

Achieving Peace while Herding Cats: Understanding Multiple Interventions in Conflict with Dynamic Causal Inference

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1 Art and Science

As the number of international conflicts continues to decline, internal disputes maintain high levels of violence and resist resolution efforts. In 2012 alone, as many as 60,000 people died in these conflicts while only four peace agreements were signed Themner & Wallenstein (2013). More problematically, conflict interventions aimed at creating peace are limited in resources, time, and critical international support (Walter 1997). With the high stakes of inaction or failure, identifying effective and efficient resolution strategies is of paramount importance. Despite this importance, the field lacks a firm foundation of knowledge about the process of conflict resolution.¹ The conflict resolution literature largely lacks this knowledge creating a problem of generalizability for past lessons. Perhaps because so many of the recorded details of a conflict are considered unique to its particular context those details are never aggregated in such a way that generalizability can be determined. By analyzing the causal effects of intervention strategies, relying entirely on the

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¹Conflict resolution and conflict management are frequently referred to as distinct types of intervention, but since both largely encompass the same strategies, principles, and goals, this paper uses the phrase conflict resolution to include both types

details provided by practitioners and qualitative analysts, can the foundation of knowledge about conflict resolution be established. It must be acknowledged that perfect causal inference is next to impossible for most of social science in general and conflict studies in particular. We can never be completely sure which strategies will work in each context. Further inhibiting inference is the lack of consensus on what the factors are that determine that context. This research attempts to add to this effort by highlighting limitations in existing analysis due both to restrictions in data and readily available methods. More importantly, it strives to make its own small step towards the goal of understanding the complex process of conflict resolution through an analysis of the strategic use and timing of intervention efforts.

The first section highlights two common problems, multiple treatments and time-varying confounders, with existing analysis of various intervention types and strategies. This presentation is general and neither intended to isolate any single work nor imply all work in conflict resolution is flawed in this manner. The second section first describes a method, dynamic causal inference as originally presented by Robins, Greenland & Hu, intended to avoid these problems and second assesses the assumptions required for the method to be valid. From the many potential uses of this method, the third section highlights one as an example. This section details a strategy of conflict management that was recently suggested in the book *Islands of Agreement*. The author explores three cases to verify the new strategy, but stops short of empirically testing the idea that early interventions improve the likelihood of resolving even the most intractable conflicts. A plan of empirical analysis is put forth in the fifth section, including a description of the data collection and R development needed prior to being able to perform even a feasibility test. Additionally within this section there is an assessment of currently available datasets. The final section concludes with the future prospects and potential limitations of the method discussed.

2 The Problem: Flawed Assumptions

Because the process of conflict intervention is highly complex, it is extremely difficult to model. The complexity is in part created by the multilevel implications of most conflicts – both domestic and international – but is also due to the characteristics and strategies of the intervention itself. Despite awareness of this complexity, existing work on the effectiveness of resolution strategies involves severe oversimplifications that may yield misleading results. Two of these simplifying assumptions are discussed here in an attempt to highlight the problem within our current understanding of conflict intervention.

2.1 Conflict Intervention as a Single Treatment

Existing work does not consider resolution attempts beyond the intervention of interest as treatments. Both scholars and practitioners acknowledge the deeply complex process they are trying to decompose as they make simplifying assumptions about the number of disputants, the number and type of interventions, and the goal of interventions, among many other factors. Particularly when considering the diverse types of intervention within a given conflict, the assumption of independence between intervention attempts or, worse, only considering a single intervention is distinctly problematic.

While most scholars admit the existence of this difficulty and its importance, very few attempt to deal with the multitude of treatments in their analysis.² Of those that do move beyond these common assumptions, most conduct in-depth case studies where the true effect of multiple interventions is lost in a sea of unique and crucial factors for that conflict. From these studies, it

²Some types of conflict intervention, such as United Nations peacekeeping operations, tend to only occur once in a given conflict, meaning multiple treatments do not exist. These studies still experience bias from the second problem highlighted below, but do avoid the issue of multiple treatments discussed here.

is frequently difficult to uncover the effect of multiple interventions. Even when discussed, the generalizability of this effect is questionable.³ Other work merely controls for the number of prior resolution attempts, which allows some insight into the process but fails to capture questions of the timing and quantity these interventions happen in addition to how many. It is not evident that an intervention attempt two weeks prior is equivalent to an attempt two or even twenty years prior.

