

## **Balance of Power Dynamics and War: A Competing Risks Model of Escalation**

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### **Abstract:**

Studies in international relations frequently use fixed measurements over the course of observations that are too aggregated for appropriately testing our theories. Such is the case with testing how the balance of power affects conflict escalation. Although power balances have been examined as a cause of war perhaps more than any other factor in international relations, studies of the balance of power all use static measurements of power which remain fixed over the course of a conflict (typically, the militarized dispute). Since we do not measure the dynamics of the balance of power, we still do not understand how the balance of power affects dispute escalation. To properly evaluate how the balance and changes in it lead to dispute escalation or de-escalation, we must apply a statistical model which allows time-varying covariates over the course of a dispute. Once measured properly and assessed in a competing risks hazard model, the balance of power does appear to affect dispute escalation, with a preponderance of power in favor of one side leading to longer disputes, but also disputes that are ultimately more likely to end in peace than war. The method applied here to balance of power can be applied more broadly to a class of similar problems where variable values change over time within what is nominally our unit of analysis.

## Introduction

Despite decades of theorizing and empirical examination, international relations scholars still do not have firm conclusions about how the balance of power affects war, and what we think we know is actually suspect. If one were to examine recent literature on conflict onset, or to survey IR scholars, there would appear to be a developing consensus that “balance of power” arguments that a balance or equality of power preserves peace are incorrect, for two reasons. First, rational choice logic suggests that the observable balance of power should have little or no effect on conflict propensities, because states will observe the balance, take it into account, and adjust their actions to it in such a way that we will never observe it to make a difference. Second, empirical research consistently finds dyadic equality of power to be associated with more conflict (not less) when compared to situations of preponderance. But it is actually quite premature for us to reach this conclusion. In fact, upon close examination it turns out that existing evidence on the balance of power and conflict is limited and specific, with solid findings only applying to the onset of militarized disputes. More importantly for this article, although we have not realized it, our studies have actually been inadequate to evaluate the relationship between the balance of power and dispute escalation to war. Because we typically treat the militarized dispute or crisis as our fundamental unit of analysis, and ask whether the dispute as a whole escalates to war or not, we are unable to study any patterns or events occurring over time within the dispute. This poses a fundamental challenge when we study the balance of power, because one of the core conceptions of crises is that they are contests of brinkmanship. Players in crises take actions in response to one another; these actions are intended to influence the other players’ behavior during the course of the crisis or dispute. Such maneuvering – including the effects of the participation of third parties joining the conflict – is *necessarily* missed by current studies. Because we miss the possibility of maneuvering and related changes in the balance of power during disputes, we actually know very little about the effects of *either* the static or dynamic balance of power on dispute escalation. In this paper, I propose a competing risks hazard model that allows simultaneous assessment of both static and dynamic variable effects, and allows appropriate assessment of how the balance of power affects conflict escalation.

The balance of power-dispute escalation link is exemplary of a class of problem that we cannot model appropriately given current common research designs in quantitative international relations. The solution I use here is applicable more broadly in social science research. Any time we use a research design which imposes a temporal or spatial delimitation on cases in defining the unit of analysis, but where there is variation in important variables within that unit, we impose a limit on the proper understanding of the problem under study. In particular, it is a problem to impose a temporal observation period on the unit of analysis when there are meaningful changes over time in variables within that unit. I refer to variables that change over time, and where we care about those changes, as “dynamic” variables. This is in contrast to static conditions that remain fixed. When our observations are cast at the level of the larger unit, we are restricted to having one and only one observation of the variable in question. Properly, this observation must be made at the beginning of the unit in question, because any other measurement would include ex post information that should not be part of building the case. But this poses an enormous problem for linking theory and analysis, because our theories often tell us that the evolution of some variable within a case is critical to the outcome.

Imposing temporal units poses a problem for studying the effects of the balance of power and other dynamic variables on dispute escalation. Our unit of observation when we study militarized interstate dispute (MID) escalation is normally the MID, with a dependent variable marking whether the dispute escalates to war (1) or not (0). But with this research design, we lose all ability to study dynamics within the MID. Any change in the balance of power over the course of a MID due to mobilization or participation of other states cannot be appropriately incorporated into this research design. The effect is that if we are using our current dominant research design appropriately, we must treat bilateral disputes or disputes with an unchanging balance of forces as identical to disputes that become multilateral or have large shifts in the balance of power. This seems inappropriate intuitively (we expect such disputes to be different), but we have not previously had a way to deal with the problem.

A competing risks model of dispute escalation provides a solution to the problem of capturing dynamic effects within MIDs. Event history/duration models allow us take account of time-varying

(dynamic) variables within MIDs, and then allow us to link duration back to the probabilities of alternative outcomes to address our fundamental interest in whether leaders commit military forces to war or reach a peaceful settlement. The technique applied here is applicable to any dynamic variable where change occurs at a level “under” our initial conceptual unit of analysis. Below, I begin by discussing the state of the evidence about the relationship between the balance of power and war. I then discuss in more detail why our current research designs do not let us evaluate over time variation within cases, and suggest that a competing risks hazard model can help. Finally, I present empirical analyses using the competing risks model that better show how the balance of power affects conflict escalation.

### **Linking the “Balance of Power” and Conflict**

#### *Definitions*

The term “balance of power” has been used in many ways in the conflict literature. Here, I use the term to mean simply the measured bilateral or dyadic military balance of capabilities between the two sides in a (potential) conflict.<sup>1</sup> A related term is “balancing,” which refers to state behavior in the context of having an option to join one side of a conflict or another. “Balancing” states are typically thought of as those that join the weaker side in a conflict, or join the side of status quo or defending state in a conflict. The alternative to “balancing” is “bandwagoning,” which refers to states who jump on the bandwagon by joining the stronger, or more aggressive, side in a conflict. The key question I explore here is how the measured balance of capabilities (and changes in it over time) affects conflict escalation, in this case the escalation of militarized interstate disputes (MIDs) to war.<sup>2</sup>

#### *Theoretical Positions*

The theoretical notion that an equal balance in power will maintain the peace between nations is one of the oldest and most central notions in world politics. It is also one of the most hotly contested.<sup>3</sup> The two core conflicting positions on questions of static power and conflict have been cast as the “balance of power” and “power preponderance” arguments. At the most basic level, balance of power theorists maintain that when a relatively equal balance of military power exists between two or more

states, the states in question will be less likely to go to war with one another (Morgenthau 1956; Waltz 1979). In contrast, when one state is more powerful than another, it may go to war to enhance its power position further. The counter “power-preponderance” model of war suggests that when capabilities are roughly equal, the states involved may both perceive a reasonable chance of winning, may stand firm or push confrontations, leading to an increased probability of disputes and escalation to war (Blainey 1988). According to this argument, when one side enjoys a preponderance of power, the outcomes of potential conflicts are clear, and states will settle disputes before they escalate to war.

