic factors may be more

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[43] See Additional Results (section 15) for results from random effects probit models.

significant for the prevention of war resumption, more is needed to rebuild political institutions and implement complex peace agreements in the early years of postwar transitions. The value-added of UN missions is in precisely playing that vital role in transitions from civil war.

While countries that have had a UN mission tend to recover better from their civil war than countries that have not had a UN mission, this effect wanes after a few years. Over time, economic development is more important for self-sustaining peace, yet experience suggests that most UN missions do not appear to make a significant difference with respect to this critical determinant of peace. Good as UN peacebuilding is in expanding political participation, it has not served to jump start self-sustaining economic growth. Time and time again, economic reconstruction is treated as a follow on to peace, when in fact it is a vital partner. It provides the visible benefits of peace that help mollify hostilities, the civilian jobs that absorb demobilized soldiers, the tax revenues that strengthen state capacity. In the most significant circumstances, it funds the national army that sustains public order rather than the looted resources that sustain the factional armies that challenge it. It pays for the soldiers who are demining rather than laying mines. In addition to being a significant determinant of sustainable peace, growth is also a determinant of sustainable democracy.[44] Efforts are under way to fill the policy gap between peacekeeping, with its humanitarian assistance, and development assistance, with its emphasis on structural transformation. Evidence of this can be found in the recent creation of a UN Peacebuilding Commission. One strategy that the Commission should follow, consistent with our results, is to coordinate UN peacekeeping interventions with development assistance offered by the international financial institutions, such as the World Bank. There is a lot to learn – no recipes are currently available for the right mix or sequencing of security and development strategies in countries emerging from civil war. But the international community would clearly benefit from an evolution that made economic reform the additional element that plugged the decisive gap between peacekeeping, humanitarian assistance, and development.

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[44] Przeworski, Alvarez, Cheibub, and Limongi (2000); Collier and Hoeffler (2004).

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Table 1 – Logit Models of Participatory Peace, Two Years after the War

Reported: coefficients and robust standard errors (in parentheses); estimates in bold are significant at least at the 0.05 level; Estimates in italics are significant at the 0.05 level with one-tailed test

	Model A	Model B
Ethnic War	**-1.5885**	**-1.6075**
(*Wartype*)	(0.5110)	(0.4952)
Log Deaths & Displaced	**-0.3179**	**-0.3392**
(*Logcost*)	(0.1370)	(0.1391)
Number of Factions	**-0.6074**	**-0.5686**
(*Factnum*)	(0.2291)	(0.2699)
Net Transfers per capita	**0.0388**	**0.0275**
(*Transpop*)	(0.0118)	(0.0118)
MultiPKO & Enforcement	**3.1039**	--
(*TransfUN*)	(1.0290)	--
Any UN intervention	--	**1.9247**
(*Unintrvn*)	--	(0.6118)
Signed Peace Treaty	**1.5799**	**1.6153**
(*Treaty*)	(0.6654)	(0.6643)
Electricity Consumption	**0.0562**	0.0422
Per capita (*Idev1*)	(0.0281)	(0.0282)
Primary Commodity	**-7.7346**	**-7.8967**
Exports/GDP (*Isxp2*)	(2.1829)	(2.2121)
Constant	**5.3226**	**5.4447**
	(1.5400)	(1.5529)
Observations:	119	119
Pseudo-R^2	33.54%	32.22%
Log-Likelihood:	-49.02	-49.99

Table 2 – Matching Estimates of UN Operations' Effects on Participatory Peace
ATT estimates, bootstrapped standard errors & t-statistics, and bias-corrected 95% confidence intervals
All matches made on common support

Transformational UN mission	Treated	Controls	ATT	St. Err.	t-stat	95% conf. interval	
Nearest Neighbor	8	5	0.750	0.237	3.162	.5	1
Radius (r=1)	8	48	0.458	0.183	2.503	.045	.76
Kernel	8	48	0.620	0.189	3.278	.294	.971
Stratification	8	48	0.589	0.197	2.990	.157	1
Any UN Intervention							
Nearest Neighbor	26	18	0.423	0.119	3.550	.2	.692
Radius (common support)	26	63	0.262	0.114	2.302	.040	.455
Kernel	26	63	0.384	0.107	3.572	.180	.653
Stratification	24	65	0.383	0.127	3.014	.114	.641

Table 3 – Duration Models of the Hazard of War Recurrence
Reported are hazard ratios & coefficient robust standard errors; Bold indicates significance at the 0.05 level; italics indicate significance at the 0.05 level with one-tailed test