Within a given conflict, there may be hundreds of different interventions, both formal and informal, militarized and diplomatic, international and regional, simultaneous and sequential. Any estimate of how one specific intervention affects the conflict's outcome will be unpredictably biased. The estimate will capture any positive or negative effects other interventions had on the outcome of the conflict and may be moderated by preceding interventions. By using regression methods to calculate how effective a particular intervention is in ending a conflict, we are attributing that intervention with the progress or damage created by all other interventions in the conflict. Given that currently a common approach is to examine the intervention with the highest level of action – typically meaning more intrusive – this is a very real problem.

This problem is not only a result of available methods but also readily available data. Very few datasets evaluate more than a single conflict resolution attempt, particularly with mediation or negotiation. The International Crisis Behavior Project presents a large category of mediation variables with valuable information, such as the goal of mediation, style of mediation, positive or negative effect on the outcome of the crisis, and effect on the pace of the crisis. However, with the exception of a single variable measuring the number of mediators/mediation teams, these

³For a thorough compilation of case studies that specifically highlight conflicts with multiple interventions, see *Herding Cats: Multiparty Mediation in a Complex World* edited by Chester Crocker, Fen Hampson, and Pamela Aall.

variables are all based on a single intervention performed by the "primary mediator," defined as the mediator who was most active (Brecher & Wilkenfeld 1997,2000). It is unclear how the primary mediator/mediation team is determined, and the dataset fails to allow for the primary mediator's involvement in multiple distinct mediation attempts. Numerous other datasets focus solely on a single intervention with various guidelines for selection. One frequent option, such as Uppsala Conflict Data Program's Conflict Termination dataset, is the intervention at the conclusion of conflict, detailing only those interventions which terminated the conflict. This is a process of selecting on successful cases – since the conflict is coded as ending in negotiated agreement if any ongoing negotiations resulted in termination, for example – but also disregards any impact "lesser" or earlier interventions may have had on the conflict's outcome (Kreutz 2010). Alternative approaches select the intervention of the highest level, such as the Mediating Intrastate Crises project (Quinn et al. 2013). Finally, many simply code whether or not certain types of intervention occur, such as the Doyle and Sambanis International Peacebuilding data (Doyle & Sambanis 2000).⁴

2.2 Unchanging Covariates throughout the Conflict

When evaluating the effectiveness of intervention strategies, a common approach is to consider a series of background covariates alongside a measurement of the intervention type using standard regression. Frequently considered covariates include type or cause of conflict, relative strength of the disputants, region of conflict, regime of any states involved, and history of conflict among many others. There is an implicit assumption within this method that the snapshot accurately captures

⁴Notable exceptions to this criticism are Bercovitch's International Conflict Management dataset and Uppsala Conflict Data Program's Managing Intrastate Low-level Conflict dataset; however, even these are limited in their inclusion of types of intervention and types of conflict respectively (Melander et al. 2009, Bercovitch 2000).

those variables of interest for the duration of the conflict. Taking battle deaths as an example, it is evident that an average over the span of the conflict would not capture the variations over time which greatly influence when the intervention occurs, what type of intervention takes place, how the conflict is resolved, and when violence ends (Regan 2002). A conflict with a consistently moderate level of violence would be measured the same as one that was highly variant – very low and very high levels of violence – but these two conflicts have very different features and would likely be treated very differently by intervening third parties. Further, a conflict with extremely high levels of violence initially followed by consistently low levels of violence is distinct from a low intensity conflict that quickly escalates. Conflicts cannot be completely defined by levels of violence, but this factor certainly has a great influence on any intervention that takes place.

Many conflicts occur over years if not decades; during this time, the factors that characterize the conflict and affect any resolution attempt may drastically change. Taking a snapshot of these factors at some point during the conflict not only obscures the relationship but also induces bias in estimates of the intervention’s effectiveness (Gilligan & Sergenti 2008). Statistically, variables which have a relationship with both the treatment and the outcome are known as confounders. Failing to include them in analysis creates omitted variable bias in estimates. In standard regression methods, assuming these confounders are measurable, the easy solution is to control for them in the equation. However, if these confounders actually vary over time, like many of the examples illustrated above, this solution risks inducing post-treatment bias (Ho et al. 2006). For example, the number of battle deaths may affect whether and what type of an intervention occurs, the intervention may affect the number of battle deaths, and battle deaths are also related to when and how the conflict ends. This issue puts researchers between a rock and a hard place: these factors induce post-treatment bias if included in the analysis, but excluding them risks omitted

variable bias. Additionally, it has been shown that the two approaches do not reliably bracket the true measure of effectiveness, so that even trying the analysis both ways does not improve inference (Blackwell 2013, Robins 1997).