The above arguments focus on the *static* balance of capabilities or power between states, that is, the balance measured at a particular point in time. Other theories have focused on the *dynamics* of the balance of power. In particular, power transition theory and theories of preventive war (e.g. Organski 1958, Organski and Kugler 1980, Kim 1989, Kim and Morrow 1992) suggest that changes in power balances create incentives for states to take actions that lead to conflict. Long-term power shifts may create dissatisfaction with the international (or dyadic) status quo, while rapid shifts in power balance in the shorter-term may create windows of opportunity for conflict that states respond to. The focus on *changes* in power offered by these perspectives added an important dimension to arguments about power and war. Typically, though, changes in the balance of power have been examined as annual changes or changes over very long time spans (10 to 20 years). Little research if any has focused on how changes in a finer time scale (for instance, within a crisis) affects conflict behavior.<sup>4</sup>

A final set of literature suggests that it is naïve to expect to observe any systematic relationship between the balance of power and conflict. Bueno de Mesquita (1981a) pointed out that the major initial schools of thought concerning parity or preponderance implicitly assumed uniform risk aversion or risk acceptance on the part of states. He further (1988) points out that power is only one element of an expected utility calculation that must combine both the probability of winning a conflict (related to power) and the utility of winning/fighting (the stakes and costs). A focus solely on power is thus too limiting, and studies trying to link power to conflict should find no consistent relationship. Indeed, the recent and rapidly-adopted information perspective deriving from rationalist bargaining models of war

suggests even more forcefully that the distribution of power should not logically function as a cause of war since the balance of capabilities is observable to all ex ante (e.g. Morrow 1989; Fearon 1995; Gartzke 1999; Wagner 2000; Powell 2002; Smith and Stam 2004). States can and should respond to observed capabilities in their bargaining, perhaps even before they decide to begin a MID, and so power should not correlate with observed dispute escalation. Either bargains short of war will take into account the distribution of capabilities, or selection processes may systematically “filter out” crises with particular balances of power.<sup>5</sup> A variety of factors related (but not identical) to the balance of power may affect conflict onset, including the gap between power distribution and benefit distribution, and private beliefs about capabilities, but not the observed distribution itself. Under the rationalist paradigm, then, even if observable power matters, analysts may never know by analyzing dispute escalation. Since only private information about the balance of power will matter, the ultimate prediction of dispute escalation to war may lie “in the error term” (Gartzke 1999).<sup>6</sup> Clearly, this poses a challenge to hypothesis testing, a point which will be returned to below.

#### *The Limits of Empirical Findings: Stages and Dynamics*

Although a few studies differ, the quantitative literature on power and war seems to have recently reached something of a consensus that power parity is associated with more conflict, and power preponderance with less (for general assessment, see Geller 2000, Geller and Singer 1998, Kugler and Lemke 1996; recent specific examples include Bennett and Stam 2004, Gartzke and Li 2003, Oneal, Russett, and Berbaum 2003, Reed 2000). Situations in which states are more evenly matched in power are more likely to become conflictual than are situations where one state has a clear advantage. But while there appears to be some consensus on this issue, we must actually be very careful in reaching this conclusion, because there are at least two specific and common interpretations of the question “how does the balance of power relate to conflict” which have fundamental implications for interpreting findings in the literature.

Studies of international conflict typically examine one of two primary dependent variables, either the outbreak of militarized disputes (or crises in some studies, e.g. James 1988), or the outbreak of war.

These dependent variables represent fundamentally different levels of violence. In disputes or crises, the potential for widespread violence (war) exists, but only some disputes/crises actually escalate to war. Crises and wars are of course linked; wars do not start in a vacuum, and few wars begin as full-scale wars on the first day of a dispute/crisis. In most cases a dispute of some sort develops, and the states involved then take sequential or simultaneous actions that eventually lead some of those disputes to escalate to war, while others end without widespread violence. These clearly represent two relatively distinct phases of the typical conflict process, namely the dispute (or crisis), followed by the war.

Explicitly noting these two stages leads to a specific reevaluation of what we mean when we think of the consensus that “preponderance is associated with peace.” First, it may be that we are thinking “preponderance leads to fewer disputes/crises.” If fewer disputes occur between states in a situation of preponderance, and dispute is a necessary step to war, then we would observe fewer wars in situations of preponderance than of balance even if war occurs at a constant or stochastic rate between disputants. But second, it may be that we are thinking “preponderance leads to a lower probability that disputes will escalate to war.” If this is true, then whatever the sources of disputes, the probability of a given dispute escalating to war is lowered by a situation of preponderance. Of course, it is also possible that preponderance affects both stages. So what is the evidence?

An examination of the literature on the balance of power / conflict relationship reveals that the consensus on “preponderance is associated with peace” is primarily borne out by empirical research only on the first of the above interpretations. Although a large number of studies over roughly the past decade have built a solid body of evidence that power preponderance is associated with less dispute onset, we have little good evidence on the relationship between power preponderance and the escalation of disputes to war. For instance, Bremer’s widely cited (1992) dyadic analysis found that more equal capabilities were associated with more frequent war occurrence, but the analysis did not separate the development of war into MID onset and MID escalation phases. As a result, we do not know at which stage the balance is having its effect. Bennett and Stam (2004) similarly employ a design that does not empirically separate the dispute initiation and escalation stages. And studies that have examined the stage of dispute

escalation to war separately themselves turn out frequently to have limited scope. For instance, Moul (1988) examines only European great powers; Moul (2003), Kim (1989), and Sample (1997, 1998) examine only disputes among major powers; and work in the power transition tradition (e.g. Kim 1992, Lemke and Kugler 1995, Organski and Kugler 1980) employs primarily contender dyads as the relevant population. Many other studies of escalation employ dependent variables that are close to war, but are not (e.g. Bueno de Mesquita and Lalman 1992, Huth 1988, Sweeney 2003). Finally, some measures in prominent studies are inadequate to fully test a continuous relationship between power balances and war. For instance, Reed's (2000) study (which appropriately separates dispute onset and escalation in a linked selection model) measures identifies dyadic "power parity" as a situation in which a smaller state's capabilities are within 80% of a larger, while Siverson and Tennefoss (1984) employ an operationalization in which any 2 major powers are considered to be at parity. Theories of "balance of power" do not specify cutoffs that would allow us to so cleanly separate "parity" from "preponderance," and data exist allowing us to use continuous measures rather than fixed cutoffs. These analyses leave questions as to whether or not we know if the balance of power affects the escalation of disputes to war.

