Living Up to the Name: Do UN Peacekeeping Operations Actually Lead to Peace?

**The Literature on PKOs and Peacebuilding**

Overwhelmingly, two decades of research have suggested a general conflict-reducing effect of third-party peacekeeping operations (PKOs) (Fortna and Howard 2008, Dorussen 2014, Walter et al. 2021). Indeed, this relationship has been established across a wide variety of operationalizations of peace, such as the rule of law (Blair 2019), economic recovery (Bove et al. 2021), inter-ethnic trust (Mironova and Whitt 2015), battle deaths (Vivalt 2015), the risk of conflict onset (Hegre et al. 2018), the risk of conflict recurrence (Fortna 2004, Quinn et al. 2007, Collier et al. 2008, Mason et al. 2011), civilian casualties (Hultman et al. 2013), and mass killings (Melander 2009). PKOs have been theorized to promote peace through their capacities to stop ongoing violence, to prevent formerly warring parties from re-engaging in conflict, and to address structural causes of violence.

In this first aspect of peace promotion, PKOs are theorized to serve as a "buffer" between groups engaged in ongoing violence. If a physical buffer is placed between warring parties, the chances of accidental engagement between groups that might disrupt the conflict termination process decreases. For those groups that may not be committed to the conflict termination process, PKOs make the process of reneging on ceasefires much more costly as PKOs can monitor which actors are violating a ceasefire. In addition, the mere presence of a PKO serving as a buffer can eliminate tactical advantages a reneging party may have had as the element of a surprise is spoiled by PKOs who publicly report violations of ceasefires. Given that PKOs often serve as buffers, reneging parties run the risk of armed confrontation with peacekeeping forces, which itself contains international costs on reputation.

In the post-conflict environment, PKOs are thought to contribute to post-conflict peace by resolving "commitment problems". In short, the commitment problem is a dilemma scholars studying conflict have identified that makes conflict recurrence a rational path for formerly-warring actors (Fearon 1995, Hartzell et al. 2001, Powell 2006). When warring parties are ready to "come to the table" and negotiate, these parties are aware that, regardless of the settlement they reach, the other party has an incentive to renege on the settlement when it is beneficial for them to do so. Barring a third-party enforcement mechanism, formerly-warring parties may return to war either because the incentive to renege increases or, for preventative purposes, under the expectation that the other party is about to renege on the settlement. In this role, PKOs can serve as a clear third-party guarantor of a settlement to help resolve the commitment problem. Lastly, recent studies have demonstrated that PKOs can serve to resolve underlying structural forces that may promote the onset and recurrence of violence. Examples of this include studies demonstrating a positive effect of PKOs on local economic well-being (Bove et al. 2021), inter-ethnic relations (Mironova and Whitt 2015), and the promotion of representative political institutions (Joshi 2013).

However, many scholars have documented disturbing occurrences that are associated with the onset of PKOs. Indeed, a large (and growing) literature details the links between U.N. PKOs and transactional sex, sex tourism, and human trafficking (Jennings 2010, Smith and Smith 2010, Smith and de la Cuesta 2011, Beber et al. 2017, Bell et al. 2018). While sexual abuse and predation is not necessarily an indicator that a post-conflict environment will return to conflict, it is nonetheless an indicator that PKOs may contribute to post-conflict grievances. Other scholars have noted the presence of "peacekeeping economies" in which local economies experience growth *because of a PKO*, but this growth is sensitive the PKO withdrawals (Jennings and Boas 2015, Jennings 2018, Beber et al. 2019). Theoretical works such as Kuperman (2008) and Rauchhaus (2009) have considered the possibility of unintentional conflict-enhancing side effects produced by humanitarian intervention. According to Kuperman (2008), as humanitarian interventions increase globally, the incentives to rebel in at-risk countries increases. Prospective rebels understand that rebellion is often accompanied by retaliation by the state that often targets civilians. In this way, humanitarian interventions designed to protect civilians can be manipulated by prospective rebels as a tool to combat the state. This creates a moral hazard effect where humanitarian intervention allegedly encourages riskier behavior by dissidents in at-risk countries. Rauchhaus (2009) acknowledged that third party actors can identify when their services are being taken advantage of, however, they can still be limited in their capacity to reduce this unintentional conflict-increasing activity by a lack of ability or will to police and punish provocative behavior. In sum, while the empirical evidence suggests a large pacifying effect of PKOs on peace, scholars have identified many worrying aspects of PKOs for peace.