Most existing research avoid the omitted variable bias by including some measure of covariates, but few mention the risk of post-treatment bias which could significantly alter results. In large part, this could be due to a lack of data availability. Most covariates of interest are measured once for the duration of the conflict rather than for each time period within the conflict. More detailed information is difficult to find without using events data, such as the Global Database of Events, Language and Tone, which is limited in conflict data and less reliable or unavailable for all but the most recent conflicts.⁵ Admittedly, many of these variables are difficult to obtain. A single measure for many of these covariates for the duration of the conflict is often unreliable much less a yearly or monthly measure.

One creative method of avoiding the problem with time-varying confounders was used by Gilligan & Sergenti to analyze the empirical effectiveness of United Nations peacekeeping operations. The authors took a snapshot of relevant covariates in the month of intervention. This snapshot was then used for matching to avoid issues of non-random assignment. Inference was based upon the matched observations and covariates measured prior to the month of intervention. This approach includes the potential confounders, preventing omitted variable bias, but excludes any measures after intervention, preventing post-treatment bias. Although effective in the specific case presented, this method does not deal with the problem of multiple interventions, since there are rarely multiple peacekeeping operations in the same conflict. This method obscures how the

⁵This generalization has many exceptions, particularly with datasets containing measures for states on an annual level. Very little time-varying information, however, is readily available for non-state actors. The Uppsala Conflict Data Project's dataset on Battle Related Deaths is a notable exception, but provides this information only at the year level, though frequently cited as a monthly measure.

timing and implementation of other types of intervention efforts by the United Nations or other actors may alter the results. Arguably, other types of interventions may have differing effects on how successful the peacekeeping operation is, but this has yet to be established. As presented by Gilligan & Sergenti, the method used is similar to the dynamic causal approach if there is only one treatment, using matching rather than inverse probability of treatment weighting.

3 The Solution: Dynamic Causal Inference

Dynamic causal inference is a recent method designed to remedy both of these problems, used primarily in biological and American government studies (Blackwell 2013, Robins, Greenland & Hu 1999, Robins et al. 2004). Rather than interpreting the treatment as a one time occurrence, which most regression methods implicitly do, dynamic causal inference considers the entire history of treatment. This allows for baseline covariates, those we can reasonably assume do not alter over the time period considered, and time-varying covariates, those whose values change over time and may bias the traditional regression analysis. Finally, by having a treatment and covariate measure for each time period, the method can include time-varying confounders without inducing post-treatment bias. Consider the following three hypothetical conflicts to illustrate the importance of this difference.

Table 1: Comparing Treatment Measures in Hypothetical Conflicts

Conflict	Single Treatment	Total Interventions	Intervention History
A	1	2	1, 0, 0, 0, 1
B	1	2	0, 0, 0, 1, 1
C	1	2	1, 1, 0, 0, 0

Each conflict lasts five months, but experiences distinct intervention strategies. In each case, methods which simply indicate the occurrence of an intervention or the total number of inter-

ventions provide the same measure, meaning the three conflicts are analyzed as the same. By evaluating the intervention history, it is evident the interventions in these three cases are distinct and very likely will have varying success in resolving the conflict. Such an approach is more fitting substantively as well as more refined methodologically. Extensive work asserts the importance of timing resolution efforts to maximize effectiveness and recognizes that early interventions may have different results than those later in a conflict (Zartman 2000, Zartman & Touval 1985, Kleiboer 1994, Blum 2007). Additionally, evaluating the history of intervention efforts allows for new types of questions that account not only for the number of previous intervention attempts, but also for how much time occurred between these attempts.

Rather than having two states – treatment or no treatment – with a corresponding potential outcome, in the dynamic setting each treatment history has a potential outcome. For T time periods and a binary treatment, the number of treatment histories is 2^T . With a monthly time period and a conflict that lasts only two years, the number of treatment histories – each with a distinct potential outcome – is 16,777,216. This number escalates even more quickly when considering various levels or types of treatment, such as military/economic/diplomatic. With so many possibilities, a nonparametric analysis is practically impossible. Instead, the method will depend on semi-parametric models, in this case a marginal structural model. With these models the mean of the distribution is fully specified, but the relationship between the treatment and the outcome is unrestricted (Robins et al. 2000). A model is given which indicates when we would expect the potential outcomes to be the same based on the treatment history and covariates. However, simply using this model is not enough to avoid the bias problem.