The larger problem with understanding dispute escalation is that studies have not accounted for the *dynamics* of changing behavior and dispute characteristics during disputes. This is particularly important for assessing the balance of power. Our studies typically ignore military mobilization, 3<sup>rd</sup>-party participation in disputes, and the fact that the balance of power can change over the course of a MID; moreover, we have not had a way to handle these dynamics empirically.<sup>7</sup> It seems likely that these factors affect the escalation of disputes to war, but we just don't know. For instance, it is suggestive to note that disputes with joiners tend to escalate to war more than bilateral. An analysis of the MID 3.0 data set shows that 3% of the 1966 bilateral MIDs escalated to war (53 cases), while 15% of the 365 multilateral MIDs (54 cases) escalated to war. However, in current research designs this assessment has been made with one ex post measurement of the number of joiners in each MID, and is inadequate as evidence of a link between dispute size and escalation. We can only conclude that through some mechanism, those disputes that ended up larger tended to be the disputes that escalated to war more often. The relationship

could be causal: as states join, disputes may become harder to resolve because of the need to satisfy multiple participants, the balance of power might shift in a way that drives the dispute toward war, or new joiners may act more belligerently than the original participants. On the other hand, the relationship could simply be incidental: disputes over contentious issues, or long-lasting disputes, might simply provide joining opportunities for many states, and such disputes might be more likely to escalate because they are inherently difficult to resolve peacefully. With research designs that measure the number of states only once in each MID, we also do not know what happens to the probability of dispute escalation over time within MIDs. As a result, we do not know how joining affects dispute escalation.<sup>8</sup>

In fact, there are many questions about dispute dynamics to which we do not have answers. For instance:

- Do joiners tend to join the weaker or stronger side in a dispute?
- Do shifts in the balance of power that result from joining affect the probability of dispute escalation?
- Does the number of dispute joiners really affect the probability of dispute escalation, or is there another mechanism at play?
- Does there tend to be an action-reaction (balancing) process in joining, so that if a third-party joins a dispute, the next state to join tends to join with the newest disputant? Or do joiners tend to bandwagon?
- Do disputes where joining is relatively “balanced,” that is, where third-parties have joined both sides with roughly equal capabilities, escalate differently than disputes where joining is “lopsided” in favor of one side?

Why don't we have answers to these questions? Until now, we have been unable to address these questions primarily because of a core research design issue. Simply, the typical unit of analysis (the MID) and statistical model (logit/probit) that we employ to analyze dispute escalation does not allow us

to assess the effects of changes over time within disputes. The MID itself is treated as a “black box,” and we do not examine any behavior, change, or dynamic within it. When the MID is our unit of analysis, we are unable to properly assess how dynamic variables like the balance of capabilities affect conflict escalation. Next, I detail this problem, and then present a solution.

### **The Problem with Studying Conflict Dynamics**

#### *Typical designs*

With few exceptions, when quantitative IR scholars study the escalation of disputes to war, we focus on the MID as the unit of analysis. The typical population of cases is a set of MIDs, likely all MID onsets in a certain time period (e.g. 1816-2001, or 1946-2001). We measure various characteristics of the MID as independent variables, such as the presence of joint democracy and the balance of power between the sides in the MID. We construct a dichotomous dependent variable distinguishing those MIDs that escalated to war (1) from those that did not (0). Finally, we then run a standard logit or probit model on this data set, and see what independent variables influence which MIDs reach a 1.

A recent variant on this approach is to employ a selection model to account for correlated errors across the onset and escalation stages of a MID. Following Reed (2000) as the typical setup, the first stage of a selection model of conflict escalation might begin with some set of dyad-years that might experience a MID (directed or non-directed) as the unit of analysis, and employs the start of the MID (initiation or onset, respectively) as the dependent variable in the first stage. Independent variables are measured for each dyad-year. Those cases that experienced a MID then have a second stage dependent variable coded as whether or not the MID escalated. The same independent variables coded for the dyad-year stage may be used in this second analysis, or new independent variables that are relevant only for cases that experience a dispute could be added at the second stage (first stage variables could also be omitted). The MID remains the unit of analysis at the second stage, and the dependent variable at that stage is a single coding of escalation.

#### *The problem: independent variables and time*

Given that we are interested in MIDs and whether they escalate, these research designs that use MIDs as the unit of analysis (which we judge to have escalated or not) seem appropriate. Unfortunately, they suffer from a fundamental problem when variables that change over the course of an observation (a MID) are important for its outcome. Our unit of analysis defines the smallest “observation unit” that we can have for each variable and case. In the case of MID escalation, we can code only one value for geography, or democracy, or the balance of capabilities within each MID. Using only one of multiple possible values is a problem when we have dynamic variables changing over time. In an appropriate research design, we must take this value by measuring it at the beginning of the observation/event. It would be using ex post information (and therefore wrong) to measure the variable at any other point in the observation. In particular, in the case of MID escalation, it is inappropriate to measure variables at the point just before the MID escalated to war, or to use information from multiple time points within the MID by constructing an “average” or “maximum” value of the variable over the MID.<sup>9</sup> Constructing these ex post measures is inappropriate because they cannot be constructed without knowing about the outcome and its timing; if we do so, we use information about the outcome of the MID in a variable intended to predict that outcome. Whether we call this inappropriate measure endogenous, or tautological, or circular, we cannot properly use this ex post measure to predict the dependent variable.

The necessity of measuring a variable only once is a problem because information on changes in it over the course of the observation must be discarded. With the MID as unit of analysis, it is as if events that occur after the start of the MID simply do not happen; they simply cannot be taken into account. In the case of balance of power dynamics and conflict escalation, there are many reasons to think that the balance of forces between two sides might change over the course of a MID, and that such changes might affect the course of the dispute. If we think of MIDs as brinkmanship, then we would expect to have MID participants moving and mobilizing military forces in an attempt to influence the other’s behavior. If capabilities are mobilized differently across the two sides, then the resulting shift in the balance of power might reasonably be thought to influence the conflict’s outcome (for instance, a state might back down if the other was able to gain a significant advantage by mobilizing quickly after an initial threat).