Of course, not all PKOs are the same and it may be the case that some of these negative aspects can be partially explained through other factors. For example, many scholars have argued that characteristics of PKOs themselves determine whether a PKO will be effective at promoting peace. Haas and Ansorg demonstrated that increased troop quality within PKOs is associated with a reduction in civilian victimization. Bove and Ruggeri (2016) detailed a relationship between increased U.N. PKO troop diversity and a reduction in civilian deaths. In a later study (Bove and Ruggeri 2018), the authors also found that a reduction in civilian and battle-related deaths is associated with decreased geographic and cultural distance between the PKO-targeted state and the composition of peacekeepers themselves. Hultman et al. (2014) showed that an increase in armed U.N. peacekeeping personnel correlated with a reduction in battlefield deaths and a later study found a similar pacifying effect for the risk of conflict recurrence (Hutlman et al. 2016). Kathman and Wood (2016) find similar support for the pacifying effect of the militarization of U.N. personnel in PKOs during the post-conflict period. In contrast, Phayal (2019) found that the military capacity of U.N. peacekeeping forces does not impact levels of civilian victimization. Di Salvatore (2019) demonstrated a crime-reducing effect associated with an increase in U.N. police personnel while an increase in U.N. military personnel is associated with an opposite effect. Studying violence more broadly, Bara (2020) arrived at a similar conclusion where increases in U.N. police personnel are correlated with a decrease in violence in general. Increases in U.N. military personnel were found, in contrast, to be associated with a decrease in civilian victimization when perpetrated by formerly warring parties only.

While the study of U.N. PKO composition is perhaps the most popular in the literature seeking to understand the conditional effects of PKOs on peace, other studies have examined how temporal dynamics impact the pacifying effect of PKOs. Kathman and Wood (2011) demonstrated how impartial interventions (such as U.N. PKOs) are associated with an increase in violence in the short term but decreases in the long term. Gilligan and Sergenti (2007) found that PKOs appear to only be effective in the post-conflict period. Sambanis (2008) detailed how PKOs create peace in the short-term, but long-term peace requires PKOs to focus on building institutions that can sustain the peace following eventual withdrawals of the PKO itself. Other studies have considered war-time dynamics as factors conditioning the success of PKOs. Beardsley et al. (2019) found an interactive pacifying effect of peacekeeping and mediation on battle-related deaths. Fjelde et al. (2018) outlined how PKOs appear to be more effective at reducing civilian victimization when violence against civilians is committed by rebels. In contrast, PKOs seem to be less effective at reducing government-led civilian victimization. Phayal and Prins (2019) find a similar effect when analyzing PKO effectiveness at the sub-national level.

Undoubtedly, the literature analyzing the effects of peacekeeping on peace has led to many valuable contributions. However, a massive shortcoming in the contemporary literature is the inability of most pieces to speak in terms of "effects". In reality, the majority of the peacekeeping literature is limited to discussing findings within the context of correlations and associations. However, as a literature whose intent is to offer and evaluate policy-applicable conflict resolution strategies, the focus of these efforts *should* be placed on assessing causal effects. The following section of this paper argues that making causal inferences in the peacekeeping literature should be a priority of scholars engaged in this literature. In doing so, the following section also reviews and critiques current efforts within the literature at making causal inferences, while also offering potential paths forward to improve the achievement of causal inference in this literature.

