Causal Inference for Peacekeeping Research: A Synthetic Control Approach

**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.

**The State of 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 improve causal interpretation. 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.

**Difference-in-Differences**

Although widely used in the broader social scientific literature (Robinson et al. 2009, Keele and Minozzi 2013, Fredriksson and de Oliveira 2019), the use of the difference-in-differences (DID) estimator is practically non-existent in the peacekeeping literature, barring Phayal (2019). 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 of the method to bet.

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. This assumption does not require that values for the outcome should be similar for treated and control units, as treated and control units may already vary based on unit-specific factors. However, DID does require that the outcome values for treated and control units follow a common pattern pre-treatment. If the outcome trends upwards in a certain time period for the control group, then we should also expect that the trend for the group that will eventually be exposed to the treatment likewise trends upwards. If the outcome trends downwards at a certain point for the control group, then we should also observe the treated group experiencing a downward trend. Satisfying this assumption is important for two reasons. First, if the trends between the treated and control groups are similar prior to treatment, this provides legitimacy for the control group as a valid control. Second, if trends in outcome between treated and control units are constant pre-treatment, then all unit-specific factors that create a difference between treated and non-treated units are controlled for. Importantly, this resolves the issue of researchers being unable to control for all unit-specific confounding factors. Despite alternative values for the outcome between treated and control units, so long as their trends follow a parallel path pre-treatment, then any deviation from the trend post-treatment within the treated group can be explained as a causal effect of the treatment.

Using the DID estimator, this project seeks to contribute to the peacekeeping literature in numerous ways. First, despite a trend in the right direction, much more research is needed in the peacekeeping literature to isolate and determine causal effects. Tools such as DID are easily applicable to this area of study and should be employed to move away from correlative analyses as researchers are fundamentally interested in the *causal* impacts of PKOs. Second, this project thoroughly examines the parallel trends assumption. As stated earlier, Phayal (2019) represents the only prior work in the peacekeeping literature to employ the difference-in-differences estimator. However, this paper provides no formal test to assess the satisfaction of the parallel trends assumption. Given that the DID estimator relies on the satisfaction of this assumption, a failure to report this information naturally calls for skepticism concerning a causal interpretation of Phayal’s (2019) results. Third, while DID accounts for time-invariant confounding factors, it does not naturally account for potential time-variant confounders. As a result, formal metrics, such as sensitivity analysis methods, are needed to determine the extent to which unobserved confounding remains a problem post-model specification. To my knowledge, this paper will be the first to use sensitivity analysis in the peacekeeping literature. In the following section, I outline the strategy of this DID design, outlining the logic of the selection of treated and control units, the operationalization of peace as an outcome, the tests utilized to assess the parallel trends assumption, the selection and management of covariates, the methods used to manage spillover effects, and the type of sensitivity analysis employed.

**Research Design**

*Treatment and Control Selection (explain and justify sub-national analysis)*

*Outcome Measurement (Terrorism because it occurs during and after conflict, Log-Transformation?)*

To assess the satisfaction of the parallel trends assumption, this paper uses two approaches common within the literature employing the DID estimator to evaluate this assumption. First, a graph is constructed that plots the average value of outcome for treated and control units pre- and post-treatment. Doing this provides an intuitive visualization of the state of the parallel trends assumption. Second, applying a more formal approach, placebo regressions are another common metric for parallel trends assumption evaluation (Fredriksson and de Oliveira 2019). Using this approach, only pre-treatment data is included, and multiple artificial (placebo) treatments are assigned. Given that this placebo is entirely artificial, no significant effect should be observed within placebo regressions. If such effects are consistently observed, this suggests that some unobserved effects may be present, causing the trajectory of the treated and control trend to diverge prior to the actual treatment. I employ placebo regressions with placebos executed at times X, Y, Z.

*Control Variables and Matching (Size of troop deployment?, population change (if using deaths))*

While satisfying the parallel trends assumption accounts for unit-specific time-invariant confounding effects, it does not organically control for time-varying covariates that may impact the outcome. Dependent on the outcome and treatment/control groups of interest, it is entirely possible that factors can emerge over time that, independent of the treatment, impact the value of the outcome for either the treated or control group. As such, it is standard practice to include controls that reflect time-varying differences to account for variation in the outcome exogenous to the treatment. Importantly, one does not need to control for unit-specific time-invariant factors as these are already accounted for if the parallel trends assumption is satisfied. Controlling can be accomplished through the standard addition of covariates in a standard regression model and/or with matching techniques.

*Addressing SUTVA*

A crucial assumption required to make causal inferences in general, regardless of the method employed, is the Stable Unit Treatment Value Assumption (SUTVA). For SUTVA to be satisfied, researchers should ensure that no spillover effects are present for units that receive treatment. If this does occur, it is difficult to isolate the causal impact of a treatment given that the outcome of some non-treated units may be tainted by the effects of treatment for other units. Indeed, especially within the context of the conflict and conflict management literatures, spillover effects are a near-constant concern as the effects of conflict and conflict management techniques tend to be “contagious” and effect contiguous non-treated areas. While this may be a positive policy feature for those seeking to maximize the pacifying potential of their conflict management program, it can be a headache for scholars seeking to isolate a causal effect of such a program. IPW can be used (Saul and Hudgens 2017). IPW can be used for DID but problems (Stuart et al. 2014).

*Sensitivity Analysis*

**Case Background**

*Introduce the case (briefly)*

*Justification of Case (Greig’s Comments About Specifying that I am talking about a large-in-scope PKO, Has any other study done a case study on this case?)*

*The Conflict*

*The Peacekeeping Operation*

**Results**

* Assess Parallel trends
* Regression Models With Robustness Checks
* Sensitivity Analysis

**Conclusion**