Such changes would affect both whether ends at any point in time (and so its duration), and also how it ends (in war, or peace). We also know that 15-20% of MIDs involve more than two states. When third parties join in a dispute, they are presumably attempting to affect the outcome, and their participation shifts the balance of power between the sides in the dispute. If a state sees the formation of a strong counter-alliance against it, it might be persuaded to back down (indeed, this is the core logic of balancing behavior).<sup>10</sup>

As a practical example, consider the sequence of moves in the July Crisis leading to World War I. Following initial Serbian-Austro-Hungarian actions, Russia, France, Germany, and Britain get involved in succession. Other states join later. As these countries issue threats, begin mobilizing, and declare war, the anticipated balance of power if war breaks out seesaws back and forth. One of the arguments concerning the origins of World War I is that Britain did not make its intention to act forcefully against German aggression in Europe clear enough at the outset. If it had (according to the counterfactual), Germany and Austria-Hungary might have backed down in the July Crisis, and WWI might have been avoided. But in any appropriate MID-level study of the dispute that escalated to WWI, the balance of power must be measured between Serbia and Austria-Hungary, with subsequent “moves” discarded. Such an analysis surely misstates the role of the balance of power and forces in that dispute.

#### *Joining and Information*

More generally, dispute joining and military mobilization over time provide important information to dispute participants about what might happen if the dispute escalates further or is settled peacefully. It may be very important to the initial participants whether third parties stay out of a dispute or join, join only on one side of a dispute or join more evenly, join rapidly or reluctantly, or shift the balance of power dramatically in favor of one side or maintain it at a relatively constant level (for instance by counter-balancing). Including changes in the number of participants and the balance of power over the course of a dispute, particularly unexpected changes, may be particularly important given recent insights from rationalist theories of international conflict. Such theories suggest that ex ante observable indicators of capability ought not to matter once a dispute begins, because states should have seen, recognized, and

responded to observable characteristics of a dyad before conflict begins. This poses an enormous problem for falsifying theories of power, as a finding of no relationship is consistent both with the argument that power does matter, and that it does not.<sup>11</sup> However, under a rationalist model we might still expect to see *changes* in the balance of power over a dispute having observable effects on conflict behavior, because such events provide new information that is not observed *ex ante*. While some conflict joining might be anticipated, at least some is likely to be unexpected. Under a rationalist paradigm concerned with the effects of information, then, new information might make an important difference in conflict behavior and correlations between power balances and conflict. But under the current common research design where the MID is the unit of analysis, we cannot test this expectation.

In summary, while some variables of interest to us as researchers are in practice relatively fixed during a MID (e.g. whether a dispute's originators are contiguous, democratic, or economically developed), others are not (e.g. the number of states involved and balance of power). With the MID as the unit of analysis, we cannot account for changes in any of these variables. This poses a real issue if we believe that the dynamic aspects of a dispute are important causes of whether and how it escalates.

### **Solution: a Competing Risks Model of Interstate Conflict Outcomes**

A solution to this problem requires us to move away from the MID as a fixed unit of analysis in our data sets (since we need to measure changing behavior and conditions over the course of the MID), but we still must be able to assess and model whether a MID taken as a whole will ultimately escalate to war. While not commonly applied in the international relations literature (but see Joyce, Ghosn, and Bayer 2005 for an application to war termination), a competing risks hazard model offers an appropriate solution to the problem at hand. Competing risks models fall within the set of event history models (also referred to as survival, duration, or hazard models) that have recently begun to be applied in political science (see Box-Steffensmeier and Jones 1997, 2004). Event history/hazard analysis is appropriately used when we are interested in explaining the time until some event occurs, which is inversely related to

the probability of that event occurring in a given interval. The underlying dependent variable in such models is the hazard, or risk, of an event occurring in a case at some time point, given that the case has persisted to that time. Typical hazards in international relations might be the hazard of having a war given that a dyad (the case) is at peace, or the hazard that a MID ends peacefully given that a MID is ongoing. Intuitively, the “hazard” of an event is very close in conception to the instantaneous probability of an event, although mathematically it is not technically a probability.

Most importantly for our purpose here, hazard models allow the use of *time-varying data* (often referred to as time-varying covariates) within events, and do so without forcing us to break up data into fixed and uniform temporal units. As key variables change over time within an event such as a MID, as is the case with the balance of capabilities, we can record the exact length of each interval with a particular set of conditions. From this data, a hazard model uses the information within each interval in estimating the instantaneous hazard within the interval; information from the multiple intervals in the data set are combined to give the ultimate hazard of different MID outcomes. Table 1 reveals what actual data might look like in this framework for two hypothetical MIDs. MID 1 has three “stages” or intervals within it, represented by three separate lines of data. The MID begins with state A initiating a MID vs. state B. The dispute remains bilateral for the first 9 days with a balance of power of 0.45. On day 10 state C intervenes, and on day 26 state D intervenes. With each intervention, the balance of power changes (as does the number of states). Each interval has a particular duration which will factor into estimation. MID 2 has a similar structure to MID 1, but with only one intervention and hence two time intervals with durations of 4 and 2 days.

The key dynamic independent variables for this project are the balance of power and the number of states involved on each side of the MID. In practice, new intervals are coded within MIDs whenever a new state joins the MID, when a participant leaves the MID, or when the MID enters a new year on January 1. The balance of power is changed by each of these events, as states coming or going add or remove fundamental “building blocks” of capabilities to either side, and a new year brings a (potentially) new value from the COW capabilities data set.<sup>12</sup>

As a subset of hazard models, *competing risks* models allow us to study appropriately the separate likelihood that events end in any of multiple competing ways. This is important here because our primary interest is in *how* MIDs end rather than in their duration. While duration models in general would allow us to employ data cast in a dynamic fashion (allowing change over time within the MID), a competing risks model will allow us to study different end states as well (e.g. war vs. peaceful settlement). Competing risks models are especially appropriate and necessary if different factors might be expected to lead a case to end in one outcome or another. In the framework used here, ongoing MIDs are considered to be at risk of ending either in a peaceful settlement, or in war. Within the data (see Table 1), a variable is coded in each interval indicating the interval's outcome, namely escalation to war, peaceful settlement, or neither (indicating dispute continuation to the next interval). Separate hazards are estimated for the competing events; the effects of independent variables on those hazards are also estimated and reported separately.<sup>13</sup> The linkage to duration is that we employ information on the time that it takes for each MID to escalate to war, or reach a peaceful end.