**Causal Research in the Peacekeeping Literature**

While causal language in very much present in the PKO literature, causal methods are much less represented. Oftentimes, studies that do not employ a research design oriented towards making causal inferences will use causal terminology such "significantly reduce the risk of further conflict" (Collier et al. 2008, p. 473), "decreased the risk of another war" (Fortna 2004, p. 283), "sustain the peace" (Quinn et al. 2007, p. 183), and "reduces violence against noncombatants" (Hultman et al. 2013, p. 10) when referring to the alleged effects of PKOs on peace and violence. At first glance, this may seem to be simply a semantic quibble. However, we should be cautious of using causal language when using correlative methods. Kocher's (2014) critique of Hultman et al. (2013) demonstrates the need for such caution. While Hultman et al.'s (2013) work suggested a causal effect linking PKOs to the reduction of civilian casualties, Kocher's (2014) re-analysis found that such a causal interpretation was inaccurate given that one-sided violence had decreased on average prior to the onset of PKOs. Further, Kocher (2014) likewise demonstrated that much of the size of the effect between PKOs and violence against civilians was explained by the sole case of Rwanda in 1994. Instances such as this relying on statistical modeling, should warrant caution of causal interpretations of regression coefficients, which require strong assumptions, such as strict exogeneity and a lack of omitted confounding variables (Samii 2016, Keele et al. 2019), that are rarely met in the peacekeeping literature.

That is not to say, however, that causal research is *impossible* in the peacekeeping literature. In select instances, researchers have managed to execute randomized controlled trials (RCTs) where access to the treatment is randomized so that no confounding can occur and causal estimates can be made (Mironova and Whitt 2015). However, these approaches are rare. Practically speaking, researchers do not typically have a say concerning the targets of peacebuilding programs, especially when that peacebuilding program is a PKO. Further, even if researchers had such capacity, it would be unethical to randomly assign potentially life-saving peacebuilding programs to some countries while others did not receive such treatment.

In the absence of experimental data, many scholars in the field have adopted an instrumental variables approach to making causal inferences concerning the effect of PKOs (Sambanis 2008, Vivalt 2015, Ruggeri et al. 2017, Blair 2019, Bove et al. 2021). Recognizing that treatments can be explained by both aspects that are determined by variables in a model (exogenous) and aspects that are, to some extent, determined by other variables in a model (endogenous) factor, the instrumental variables approach seeks to remove aspects of the treatment that are endogenous and retain the exogenous aspects of the treatment to isolate the causal effect of the treatment. The instrumental variables approach does so by identifying a variable (an instrument) that is correlated with the treatment, is not correlated with other confounding factors, and is correlated with the outcome *only through the* treatment. If these conditions are met, it can be assumed that the instrument reflects a portion of the exogenous aspects of the treatment and is untainted by confounding factors. Predicted values are generated by regressing the treatment on the instrument and these subsequent predicted values are used to estimate the causal effect of the treatment on the outcome. While this method is appealing when there are theoretical reasons to believe that confounding variables are present that current data either does not or cannot account for, this approach has not been implemented without controversy. Gilligan and Sergenti (2007) criticized the use of instrumental variables, referring to causal estimates from such an approach as invalid. These authors argued that the literature has a good grasp on the confounders that complicate the relationship between PKOs and peace, rendering the concern of unknown confounders relatively unimportant. Further, the authors were also skeptical that an instrument for this type of research *could* exist on the grounds that "Any factor that affects 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" (Gilligan and Sergenti 2007, p. 91). Essentially, the authors argued that there are no exogenous aspects of the treatment (UN PKO) given that the authorization of PKOs are heavily influenced by endogenous factors related to conflict and peace duration. Indeed, the discovery of valid instruments are particularly difficult given the challenge of satisfying the excludability assumption in which the instrument effects the outcome solely through the treatment. For example, weather is commonly used as an instrument in conflict studies employing an instrument variables approach. However, recent work has suggested that this once-reliable instrument heavily violates the excludability assumption (Mellon 2021). Such findings present a fundamental problem with the use of instrumental variables. Instruments are as valid as our ability to argue that the instrument effects the outcome solely through the treatment, rendering the validity of these instruments incredibly sensitive and subjective.