To avoid omitted variable bias without inducing post treatment bias, the observations are weighted by the inverse probability of treatment (IPTW). Regression and matching both attempt

to solve the omitted variable bias problem, but do so in most cases by conditioning on the time-varying covariates which may induce post treatment bias. Because IPTW does not condition on the confounders, the reweighted data using this method, assuming no unmeasured confounding, will be distributed identically to the potential outcomes (Robins et al. 2000). Using a distinct model – typically a logit model for binary treatments (Blackwell 2013) – to estimate the probability of treatment, the weights are calculated for each time period within an observation; the product of these weights produces a weighting for the observed conflict. Finally, these weights are the inverse probabilities of treatment, so the numerator is normally one. Robins et al. show that using the marginal probability of treatment in the numerator instead stabilizes the weights by reducing variability and increasing efficiency. A final model – again, typically logit – is estimated for the marginal probability based on treatment history, but does not include the time-varying covariates.

4 The Application: Islands of Agreement

Though each has made a valuable contribution to the field, many of the widely accepted theories and trends of conflict resolution have yet to be empirically tested in such a way that avoids inducing bias. This limitation is largely due to the complexity of the process, lack of complete data, and limited methods of determining causal effects. As the statistical literature shows, these biased results do not even give an accurate approximation or range for the true effect. Rather than select between biased results or overly simplify the process of conflict resolution by considering it a single treatment, this research project implements a relatively new method designed to remedy both of these problems. Reexamining any of these could be a worthwhile endeavor if it sheds light

on the complicated process with which practitioners are regularly faced. The following serves as merely one example of a promising new theory that requires validation.

4.1 Refuge in the Storm

In a recent book, Blum isolates early interventions that have the primary goal of improving relations from later interventions that have a peace agreement in mind. These conflict management efforts are not intended to fully resolve the conflict nor are they designed explicitly as a stepping stone to resolution, though the author argues this as a happy side effect. Rather, the intervention of interest for Blum is one focused on areas of asylum from which the conflict may be excluded (Blum 2007, 19), called islands of agreement. The limited intent of these agreements are described, "The islands do not dry out the sea; large or small, they are always surrounded by the flood (Blum 2007, 19). Historic examples of such agreements include the ban on attacking fishing boats made by King Henry IV of England, Charles V of the Holy Roman Empire, and Francis I of France, and other such agreements to limit the extent of warfare. This agreement represented a formalized common interest that would be respected by persistent rivals. Despite asserting these agreements are distinct from conflict resolution efforts, Blum describes the goal of islands of agreement as an effort to contain or resolve parts of the conflict, create regimes of interstate cooperation, and potentially carve out pieces of the conflict and attempt to sustain an equilibrium of more-limited hostile engagement (Blum 2007, 20).

In further description, Blum offers a laundry list of characteristics⁶ that are necessary to the building and sustainment of islands (Blum 2007, 244). These characteristics may be more general

⁶The characteristics include: divisibility, symmetry of costs and benefits, practicality, formality, clarity and ambiguity, monitoring and enforcement mechanisms, institutionalism, representation and accountability, intergovernmental subgovernmental and nongovernmental cooperation, third party participation, and the application of international law. See (Blum 2007, 245-266) for detailed discussion of the characteristics.

than Blum allows and are also helpful in describing conflict resolution efforts, but include several that require a tightrope walk of perfection. For instance, the guidelines for maintaining symmetry rely not on the objective or measurable benefits each side receives from the agreement, but instead on the disputants perceptions of these positives (Blum 2007, 247). Additionally, symmetry in some cases may mean an even split or a division based on relative strength, which may be determined in a variety of ways.

Proposing a stricter test of the hypothesis, Blum evaluates the idea of islands of agreement to enduring rivalries. Though the term is often used in a variety of ways, Blum defines a set of conflicts that fester for years without resolution (Blum 2007, 15), a definition that could include the concept of intractable conflicts. Indeed, the author explicitly uses the terms interchangeably because it is these quarrels iron grip over diverse aspects of national psyche and culture that merits special attention (Blum 2007, 15). At the time of attempted islands of agreement formation, Blum assumes these conflicts are both multidimensional and intractable for the foreseeable future. Specifically, the authors examines three cases of enduring rivalries and highlights the role of islands of agreement in each: Greece-Turkey, Israel-Lebanon, and India-Pakistan.