While the application of such models to MIDs is straightforward, it is also relatively new. Making a parallel to another perhaps more familiar venue, we might most easily think of competing risks for a dependent variable such as "leader tenure." The length of time a leader remains in office is determined by when he or she leaves office, which in turn may be determined in several ways. A leader may step down voluntarily, die in office, lose an election, or fall prey to a coup. The processes that might lead to each of these "termination events" are going on simultaneously – a leader might be planning to hold elections, but die before the election is held, or a coup might occur even though the leader was already making plans to step down. Circumstances (variables) in the case influence how these processes develop, and influence which risks are high or low, and how they are related to one another (for instance, a leader's age might increase the probability of death from illness, and the probability that they retire). While the leader's tenure *will* only end in one way (the actual outcomes are mutually exclusive), it *could have* ended in any of them (processes associated with each outcome continue through the leader's tenure). Militarized dispute escalation and outcome processes parallel this example. If we consider the simple

dichotomous outcomes of peaceful settlement vs. escalation to war after some period of time, these outcomes represent two distinct end states. An agreement or decision to settle a dispute short of war is quite distinct from committing forces to war as an outcome. Moreover, the road to settlement and war are processes that can occur simultaneously, as states frequently mobilize military forces and bluster at the same time they are negotiating (as only one example, consider, the Cuban Missile Crisis). Military acts and "peaceful" negotiating acts occur in parallel, and in fact these competing processes form a core part of the bluffing that occurs during international crises, much as age might affect competing processes by which a leader's tenure might end.

A final feature of hazard models is that they allow us to examine whether processes are duration dependent. Duration dependence exists if the length of time that an event has been ongoing affects its hazard of ending. In the case of MIDs, we might speculate that MIDs are negatively duration dependent, in that longer MIDs become even further stretched out as leaders' positions become entrenched. Duration dependence in the parametric models to be employed here will be captured in the model parameter  $\rho$ .<sup>14</sup>

Ultimately, turning to predicted values from our model, competing risks models can produce an expected duration time until the outbreak of war in a given MID, and a separate expected duration time until a peaceful settlement. We thus evaluate variable effects on both duration and outcome. For MIDs that are very war-prone, the expected time to a war will be short relative to the expected time to peaceful settlement, while the opposite will be the case for MIDs that are likely to see settlement rather than war. We can see whether changes in independent variables affect both expected times, or only one, and can assess the rate at which each changes. For instance, if a shift toward power equality in a MID has the effect of cutting the expected time to war in half (reflecting an increased instantaneous probability of war), but has no effect on the expected time to settlement, then we could conclude that shifts toward equality have the effect of making escalation to war relatively more likely. In contrast, if this shift cuts the expected time to war by 20% (an increased probability of war), but cuts the expected time to settlement by 50% (an increased probability of settlement as well), then we could draw two conclusions. First, in this case shifts toward equality have the effect of shortening MIDs overall, because both expected

durations are decreasing (and both probabilities are increasing relative to the probability of continuation). Second, this shift makes the chance of peaceful settlement more likely relative to the chance of war, because the probability of settlement is increasing at a faster rate than the probability of war.

To construct a data set appropriate for this analysis, I developed a custom procedure within the *EUGene* software program (Bennett and Stam 2000) which split each MID into intervals whenever states joined or left the dispute, and at January 1 if the MID crossed a year boundary. Participant capabilities were summed within side and interval to yield the total capabilities for each side in each interval. In some sensitivity analyses, capabilities were discounted for the distance to the expected point of conflict for the MID's originators, and to the nearer of the originators for dispute joiners.<sup>15</sup> In different analyses, I construct measures of the directed balance of capabilities (initiator capabilities divided by total dyad capabilities), and the nondirected balance (larger state capabilities divided by total capabilities).<sup>16</sup> These variables allow us to estimate the effect of the balance in any interval on the hazard of various outcomes, and compare the balance both across different MIDs and different intervals within the same MID. To judge whether *changes* in the balance within a MID affect dispute outcomes, I computed the "change" variable as the directed balance of capabilities (initiator/total capabilities) in period  $t$  minus the directed balance of power in period  $t-1$ . This is appropriately a directed measure, with positive values for change indicating that the balance shifted in the initiator's favor, and negative values indicating shifts in the target's favor.<sup>17</sup> I also include the absolute value of the change, which indicates whether there was a major shift in the balance (larger absolute values) or only a smaller shift (smaller absolute values), regardless of whether those changes favored one side or the other. Values on the "change" variable were set to 0 in the first interval of each MID (this interval sets the baseline and so there is no change yet), and also in any interval where change was due solely to the MID crossing a year boundary.<sup>18</sup> Finally, the length of, number of participants within, and the outcome of each interval were also coded. With the number of participants in each interval coded, we can correctly assess the effect of number of participants on the probability of dispute escalation, as this measurement changes over time.<sup>19</sup>

The outcome variable in all of the analyses codes MID outcomes as either “escalation to war” if the MID escalates to the level of a COW war (Small and Singer 1982), or as “peaceful settlement” for any non-war outcome. This follows the core literature on dispute escalation that considers the critical question to be which MIDs end in war vs. which MIDs avoid this (normatively undesirable) outcome.<sup>20</sup> Such non-war outcomes are “peaceful settlements” in that the MID ends without war; this outcome does not imply that underlying issues are settled (or even discussed) or that longer-term hostility is reduced. The non-war outcome implies that state leaders decided that the issues and circumstances of the MID were not worth risking the numbers of fatalities or extended engagement that a war would entail.