Instead of instrumental variables, Gilligan and Sergenti (2007) suggested the adoption of matching as an approach to improve causal estimates in the peacekeeping literature. The virtues of matching, as the authors claimed, can be attributed to the relative simplicity and transparency of the technique. Units are matched to each other according to their similarity with a specific number of confounding factors. They differ, however, with respect to their treatment status. Given the similarity between matched units, the difference in outcome between matched units *may* be indicative of a causal effect of the treatment. Matches can be made transparent along with the variables on which they are matched. Indeed, given the intuitive nature of this approach, matching is widely employed in the peacekeeping literature (Sambanis 2008, Kathman and Wood 2011, Hultman et al. 2013, Hultman et al. 2014, Ruggeri et al. 2017, Di Salvatore 2018, Fjelde et al. 2018, Haas and Ansorg 2018, Beber et al. 2019, Bara 2020), albeit, not always as a method to explicitly improve causal interpretation (oftentimes, matching is employed as a “robustness check” to assess the strength of results estimated with adjusted covariates). A significant limitation of this approach is its inability to address unspecified confounding variables. Again, units are matched to each other according to researcher-specified confounding factors. This method cannot resolve confounding effects that are not specified by the researcher meaning that the risk of omitting a potential confounder and biasing a causal estimate is still present.

Much like the use of matching, fixed effects are often employed in the peacekeeping literature (Joshi 2013, Hultman et al. 2014, Kocher 2014, Bove and Ruggeri 2016, 2018, Di Salvatore 2018, Fjedle et al. 2018, Haas and Ansorg 2018, Beber et al. 2019, Blair 2019, Di Salvatore 2019, Phayal 2019, Phayal and Prins 2019, Bara 2020, Bove et al. 2021), oftentimes not explicitly for causal inference purposes. The implementation of fixed effects can be helpful for making causal inferences due to its capacity to control for all observed and unobserved *time-invariant* factors of a specific unit. By creating a dummy variable for each unit, researchers can remove confounding effects that are unit-specific. Confounding effects such as these are often hard, if not impossible, to identify, which lends credit to the implementation of fixed effects. However, two glaring issues with the implementation of fixed effects for making causal inferences in the PKO literature should be noted. First, the implementation of fixed effects for the study of PKOs *as an event* for the study of post-conflict peace is impossible given that the presence of a PKO in the prior conflict is a *time-invariant variable*. In other words, it is *fixed*, meaning that a scholar studying PKOs would be unable to determine the effect of PKOs independent of the other unit-specific fixed factors. This problem, in particular, can be avoided if one alters their research question and/or their measure of PKOs. For example, if one is studying the potentially pacifying effects of PKOs *during* conflict, then PKOs, as an effect, are not fixed because the data set covers the temporal range both pre- and post-PKO. Alternatively, if one chooses not to measure PKOs using a dummy, opting to include a fluid measure such as the number of personnel involved in the PKO instead, fixed effects can still be employed given that the PKO measure is no longer a time-invariant variable. Still, while fixed effects accounts for all time-invariant aspects of a unit, it does not eliminate the potential for a *time-variant* confounder to slip through the cracks and bias estimates.