A defining feature of enduring rivalries is the intense complexity that develops over long periods of time and precludes successful resolution. Existing work largely focuses on the context and characteristics of resolution efforts that are most likely to create peace (Bar-Tal 2000, 2007, Bercovitch & Diehl 1997, Goertz & Diehl 1995, Kriesberg 1996). Rather than rehash these efforts, Blum refocuses attention on a different approach. Typically considered a "second-best effort," conflict management has the potential to reform the relationship between rivals (Blum 2007, 17). Enduring rivalries pose a greater problem for conflict resolution or management largely because they are persistent enough to "develop to a point where they shape identities, as peoples, nations,

and countries define themselves in opposition to their rivals” (Blum 2007, 21).

4.2 Look Before You Leap

Blum proposes that the goal of island making is to create a ”magnetic pull towards cooperation in spheres around and outside [the islands] would render them catalysts” (Blum 2007, 269). Further, within each agreement text Blum considers, the disputants reaffirm their commitment to peace by signing the agreement. Despite this intended goal, the actual achievements of islands of agreement are extremely uncertain. Even in the cases evaluated, implementing islands of agreement have failed to significantly alter the disputants relationship or speed the conflicts resolution. It is thus unclear what the strategy is intended to achieve in many cases, other than purely limiting an existing conflict or preserving a tenuous status quo. In fact, in concluding the book, Blum persuades practitioners to pursue islands of agreement as a means to temporarily reduce pain and suffering with the hope that the attempt may ”bring us closer to peace” (Blum 2007, 272).

Further, Blum’s proposed strategy may suffer from serious setbacks or issues due to unspecified factors. Blum identifies the potential for disputants to use the islands of agreement to avoid negotiation over the roots of conflict, but fails to consider whether the islands have any effect. The relationship created by the island may fail to spread into other issue areas, as ”the parties themselves resist any recognition of their relationship outside the conflict” (Blum 2007, 19). In fact, by definition, the island must be isolated from other aspects of the conflict. Citing a potential issue preventing the creation of an island, Blum acknowledges that issues may appear divisible from causes of the enduring conflict only to be strategically linked, and thus blocked, by political leaders. It is impossible to tell, even from the in-depth case analysis provided, whether these side agreements modified the disputants interactions or were merely the visible result of an already

changing relationship. Given how the relationship between enduring rivalries evolves, with greater period of rest between conflicts and more limited warfare over time (Bercovitch & Diehl 1997), islands of agreement may naturally form later in conflicts without creating a significant shift in the relationship. In fact, the case studies presented discuss islands of agreement formed decades after the rivalry formed.

The effect of the proposed strategy is not only unclear, but quite reasonably could result in a major setback in the resolution of the entire conflict. The author concedes the strategy "may be a two-pronged tool, harboring danger as well as promise" (Blum 2007, 267). The conflict may be prolonged by removing potential issues of common ground that might otherwise be used strategically to help reach a peace agreement. Islands of agreement also reduce the costs of war by protecting important areas, such as fishing boats in the given historic example or water supply in the Israel-Lebanon case study. Finally, if the terms of the agreement are not implemented to the full expectations of each side, the strategy risks reaffirming distrust and further entrenching the rivalry, precluding any future resolution attempt. Short-term efforts, such as establishing islands of agreement, are often achieved at the detriment of achieving longer-term benefits such as resolution (Quinn et al. 2013).

5 The Plan: Testing the Effectiveness of Intervention

As a first attempt at using dynamic causal inference in conflict studies, this research will examine the intervention history of conflicts on the outcome of peace. Specifically as a test of the islands of agreement argument, the analysis will identify interventions which meet the criteria and characteristics presented by Blum and estimate how effective this approach is in achieving peace. In

particular, the strategy should improve the likelihood of a mediated or negotiated peace agreement since the formation of islands of agreement aim to improve the diplomatic relationship of the disputants and build trust in the third party intervener. Using Blum's theoretical explanation as a foundation for the hypotheses, the following outcomes will be examined:

H1: Conflicts in which islands of agreement are formed are resolved more quickly than those without islands of agreement

H2: Attempted mediations and negotiations in conflicts in which islands of agreement have already been formed are more likely to result in a successful agreement than attempts in conflicts in which an island of agreement had not already been established

H3: Conflicts in which islands of agreement are formed are more likely to end in a mediated or negotiated peace agreement than those where no island was formed.