The data set with the new structure contains significantly more information than the standard format data on MIDs. Using the MID 3.0 data from 1816-2001 (Ghosn et al. 2004), the initial data contained 2331 MID onsets. Once sub-intervals were added, there were 3545 total intervals, over 50% more observations. 741 of the 1214 new observations (61%) were added due to a MID crossing a year boundary, and 473 (39%) were added because of state joining or departures. Both types of intervals add new information to the data set, although we would expect smaller changes to the balance of capabilities when a MID crosses a year boundary than when states join or leave the MID.<sup>21</sup> The new information is added disproportionately to a small set of cases, specifically to the 29% of the MIDs that last long enough to cross a year boundary or have joiners. It is not surprising that new information unevenly distributed, as it is a small set of long, multilateral and (circumstantially) more dangerous MIDs that we have reason to explore. Ultimately, after dropping cases because of missing data on independent variables, the competing risks models were estimated on a set of 2262 MIDs with 3429 intervals, through the year 2000 (available data on alliances ends in 2000).

As discussed above, we know the new data structure will allow us to explore different questions about MID escalation than have been explored before. But regardless of how many new cases were added to the data, the critical question to be answered in our analysis is whether this additional information makes a difference in our findings. The analysis below suggests that it does.

## Analysis

With the new data organization, we can assess both how states balance or bandwagon within militarized disputes, and how these behaviors affect conflict escalation. I first examine whether states in general tend to balance or bandwagon within MIDs. I then examine how power dynamics affect MID escalation by presenting two typical baseline analyses, followed by the competing risks model of escalation duration.

### *Balancing vs. Bandwagoning within MIDs*

Balance of power theorists hold that states should balance against aggression in order (ultimately) to preserve their own survival. While this is often depicted as applying to the formation of alliances at a pre-crisis stage of international behavior, it might apply within disputes as well, since states will be acting to deter adversaries or support friends. There are at least three ways we could think of balancing and bandwagoning applying to joiners into a MID:

- Version 1: Does a joiner state join with the initiator, or target? If a state joins with the initiator, then it exemplifies bandwagoning behavior; if it joins with the target, then it exemplifies balancing (acting to oppose the conflict initiator).
- Version 2: Does a joiner state join the weaker side, or the stronger side? If a state joins with the stronger, then it exemplifies bandwagoning behavior; if it joins with the weaker, then it exemplifies balancing (as this shifts the balance in favor of the weaker).
- Version 3: Does a joiner state join on the same side as the last joiner, or on the other side? If a state joins on the same side as the last joiner, then it exemplifies bandwagoning behavior; if it joins on the opposite side, then it exemplifies balancing (as this works against the previous joiner, with the result that the balance of power would seesaw back and forth).

I assessed these variants on balancing and bandwagoning within MIDs by assessing how many states joined one side or the other as defined above in ongoing MIDs that had not already escalated to war.<sup>22</sup> There were 339 cases of states joining an ongoing MID. Of those, 215 were the first joiners into a then-bilateral MID, and 124 were cases of subsequent joining.

Assessing the listed variants, first, did joiners tend to join with initiators (side A of the MID) or targets?<sup>23</sup> Of 339 cases where a new state joined a MID, there were 166 instances where the joining state joined the side with initiator. There were 173 instances where the joiner joined the side with the initial target. The small difference between 166 and 173 is not statistically significant, suggesting that if we think about balancing as an act in which states join to oppose the initiating state, there is no support for the argument that states balance.

Second, did joiners tend to join the weaker side in a dispute, or the stronger? Of 339 cases of joining, there were 184 instances in which the joiner joined the stronger side in the dispute, where “stronger” is defined as the side with more total capabilities in the dispute in the immediately preceding period (based on the COW CINC score). There were 155 instances where the joiner joined the weaker side. On average, these joinings did not shift the balance of capabilities in a dominant direction between the start and end of the dispute. The average change in the capability balance (with balance measured as initiator capability / total capability) from the start to the finish of each dispute across all cases was only -0.0015; if only disputes where there was some change are examined, the average shift is still only -0.02 on a balance variable with a range from 0 to 1, a negligible shift. A few more states (184) bandwagoned than balanced (155), while the balance of capability shifted negligibly against the initiator. The evidence thus appears to run counter to the argument that states balance against capability.<sup>24</sup>

Finally, did joiners tend to join on the same side as the previous joiner in the dispute? Of 124 cases of states joining MIDs where at least one state had already joined, there were 97 instances where the joining state joined the same side as the previous joiner. There were only 27 instances where the joiner state joined the opposite side from the previous joiner. There is a clear statistically significant difference between 97 and 27; states appear to be bandwagoning rather than balancing when we define balancing as joining against the previous joiner.<sup>25</sup> This evidence thus runs counter to the argument that states balance.

In summary, joining states have tended if anything to join the stronger side of a dispute, not the weaker, and have tended to join disputes on the same side as the previous joiner rather than joining

against it. They appear not to systematically join either the initiator's or target's side in MID. The expectation of balance of power theory is not supported by these three tests.

*Baseline: Balance of Power and Dispute Escalation in Standard Models*

Before estimating the competing risks model, I first analyzed the effects of the balance of power on dispute escalation in two standard designs, namely a typical logit model and then a selection model of dispute escalation. In all models I included a set of typical controls for possible confounding effects on dispute escalation, although these variables are not central to the analysis (and the competing risks analysis is robust to changes in them). These include the major power status of the disputants, contiguity, joint democracy, presence or absence of alliance, the S score between originators (Signorino and Ritter 1999), Crescenzi and Enterline's (2001) conflict interaction level, and the number of years of peace in the dyad since the last dispute in the dyad-year analysis (along with appropriate splines, see Beck, Katz, and Tucker 1998). These variables capture both current relations (alliance, S, conflict interaction level, peace years) and static situational variables we might expect to affect behavior (democracy, contiguity, major power status).<sup>26</sup>

Table 2 shows the results of a conventional logit model of dispute escalation to war. Here, the population of cases is all militarized dispute onsets between 1816 and 2000, and the dependent variable is a "1" if the dispute escalated to war. All variables are measured at the beginning of the MID, and so are measured only for the disputes' originators.<sup>27</sup> The 4 models use 4 different variants on measuring the balance of power, and demonstrate that no variant performs much differently than the others. The first variation measures the balance in a directed fashion (initiator capabilities divided by total dyad capabilities), while the second uses a nondirected measure of balance (larger state capabilities divided by total capabilities). The third and fourth variations adjust the capability measures for distance between the participants. The results in table 2 suggest that regardless of the variant, in this standard setup *the balance of capabilities has no effect on the probability that disputes escalate to war*. The only variables that appear to affect whether disputes escalate to war are major power status, contiguity, and joint democracy.