Transitioning away from approaches seeking to make causal inferences from general treatments across time and space (such as PKOs), other causal inference methods designed to isolate the causal effect of a *specific* treatment have been employed rarely in the PKO literature. To my knowledge, only one study in this literature attempts this approach (Phayal 2019). The author employs a difference-in-differences (DID) estimator to isolate the causal effect of UNAMID (United Nations – African Union Hybrid Operation in Darfur) peacekeeping deployments on civilian casualties in Darfur, South Sudan. This approach allows for causal inferences to be made in non-experimental settings by accounting for within-group differences (pre- and post-treatment) and between-group differences (treatment and control) over time. Using this method, the causal effect is estimated as the numerical *difference* between the *difference* between treated and controlled units before and after treatment. One can obtain this numerical value by estimating a regression model, specifying an interaction between a dummy denoting whether treatment has taken place and a dummy denoting whether a unit is exposed to treatment. The coefficient on this interaction represents the causal effect of the treatment. Using this method, it is conceptually helpful to view the causal effect as the difference between the observed outcome for the treated unit and the counter-factual outcome where the treated unit continued its trend prior to treatment. The intuitive nature of this approach lends itself well to the goal of making causal inferences from peacekeeping operations. Within the context of the study of PKOs, assuming that key assumptions of the method are met, this approach requires at least two similar units (states or sub-national units, for example) that differ in their exposure to a PKO, with ample data covering pre- and post-treatment periods.

While simple in its setup, DID does require certain key assumptions to be met. The most important assumption of DID to satisfy is the parallel trends assumption. In short, this assumption states that the trend in the outcome for both the treated and control units are parallel prior to treatment. However, this assumption is often difficult to satisfy, especially in the peacekeeping literature as pre-treatment units that will eventually be treated may easily be experiencing a very different trend in violence from units that never receive the treatment. For example, peacekeepers may be more likely to deploy to regions where violence is increasing at an exponential rate in contrast to units where the trend in violence is linear with a comparatively gentle slope. If the parallel trends assumption cannot be satisfied, then the DID estimator is unreliable for causal inference. Issues with making causal inferences from DID are only further complicated with “staggered rollouts” where the treatment is not applied at one specific time rather, multiple time units. Dependent on the spatial and temporal aggregation within a study analyzing the conflict-reducing effects of PKOs, the staggered rollout of a PKO can quickly become an issue as PKO deployments may arrive at different time intervals between grids, provincial regions, etc.

To avoid issues with sub-national staggered rollouts and the satisfaction of the parallel trends assumption, researchers may be tempted to use the synthetic control method (SCM), which estimates a causal effect by measuring the gap in outcome between the real unit (a country, in this example) and a synthetic unit that is meant to approximate a counter-factual country that differs from the real country *only in its exposure to treatment* (a PKO, in this example). However, this approach is also burdened by a number of limitations that plague comparative and international politics research. A counter-factual synthetic unit is created by taking the weighted average of specified confounding variables from units that never experience treatment. The validity of a synthetic unit is measured by its ability to closely follow the trend in outcome for the real unit *pre-treatment*. However, this approach requires *quality data* as data aggregated at a country-level that is features notable bias is unlikely to create a synthetic unit that will be able to closely approximate the outcome trend for the real unit. Much like the dependence of DID on the parallel trends assumption, the synthetic control method is likewise dependent on a reliable synthetic unit for any causal effects to be estimated.

Following this review, it may appear that I have made the case that making causal inferences in the peacekeeping literature is beyond reason. This is not the case. However, researchers seeking to make causal inferences in this field (and adjacent areas of study reliant on observational data) must be very careful and intentional with the methods and variables they select. Not all methods may be appropriate due to poor data quality or the nature of the treatment itself. Further, not all variables associated with the outcome or treatment are confounders that need to be accounted for in any design seeking to make causal inferences. Researchers should be explicit about their causal assumptions of variables to isolate *confounding* effects, specifically. Lastly, regardless of the method employed, researchers in the PKO and conflict management literatures are still overwhelmingly working with *observational* data. While a variety of methods and approaches such as matching, fixed effects, difference-in-differences, and more can be used to improve causal inference, there always exists a possibility that an unspecified confounder complicating the relationship between treatment and outcome is present.