The setup described here is intended as a very limited feasibility test of the hypotheses presented. Any thorough analysis would require such extensive data collection and method development that merit a book-length project. The feasibility test will be based on available data collected by Regan. It is important to note that Blum never applied the strategy to internal conflicts, instead choosing to focus on enduring rivalries between states as a strict test. Though the author does not exclude the strategy's use in internal conflicts, the above hypotheses are not a test of the argument exactly as presented by Blum. Rather, this analysis determines how general or useful the strategy is in other more common contexts. This decision is practical for two rea-

sons. First, as described in previous sections, the empirical data needed to test the precise context proposed by the author is not readily available. Information on the relevant interventions and relevant time-varying covariates simply does not yet exist; however, Regan’s dataset does provide some intervention information for internal conflicts and also has well specified criteria for which conflicts are included making it easier to match those conflicts across other available datasets. Second and more importantly, the number of ongoing enduring rivalries is small in comparison to the number of internal conflicts in the world today. Certainly there is a need for developing better strategies to resolve those intractable conflicts, but the sheer number of internal conflicts, in addition to the widespread violence and consequences of these conflicts, validates efforts to improve resolution strategies. Though many factors are different between intrastate and interstate disputes, it may be that these internal conflicts share important characteristics for the islands of agreement to succeed.

The unit of analysis is the conflict-month, identified by $i = 1, \dots, N$ and $t = 1, \dots, T$. The time period of the analysis is uniquely important for dynamic inference. Ideally, the data would be extremely fine-grained with observations for each day. Unfortunately, data availability and reliability is again an issue, so for this analysis, the time period used will be one month. This requires a smoothing of real measures, implicitly assuming a conflict that ends on the first of a given month is equivalent to termination on the last day. However, it is difficult to see how this minor assumption would create any significant problem for analysis. For each hypothesis, the treatments are any third-party interventions in the conflict. The treatment in this case excludes any direct negotiations between the disputants that do not involve a third party. To simplify the process for a feasibility test, the treatment is a binary measure of some type of third party intervention in a given month or none ($P_{it} = 0, 1$). In a full analysis, the measure should reflect the

diverse approaches an intervention may take, such as the distinction between a primarily military, economic, or diplomatic effort. A distinct category will be created for interventions which meet Blum’s criteria of establishing islands of agreement.

5.1 Dependent Variables

Each of the hypotheses requires a distinct dependent variable. The first hypothesis seeks to determine the strategy’s effectiveness in assisting with the resolution of the conflict. This dependent variable is a binary measure of the conflict continuing ($Y_{it} = 0$) or ending ($Y_{it} = 1$). It is important to note two things about this choice of measurement. First, this binary variable is neutral in terms of how the conflict ends. In other words, the setup does not distinguish between a military victory or a diplomatic one; it simply estimates how establishing an island of agreement affects when the conflict ends. Second, the precise start and end dates of conflict are often disputed. In order to deal with this uncertainty, this analysis will record the dependent variable for each time period (in this case, a month) which gives some flexibility on the precise date.

The second hypothesis estimates how islands of agreement affect the success of future mediation or negotiation attempts. Here the dependent variable is a categorical measure of success, based loosely on Haas’s index of success (Haas 1986) and recorded from UCDP’s Peace Agreements dataset. This measure distinguishes a full agreement, which resolves the core issues of the conflict, from a partial agreement, which resolves some issues related to the conflict, from a peace process agreement, which generally arranges for future negotiations, from no agreement reached. There are a variety of methods used to measure the success of an agreement. A binary measure is common, but either requires that a full agreement be reached in order to be considered successful or ranks negotiations that achieve a peace process agreement equal with a full agreement that completely

resolves the conflict. Rather than requiring that negotiations fully resolve the conflict in order to be considered successful or losing information about the types of agreements reached, this measurement allows for a variety of goals and purposes in negotiation while retaining simplicity and permitting comparison.

Finally, the third hypothesis also considers how the conflict ends. Based on the logic presented by Blum, the islands of agreement improve trust and diplomatic relations between the disputants, which encourages the parties to reach a mediated or negotiated agreement to resolve the conflict. The dependent variable here is taken from UCDP’s Conflict Termination dataset. A strict version of the variable is recorded as successfully resolved by mediation or negotiation ($Y_{it} = 1$) if the conflict terminated due to a peace agreement. A more lenient version of the dependent variable also includes termination due to any type of ceasefire agreement. Conflicts terminated for any other reason, such as one side has achieved victory or violent activity has subsided below 25 battle deaths per year, are not recorded as being resolved diplomatically ($Y_{it} = 0$). The durability of the agreement is not considered here, only whether the agreement succeeds in breaking the continuous years of armed conflict activity.