Table 3 shows the results of a simple selection model of dispute initiation and escalation, since selection arguments might suggest that the balance of capabilities appears not to affect escalatory behavior within MIDs only because the balance is known ahead of time to the participants, and because correlated errors between the selection (onset) and escalation stages may be biasing our second stage estimates. Here, the population of cases at the first stage is non-directed dyad years from 1816-2000, with the dependent variable marking whether the states engaged in a MID against one another.<sup>28</sup> At the second stage, the dependent variable marks whether a MID started in stage 1 escalates to war. Because the analysis is nondirected, I employ the nondirected measure of the balance of capabilities only. To ensure model identification, I omit some variables in the dispute onset model from the escalation model. Separate models of onset and escalation are shown for comparison, although I do not discuss them. The combined model indicates that there are linked processes here, with a  $\rho$  of .76. Several variables appear to affect the probability of dispute onset, including the balance of power, major power status, contiguity, joint democracy, conflict interaction level, and distance. At the dispute escalation phase, several of these factors also matter, but most importantly, the balance of power remains insignificant in affecting dispute escalation in both the logit and selection models. Running a selection model still suggests that the balance of capabilities does not affect dispute escalation.

#### *Conflict Dynamics and Dispute Escalation in the Competing Risks Model*

Table 4 presents the new competing risks analysis which uses multiple observations per MID. The competing risks model estimates two sets of coefficients in each analysis. The first set of coefficients gives the effect of covariates (variables) on the hazard of war, while the second set gives the effects on the hazard of peaceful settlement. I report coefficients in the model rather than hazard ratios, and estimate robust standard errors that take into account clustering within each MID or dyad. Coefficients are shown in the accelerated failure time (AFT) metric, and so positive coefficients in  $\beta$  predict longer durations to the outcome when X increases, while negative coefficients indicate a shorter duration. Tests suggested that nonproportional hazards could be a problem with three variables (joint democracy, conflict interaction level, and major power status). Interaction terms were included between these variables and

time to deal with the issue (see Box-Steffensmeier and Jones 2004). The models I present are robust to a number of minor specification and data changes.<sup>29</sup>

Overall, the most important finding emerging from the competing risks model is that several aspects of the balance of power do, in fact, effect dispute escalation. In particular, a preponderance of power leads both to longer MIDs, and to a greater chance of peaceful settlement. MIDs that experience major shifts in the balance of power, in any direction, are more dangerous than those where the balance is relatively constant across the span of the MID. Finally, the more states join the MID, the more likely the MID is to escalate to war rather than end peacefully. Below, I detail these results.

*The balance of power:* Models 1 and 2 employ two measures of the balance of power, with Model 1 using Side A's capabilities divided by total capabilities (which measures the initiator's advantage), and Model 2 using the larger side's capabilities divided by total capabilities (which measures the larger side's advantage). The first of these has no effect on conflict escalation, much as in earlier models. This suggests that the balance for or against conflict initiators has little effect on conflict escalation.<sup>30</sup> However, model 2 reveals that the non-directed balance of power does have a significant effect, and in fact, has a significant and positive coefficient on the hazards of both war and peaceful settlement. Interpreting the magnitude and direction of the effect requires a few steps. First, the coefficients for both war and peaceful settlement are positive. In the AFT metric, this suggests that as the balance of power is more extreme (preponderance), the expected time to both war and peaceful settlement is increasing. This reflects a lower probability per unit time (per day, for instance) that the dispute ends in either war, or peaceful settlement. How is this possible? It is possible because the probability of the third outcome, dispute continuation, is becoming higher. Here, situations nearer preponderance increase the expected time to any type of settlement, and so we expect longer disputes given preponderance vs. balance. However, we can also tell that even as disputes are expected to be longer given preponderance, we expect them to end in peaceful settlement more often. The positive coefficient on the "war" outcome is much higher than the positive coefficient on "settlement." While the time to both outcomes is increasing, the time to war is getting particularly longer. This "greater lengthening" of the time to war

corresponds to a dropping probability of war per period, and suggests that peace is being advantaged. Given these probabilities, as the dispute progresses, we expect to “hit” a peaceful settlement before we hit a war. Thus, a preponderance of power leads to both longer MIDs, and to a greater chance of peaceful settlement.

This finding is illustrated graphically in Figures 1 and 2. Figure 1 shows the *expected time to war* and *expected time to peaceful settlement* for a dispute as the balance of power varies from 0.5 (equality) to 1.0 (preponderance).<sup>31</sup> Note that each line corresponds to a different axis for display purposes. The expected time to war is always much higher than the expected time to peaceful settlement in a MID, because most MIDs end quickly with very few escalating to war. Because it is more intuitive to discuss probabilities, Figure 2 converts the expected duration to the *per-day probability* of a MID ending in war or peaceful settlement (note that the probabilities appear small because they are per-day; if we converted them to yearly probabilities they would be of a magnitude more familiar to most analysts).<sup>32</sup> It shows that the per-day probability of both peaceful settlement and war is lower in situations of power preponderance than of power equality. However, the probability of war under circumstances of preponderance has dropped much more than the probability of settlement. The gap between those probabilities increases as the balance moves toward preponderance, with probability becoming the more likely of the two outcomes. In numeric terms, an interval characterized by power preponderance has a 29% lower probability of peaceful settlement than an interval characterized by equality. But such intervals of preponderance simultaneously have a 61% lower probability of war than balanced intervals. A final way to phrase the finding is that the relative risk of peaceful settlement compared to the risk of war escalation is approximately double in situations of preponderance vs. equality.

*Shifts in the balance of power within MIDs:* Analysis suggests that the overall magnitude of changes in the balance of power (regardless of direction) affect dispute escalation, although change specifically in favor of or against the MID initiator did not matter. In particular, intervals when the balance had shifted had a significantly lessened expectation of the time to war, with no change in the expected time to peace. Reversing direction to speak in terms of probabilities, when there was a major

change of the balance of power in any direction, the probability of peaceful settlement remained about the same, but the probability of war increased significantly. Intervals experiencing major shifts in the balance of power, in any direction, are more dangerous than those where the balance is relatively constant. This finding parallels findings in the power transition and other power shifts literature, but those findings concern shifts over much longer time periods. Here, it appears that even within MIDs, major fluctuations in the balance of power over the course of time increase the chances of war.