Recognizing these limitations, this paper contributes to causal research in the peacekeeping literature by offering an explicit discussion of the causal relationship between covariates and the treatment/outcome in order to isolate confounding factors, introducing inverse probability weighting (IPW) to the peacekeeping literature as an alternative to more restrictive matching techniques such as coarsened exact matching (CEM) that are often used in this literature, and employing sensitivity analysis to formally recognize the degree to which unspecified confounding may bias the causal estimates of this analysis.

**Research Design**

Matching techniques are often employed for causal analysis given their simplicity in both design and execution. While matching methods themselves can vary greatly, the underlying idea behind all matching techniques is that observations in data sets are matched along a specified set of confounders where they differ in treatment status to create a balanced data set. Essentially, matching “matches” each observation with another observation very much like itself. They key difference is that one observation is exposed to the treatment while the other is not. Any difference between matched pairs then, assuming all confounders have been specified, represents the causal effect of the treatment. A key disadvantage of many matching methods, however, is that a lot of information can be discarded if observations are unable to find a match. Further, some observations may be unable to find a match, not because they are fundamentally different than the rest of the observations, but because another observation was *slightly* more similar to a nearby observation. Indeed, given the comparatively smaller size data sets that many scholars in the conflict management literature work with, these drawbacks are particularly problematic.

In contrast, inverse probability weighting (IPW) is an alternative technique that resolves these issues by generating weights to balance the data set along specified confounders in a manner that does not omit any observations.[[1]](#footnote-1) IPW does so by following a two-step process. First, a numeric value for the propensity of receiving treatment predicted by specified confounders is generated for each observation. Given that treatments are often binary, logistic regression is commonly employed for this purpose. Next, these observations are weighted. Observations are weighted more heavily when their propensity to receive treatment differs largely from their *actual* exposure to treatment. For example, according to the results of a logistic regression model, if a country was very unlikely to receive a UN PKO and *still received one*, this observation would be weighted heavily. Likewise, if the results of the model predicted a certain country had a very high chance of receiving a PKO and *did not*, then this observation is also weighted heavily. Observations that experience the treatment in accordance with the predicted propensity to receive treatment are less heavily weighted. Rather than relying on distance to another observation to be weighted, IPW does not require any data points to be dropped as weights are generated agnostic of other observations. While one may be tempted to match on the generated propensity scores themselves, using these scores themselves as matching criteria creates a number of issues for making causal inferences (King and Nielsen 2019).

To compare the difference in causal estimates from IPW and other matching methods, I also execute a set of analyses using nearest-neighbor matching (also known as “greedy matching”) using the Mahalanobis distance and coarsened exact matching (CEM). I execute nearest-neighbor matching analysis due to its simplicity (which has aided its popularity) as observations are matched to one another based on the cumulative “distance” between each other based on a set of specified confounders. I also employ CEM given its popularity in the PKO literature (Ruggeri et al. 2017, Di Salvatore 2018, Beber et al. 2019, Bara 2020). In an effort to ensure that matched units are as similar as possible, differing only in exposure to treatment, CEM imposes a harsh criterion for matching observations. With CEM, observations must be identical along the specified confounders to be matched. For continuous confounders, values are binned so that matching can occur. Given these strict requirements, it is unsurprising that many observations are dropped. Due to this and the aforementioned concern of comparatively small data sets in conflict management research, I do not believe that CEM represents an optimal matching strategy. Nonetheless, CEM is executed to compare the difference in the estimated causal effect as determined by CEM and IPW. Lastly, I also include the “standard” approach in the conflict management literature of adjusting for a confounder by simply specifying the confounder in the formula for the model. I do not expect to yield a valid causal estimate from this approach. However, the difference between this estimate and estimates generated from matching should be informative.