5.2 The Model and Covariates

Within the method of dynamic inference, a marginal structural model must be established. The hypotheses above ask different types of questions: the first about the duration of the conflict after treatment, the second and third about the likelihood of other occurrences. These different questions require distinct types of models to answer them. The hypothesis will be tested using a Cox proportional hazard model, also a semi-parametric model, which is commonly used in the conflict resolution literature. The hypothesis questions how establishing islands of agreement

affects when the conflict ends. The estimate produced by the model will provide a multiplicative coefficient to the rate of the conflict ending. This is easily interpreted as either accelerating or decelerating the termination of the conflict. Additionally, the Cox model easily allows the use of time-varying covariates but also can provide time-varying estimates. In other words, the model can estimate different effects the treatment has in each time period. This allows insight into whether the islands of agreement have a short-term benefit in bringing about the end of conflict via violence reduction that is different from its longer-term benefit of relationship building (Martinussen & Scheike 2006). This analysis will also take advantage of the time variance by evaluating the impact of intervention early in the conflict – the first six months – compared to late in the conflict – the final six months. The dependent variable in this model is based on the conflict terminating ($Y_{it} = 1$) or continuing ($Y_{it} = 0$) to be explained by the treatment and covariates discussed.

The second and third hypotheses will be tested using the same model since both predict answers to more simple questions of likelihood. The second hypothesis responds to a question of how islands of agreement affect later diplomatic resolution efforts. Because these efforts are each recorded as a categorical measure of success, with four levels ranging from no agreement to full agreement, the marginal structural model will be a multinomial logistic regression. Estimates produced from the model will provide an odds ratio of having a particular level of success given the formation of an island of agreement. Whereas analysis of the first hypothesis estimates how the treatment affects the timing of conflict termination, the third hypothesis predicts how the treatment affects the method of termination. The dependent variable is recorded as a binary measure with a diplomatic resolution as the category of interest ($Y_{it} = 1$). Both the strict and lenient version of this dependent variable are binary, so the marginal structural model will be a

logistic regression. Estimates of this model will be interpreted as the multinomial version above.

Each model will include the same set of baseline and time-varying covariates. These same covariates will also be used in the inverse probability of treatment weighting procedure. The baseline covariates are expected to be relatively constant overtime, so the measure will be the same across all time periods for each observation. Baseline covariates will largely include conflict characteristics such as the region, number of disputants, total fatalities, an indicator of whether the conflict occurs during the Cold War, type of conflict, and dispute issue. Additionally, analysis will account for the intervention history which are lagged measures of the treatment. For simplification purposes, only the previous six months of interventions will be included in the history. This is an arbitrary cutoff point that will be tested using sensitivity tests, but is necessary given the limited number of observations. Ideally, time varying characteristics would include battle-related deaths, duration of the conflict, size of the opposition, state military expenditures, and alliance support. Unfortunately, despite extensive searches, none of this data is available at the monthly level⁷.

5.3 Model Assumptions

In order to establish independence between observations, this research assumes the availability of an intervention effort is not limited. Intervention in other conflicts is assumed to not affect the likelihood of intervention in a particular conflict. In reality, this assumption is likely violated since there is a finite number of actors in the international community and each of these actors are unlikely to become involved in a large number of conflicts. On the other hand, this finite number of actors is quite large when including states, international organizations, non-governmental

⁷Several recent works acknowledge time-varying confounders as a problem and use methods to account for them, but implement analysis on data that is not time-variant. For example, monthly battle deaths are in fact annual estimates divided by 12, thus unchanging from month to month (with the exception of December to January).

organizations, and private actors in any combination. While a state is less likely to intervene by itself in a new conflict when it is already involved in another, it does seem likely that the state may join a coalition. Although technically a reduction in likelihood of treatment, this decrease seems insignificant enough to assume away.

In addition to the standard assumptions, this method involves two crucial assumptions for this application: positivity and sequential ignorability. Positivity, the assumption that each possible treatment history has a nonzero probability of occurring, fails if some possible intervention histories are never observed. This failure is more likely as the number of observations is relatively small and the treatment history is more complex, both of which occur naturally in conflict studies without careful selection of analysis in areas of common support. For instance, if international intervention never occurred in crises that lasted only one day – an entirely reasonable situation – positivity would be violated. To eliminate or at least minimize the bias induced by violations of positivity, areas of common support are selected. In this case, only conflicts with a duration of at least 12 months are being analyzed. While this may limit the study’s generalizability, this limitation is acceptable for unbiased results.