	Model 1	Model 2	Model 3	Model 4
Ethnic War	1.14	0.996	0.95	1.12
	(0.31)	(0.27)	(0.29)	(0.31)
Dead & Displaced (log)	**1.14**	*1.12*	1.13	**1.20**
	(0.065)	(0.070)	(0.077)	(0.08)
Number of Factions	1.04	1.06	1.06	1.01
	(0.099)	(0.08)	(0.085)	(0.07)
Net current transfers	0.999	0.999	0.999	0.999
	(9.18e-07)	(1.13e-06)	(1.22e-06)	(1.01e-06)
Ethnic fractionalization	**3.78**	**3.81**	**3.94**	**4.32**
	(1.88)	(1.98)	(2.11)	(2.32)
Electricity consumption	0.999	--	--	--
	(0.0002)	--	--	--
Real GDP growth	**0.96**	**0.96**	**0.96**	**0.96**
	(0.012)	(0.014)	(0.015)	(0.014)
Real GDP (log)	--	**0.78**	*0.79*	*0.74*
	--	(0.10)	(0.11)	(0.13)
Primary Commodity Exports/GDP	**3.52**	**3.29**	*2.92*	2.38
	(1.90)	(2.00)	(1.79)	(1.42)
Any UN Intervention	**0.54**	--	--	--
	(0.16)	--	--	--
UN Chapter VI Missions	--	**0.48**	**0.47**	**0.41**
	--	(0.17)	(0.18)	(0.16)
Negotiated Settlement	--	**0.43**	**0.37**	**0.33**
	--	(0.17)	(0.14)	(0.13)
Military Outcome	--	**0.54**	**0.50**	**0.38**
	--	(0.18)	(0.16)	(0.12)
40's peace st.	--	--	--	*0.22*
	--	--	--	(0.197)
50's peace st.	--	--	--	**3.37**
	--	--	--	(1.80)
60's peace st.	--	--	--	1.30
	--	--	--	(0.50)
70's peace st.	--	--	--	0.80
	--	--	--	(0.33)
80's peace st.	--	--	--	0.57
	--	--	--	(0.33)
Time dependence (p)	--	--	**0.62**	--
	--	--	(0.055)	--
Observations	129	131	131	131
Number of failures	69	70	70	70
Log pseudo-likelihood	-267.95	-268.91	-182.74	-260.32
Wald χ^2 (d. f.)	73.73	102.88	108.93	154.32
	(9 d.f.)	(11 d.f.)	(11 d.f.)	(16 d.f.)

Table 4 – Duration Models of the Risk of War Recurrence with Time-Varying Covariates

Reported are hazard ratios & coefficient robust standard errors; Bold: significant at the 0.05 level; italics: significant at the 0.05 level with one-tailed test

	1	2	3	4	5	6	7	8	9
Ethnic War	1.391	1.402	1.283	1.284	1.199	1.290	1.221	1.201	1.209
	(0.422)	(0.436)	(0.410)	(0.411)	(0.401)	(0.412)	(0.400)	(0.396)	(0.396)
Ethnic Fraction.	**2.973**	**3.119**	**3.222**	**3.109**	**3.409**	**3.268**	**3.228**	**3.066**	**3.104**
	(1.293)	(1.387)	(1.420)	(1.340)	(1.570)	(1.445)	(1.449)	(1.356)	(1.384)
Dead/Displaced (log)	**1.130**	**1.178**	**1.145**	1.148	1.127	1.146	1.130	1.131	1.129
	(0.065)	(0.064)	(0.065)	(0.066)	(0.064)	(0.065)	(0.063)	(0.064)	(0.064)
GDP pc (log)	**0.670**	**0.707**	**0.707**	**0.710**	**0.690**	**0.705**	**0.704**	**0.708**	**0.702**
	(0.095)	(0.099)	(0.107)	(0.109)	(0.102)	(0.108)	(0.099)	(0.097)	(0.097)
Population (log)	0.909	0.928	0.912	0.909	0.947	0.910	0.934	0.935	0.936
	(0.077)	(0.087)	(0.073)	(0.073)	(0.076)	(0.073)	(0.070)	(0.070)	(0.070)
Oil exports	1.597	1.468	1.456	1.473	1.420	1.475	1.375	1.373	1.393
	(0.543)	(0.434)	(0.546)	(0.566)	(0.457)	(0.562)	(0.434)	(0.423)	(0.437)
Treaty Signed	1.411	1.453	1.194	1.241	1.066	1.174	1.180	1.256	1.199
	(0.499)	(0.501)	(0.405)	(0.417)	(0.352)	(0.409)	(0.409)	(0.420)	(0.401)
UN Intervention	*0.483*	**0.446**	*0.490*	--	--	*0.470*	0.349	--	--
	(0.184)	(0.162)	(0.195)	--	--	(0.202)	(0.247)	--	--
Chapter VI UN ops.	--	--	--	**0.418**	--	--	--	*0.177*	0.218
	--	--	--	(0.180)	--	--	--	(0.168)	(0.211)
MultiPKO interacted	--	--	--	--	0.449	--	--	--	--
with PKO duration	--	--	--	--	(0.036)	--	--	--	--
TradPKO interacted	--	--	--	--	1.001	--	--	--	--
with PKO duration	--	--	--	--	(0.003)	--	--	--	--
Time-dependence (p)	--	--	**0.618**	**0.620**	**0.617**	**0.623**	**0.629**	**0.635**	**0.633**
	--	--	(0.059)	(0.059)	(0.060)	(0.059)	(0.059)	(0.062)	(0.062)
Observations	1323	1323	1323	1323	1323	1323	1323	1323	1323
# Subjects	122	122	122	122	122	122	122	122	122
# Failures	64	64	64	64	64	64	64	64	64
Wald χ^2 (d. f.)	50.99	46.42	46.15	44.64	167.93	45.48	45.42	39.65	37.94
Log Likelihood	-246.01	-168.99	-171.47	-170.95	-172.13	-171.40	-171.65	-170.64	-171.28