To analyze the causal effects of UN peacekeeping operations on peace, levels of state-based, non-state-based, and one-sided violence in civil conflict and post-civil conflict countries are employed as outcome measurements. For robustness purposes, levels of violence are operationalized as both the count of deaths at a violent event and the count of violent events within countries. Rather than relying on a single aggregate count of violence, I choose to disaggregate violence to holistically examine the potentially pacifying effects of UN PKOs. Data on state-based, non-state-based, and one-sided violence is acquired from the Uppsala Conflict Data Program (UCDP) Georeferenced Event Dataset (GED) v.22.1 (Sundberg and Melander 2013). This data set was selected due to its comprehensive coverage of various forms of violence. While aggregated at the event-year level, I collapse this data to aggregate counts of deaths and events at the country-year level. State-based conflict involves any violent event in which at least one member of the conflict was a state. Non-state-based conflict includes any violent event in which all belligerent members of the event are organized armed forces that are not of a state. Lastly, one-sided violence events encompass any event in which a state or an organized armed group used armed force against civilians.[[2]](#footnote-2)

Information on the location of UN peacekeeping operations is acquired from the Geocoded Peacekeeping Operations (Geo-PKO) Dataset v. 2.0 (Cil et al. 2020). Much like the UCDP GED, the Geo-PKO Dataset is aggregated at the sub-national level. However, I aggregate this data at the country-year level as well to account for confounding effects where data is aggregated at the country-year level. UN PKOs are operationalized as a dummy where a value of “1” denotes a country-year where a UN PKO is present and a value of “0” denotes a country-year where a UN PKO is not present.

A number of confounding factors complicate the causal relationship between PKOs and peace. To isolate a causal effect, it is necessary to account for these confounding factors. IPW is utilized as a strategy to address the influence of confounders as referenced earlier. In this section, I specify the confounders used to generate propensity scores for IPW, their sources, and how these variables are measured. First, a country’s level of development likely impacts both its proclivity towards violence and the likelihood of receiving a UN PKO. The UN has often sent its peacekeeping forces in situations that may be considered “difficult”, as demonstrated by the UN’s disproportionate involvement in Sub-Saharan African conflicts. Alternatively, a case can be made that the calculus for success motivates a UN PKO prior to deployment. If the UN expects to be involved in the peacebuilding process, it may select cases where development levels are comparatively higher to other conflict-afflicted nations to ensure that a UN PKO is successful. Development is measured using the log-transformed GDP per capita of countries (Fariss et al. 2021). Likewise, the size of a potential target country’s population may also influence the likelihood of conflict and receiving a UN PKO. Many studies have found that countries with large populations are more prone to civil war (Collier and Hoeffler 2004, Raleigh and Hegre 2009). In addition, if the UN is concerned with its post-intervention success, smaller countries may represent a more tempting target as they might be easier to manage and govern. A log-transformed population variable is therefore introduced to account for this confounding effect. Third, given the many dangers associated with the resource curse, natural resource wealth is often correlated with the likelihood of experiencing civil conflict (Collier and Hoeffler 1998, Ross 2004, Lujala et al. 2005). Further, given that UN PKOs are authorized by states with economic and strategic interests in maintaining a stable flow of valuable natural resources, the authorization of UN PKOs may be impacted by the amount of valuable natural resources a target country hosts. To account for this, I include a log-transformed measure of the real value of a country’s petroleum, coal, natural gas, and metals production per capita (Haber and Menaldo 2011). Finally, many authors have argued that the UN has a general aversion to entering armed combat with military forces of the state (Gilligan and Stedman 2003, Fjelde et al. 2018). Likewise, the capacity of a state during civil conflict also bears implications for the prospects of peace during and after conflict as a more capable state may be able to bring conflict to an end faster (albeit, potentially through a period of intense violence an repression) while also governing more effectively in the post-conflict period. In sum, a more capable state should impact both the prospects and levels of peace and the will of the UN to intervene. Government capacity is measured as the log-transformed number of military personnel acquired from the Correlates of War National Military Capabilities data set v.5.0 (Singer et al. 1972, Singer 1987).