Sequential ignorability in the context of dynamic inference is a relaxation of the standard assumption to allow for time-varying actions. Simply stated, this assumption means that the treatment at a specific time is independent of the potential outcomes conditional on the history of treatment and covariates to that time, that there are no unmeasured confounders. Problematically, it is not evident that the field of conflict studies has discovered and measured all confounding variables. While extremely difficult to confirm without a random experiment, its counterpart is similarly problematic in standard regression, and there is no reason to believe violations are more significant in the dynamic setting. This assumption is likely the most problematic for this

research, thus performing sensitivity analysis and balance checks are uniquely important here. Balance checks will be performed to compare the background covariates of the sample before and after using inverse probability of treatment weighting. Additionally, sensitivity tests will demonstrate the validity of the assumptions, particularly revealing the impact of any omitted variable bias, whereby the outcome will be artificially adjusted to include specified levels of bias and the coefficients reestimated (Robins, Rotnitzky & Scharfstein 1999).

6 Conclusion: Why the Paper Ends Here

There are several problems with the proposed feasibility test. First and foremost, this feasibility test is not feasible itself due to a lack of data. Previous work has likely not considered the time-varying nature of conflict in part due to this same problem. There are no measures of time-varying covariates available. As noted, several studies claim to include these measures, but in fact use averages or some other type of static measurement. As a result any analysis on this data still induces post-treatment bias, since the measured covariates for each time period are altered by events after the treatment has occurred. The feasibility test then suffers the same dilemma as standard regression methods. Additionally, without these covariates, the proposed analysis could severely overstate the effect of treatment since only the treatment measures would vary across the time period considered. This risk makes the feasibility test essentially useless without extensive data collection. This data collection is in the horizon for this larger project, but will necessarily involve field research that is well beyond the scope of this semester's project. Even with this attempt at data collection, the reliability of these measures would be questionable. A more reasonable approach might be to use available annual measures and qualitative accounts of

the conflict to estimate monthly measurements of time-varying covariates. The next step in this project is to investigate potential estimation procedures that best fit this application as well as perform a first sweep of qualitative accounts for more detailed covariate measures.

Data collection will also expand the types of conflicts included beyond low-level internal. For a true test of most theories, particularly the islands of agreement strategy, interstate conflict must be included. As a first step in this direction, data collection will initially use the Uppsala Conflict Data Program's Armed Conflict Dataset matched with Bercovitch's International Conflict Management dataset. This combination of datasets will provide both data on the treatment of interventions – though limited to mediation and negotiation attempts – as well as detailed measures of conflict characteristics which will be used as covariates. The most detailed data about resolution attempts has been collected from Bercovitch's International Conflict Management dataset. Each intervention is associated with a conflict of various types: a militarized interstate dispute, a civil conflict, or crisis. These conflicts were matched to Uppsala Conflict Data Program's Armed Conflict Dataset, which includes interstate and civil conflicts with an annual battle death threshold of 25. This matching process highlighted the selection of cases included in Bercovitch's dataset: civil conflicts were only included in the dataset if the conflict was internationalized, meaning significant international interest or involvement. Further qualitative investigation will attempt to fill these holes and avoid selection bias in the observations included.

The second major problem encountered is a method limitation. The custom in conflict resolution analysis is to evaluate an intervention by its effect on the survival of the conflict. Most commonly chosen is the Cox proportional hazards model, which was also selected here for the feasibility test. Within both R (survival and Zelig packages) and Stata, these methods require the data to be organized by conflict rather than conflict-month. This forces the data to one mea-

surement of treatment and any covariates included. Rather than being a limitation of the Cox proportional hazards model, the setup of the package prevents its use for dynamic inference. As a result, a survival object cannot correctly be created and the analysis produces meaningless results ($R^2=1$ and all p-values = 0). Further investigation will look into alternative packages and if necessary develop a package or extension to handle an expanded data setup in the form of observation-time period.

Once these crucial problems are overcome, this analysis is a limited test of the concepts presented. Empirically testing the theories proposed by scholars is crucial before integrating these strategies into practitioners' toolboxes. The consequences of failed intervention attempts extend beyond the immediate resumption of violence and may have irreparable repercussions. It is this motivation that drives existing work and this proposal. But empirical tests are not enough if the estimates and therefore the conclusions drawn from that analysis are biased and potentially misleading. In order to mitigate this ever present risk, it is important to use new and innovative methods that recognize and avoid known biases. Dynamic causal inference is certainly not perfect; it carries its own set of assumptions which certainly affect estimates when violated. The importance of this method and this research is taking one small step closer to an accurate estimate of the effectiveness of various resolution strategies in order to maximize the success of each intervention in terminating the ongoing violence.

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