The goal of all matching/weighting techniques is to balance the data set along the specified list of confounders. In doing so, the idea is that the values for confounding variables are similar for both treated and non-treated units. Matching/weighting alone does not automatically ensure balance, however. As a result, it is good practice to include balance tables that detail valuable summary statistics, such as standard deviation and mean values of the confounders pre- and post-balancing. Table 1. conveys this information for all methods involved (barring the standard “controlling for covariates” approach).

*Balance Tables*

One critique of this comparatively small list of confounders may be that it represents an insufficient collection of potentially confounding effects. Notably, features of the conflict itself, such as war duration, conflict intensity, whether conflict is territorial, whether conflict is ethnic, etc. are excluded from the list of included confounders. Despite theoretical reasons to believe that many of these wartime characteristics may be confounders complicating the causal relationship between PKOs and peace, I exclude these variables for a number of reasons. Given that this study represents a *holistic* attempt to examine the pacifying effects of PKOs, I include conflict and post-conflict cases to examine both the immediate and residual effects of PKOs. However, in doing so, this limits my capacity to account for war-time characteristics. For example, during conflict, features such as war duration and conflict intensity may impact the likelihood of the onset of a UN PKO, but they *certainly* are correlated with the outcome because, to some extent, they are a measure of the outcome itself. If the outcome is conceptualized as “peace” then variables such as war duration and conflict intensity inherently represent the same concept as the outcome. In contrast, these variables could prove as important confounders to account for in a set of strictly *post-conflict set of cases* because the confounder would be *conflict-level* attributes while the outcome would be a *post-conflict-level* variable. In this manner, *war-time* violence is a separate concept from *post-war violence.* However, a similar problem emerges when analyzing strictly post-conflict cases. If a researcher limits their range of cases to the post-conflict environment (and their treatment of interest is an event that occurred *during* conflict), then no post-conflict attribute can be considered as a confounder. For example, levels of development, resource wealth, government military capacity, etc. in the post-conflict period do not influence the onset of a PKO during conflict because the former has not occurred by the time of a PKO onset. To claim that such factors impact PKO onset is to claim that the UN has the capacity to look into the future. The UN certainly conjectures over the future state of these variables, but the UN’s conjecture is fundamentally different than the actual post-conflict reality. A solution to this is to separately analyze cases where observations are split between conflict-years and post-conflict-years (Gilligan and Sergenti 2008). While this is a legitimate strategy, it still suffers from its inability to account for a common set of confounders across both cases (ex. development may have impacted the likelihood of an onset of a PKO and the prospects of peace, but the post-conflict cases still cannot account for this). However, in my attempt to comprehensively estimate the causal effect of UN PKOs, I opt to include both conflict-level and post-conflict-level cases in the data set, at the expense of omitting select potential confounders. Further, given the conclusions on the poor predictive power of many commonplace civil war covariates as demonstrated by Ward et al. (2010), there is additional reason to believe that the relative few number of variables accounted for in this design may be sufficient to account for a large degree of confounding.

*Sensitivity Analysis*

Fortunately, we do not have to blindly wonder if an estimated causal effect from observational data is legitimate or if it is biased by an unspecified confounder. Sensitivity analysis can be employed to quantitatively assess the extent to which unobserved confounding may bias observed results.

**Results**

*Explain What Causal Effect We Are Estimating*

*See Andrew Heiss’s Blog Post for How to Visualize These Effects*

*Sensitivity Analysis*

**Conclusion**

1. However, it is not uncommon to manually remove select observations if their generated weights are extreme. This occurs when a single non-treated observation, according to the propensity scores, was *very* likely to have been treated and vice versa. To overcome potential issues of massive weighting, I trim any observations from the data set where the propensity score is 95 for a non-treated observation or 5 for a treated observation. [↑](#footnote-ref-1)
2. Due to the count nature of the dependent variable, negative binomials are estimated using the weighted data set. [↑](#footnote-ref-2)