*Number of disputants:* Finally, the new analysis also allows proper assessment of whether having more parties involved affects a dispute's escalation. The findings on this variable are quite unequivocal: having a larger number of participants in a dispute interval leads to a shorter time to war (that is, an increased per-day probability of war), as well as a longer time to expected peace (that is, a decreased per-day probability of peaceful settlement). In fact, the substantive effect of an increased number of states is quite large; for example, dispute intervals with 4 participants rather than 2 have more than double the risk of escalating to war, while the probability of a peaceful settlement drops in half. When more players are active in a dispute, the dispute is much more likely to escalate to war. This is the same substantive result as Petersen et al. (2004), but the finding here is not contaminated by endogeneity over the full course of a dispute.

#### *Additional Findings*

Several other findings are very interesting, and the new method applied here reveals several nuanced variable effects on dispute escalation.  $\rho$  is less than 1 in all models, indicating negative duration dependence in the time to MID termination. This suggests that the longer a MID has been ongoing, the longer it is likely to go in the future before ending in either war or peaceful settlement. Contiguity continues to have a strong effect on both war and peace, with contiguous states having a higher per-day probability of escalation to war and a lower per-day probability of peaceful settlement than non-contiguous states. Alliances and friendlier relations between states (measured by high values on conflict interaction level) both shorten the expected time to peaceful settlement while leaving the expected time to war unaffected relative to states with no alliance and less-friendly relations. But foreign policy/alliance

similarity as measured by S actually appears to affect disputes in exactly the opposite direction, with higher S scores increasing the expected time to peaceful settlement (lowering the per-day probability of peaceful settlement). The net effect of major power status and time is that disputes in which at least one originator is a major power have higher per-day probabilities of war relative to disputes without any major powers, although the probability of war decreases slightly as disputes become longer. Disputes with no major powers also have a higher per day probability of settlement than disputes with major powers; the probability of peaceful settlement increases over time in both types of dyads.

Finally, the effect of joint democracy is worth special discussion as it illustrates the power of the competing risk setup and of non-proportional hazards. Overall, MIDs where the states are jointly democratic are shorter on average than MIDs where they are not (in keeping with Bueno de Mesquita et al. 2004). However, the effect of democracy on the risk of a MID terminating in different ways changes as disputes lengthen. Estimating the effect requires taking into account the coefficients on both the individual democracy variable and the interaction with time. These combined effects are shown in Figure 3, which shows the per-day probabilities of war and peaceful settlement at different dispute durations (up to one year) for democratic and non-democratic pairs.<sup>33</sup> Considering first the per-day probability of war (left section of figure), we see that jointly democratic dyads always have a much lower probability of war than mixed or autocratic dyads. The per-day probability of war drops for both democratic and non-democratic dyads as disputes lengthen. In terms of peaceful settlements, though, a different effect is found (right side of figure). In jointly democratic dyads, the per-day probability of peaceful settlement actually drops as the dispute lengthens, while this probability increases in the non-democratic dyads. Disputes between democracies become increasingly stretched out as disputes lengthen. Most disputes involving democracies end rather quickly, and this decreasing probability over time is not realized. But for those MIDs that do not reach a fast settlement, democracies face an increasing probability of continuation relative to disputes where at least one state is not democratic. We would not have been able to find these separate effects involving change over time in a conventional analysis framework.

## Conclusions

The key methodological argument of this paper has been that international relations scholars have been unable to study the effects of variables that change over the course of militarized disputes within our standard one-case-per-MID research design. Any dynamic variable in such a setting must be reduced to a single static measurement, with the measurement appropriately taken at the beginning of the MID. This leads to a substantive argument: our knowledge about whether and how the balance of power affects dispute escalation has been fundamentally limited because of our research design. A competing risks hazard model with time-varying data does allow us to study how dynamic variables affect dispute escalation, however. Variables such as the number of actors and the balance of power are prime candidates for examination with this method.

While analysis using common research designs found no relationship between the balance of power and dispute escalation, analysis applying the competing risks methodology found that disputes with a preponderance of power for one side within them were more likely to end in peaceful settlement. Such disputes were likely to be longer than disputes with relatively equal power, but the probability of war escalation in these disputes was less than in disputes with a relatively equal power balance. Disputes where the power balance fluctuates are also more likely to escalate than those where the balance remains fixed, and disputes with more participants are similarly more likely to escalate. These findings emerge from a new data set that includes data about changes in power and the number of states involved over time within MIDs. They suggest that the additional information on those changes, and the new methodology that allows us to link information across time intervals to form assessments of overall MID behavior even while modeling sub-MID activity, is beneficial.

The method applied here is broadly applicable to any area of social science where there are multiple categorical outcomes but also variation in important variables within our typical unit of analysis. When theories suggest to us that the evolving dynamics of a situation are important to event outcomes, we can measure variables over time and link them in a duration model, rather than impose an observation period on the unit of analysis that may or may not be appropriate. Because we can reverse the

interpretation of “expected duration” to be “probability per time period,” we lose no power of interpretation or intuition about what it means for a situation to have a high or low probability of a given outcome. We can also explore new hypotheses and patterns, such as how effects and processes change over time and are linked to substantive variables.

Extensions and other directions employing the competing risks framework are of course possible. For instance, we might be able to use the design proposed here to better study when states choose to join conflicts. If we could estimate a selection model followed by a competing risks duration model, we could doubly-verify that findings about dynamics within cases are not biased by the fact that only a particular subset of cases began. Unfortunately, there is no current estimator that combines these separate models. Another expansion relevant to bargaining theories of war in international relations would involve looking at pre-war expectations of the balance of power, and examining deviating dynamic moves within disputes as unexpected shocks which might have major effects on the probability of war. Analysis with such a “shock” variable would perhaps allow more direct testing of the expectations of the rationalist research paradigm that suggests that pre-war relative power should have little effect on conflict escalation processes. This paper takes one step in the direction suggested by that literature by modeling changes to the pre-war dyadic balance, as this change brings us new information content. Further exploration of these and other directions remains.

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<sup>1</sup> Other uses of the term have included the “balance of power” as a description of the status quo, a description of past conflict behavior, a description of a type of international system, or a description of a state’s foreign policy. See Nye (2005).

<sup>2</sup> Braithwaite and Palmer (2005) examine a variety of conceptions of dispute escalation (e.g. reciprocation, escalation to war, varying fatality levels, expansion) and demonstrate that these are not interchangeable. Factors that influence escalatory processes as measured by one variable do not necessarily have the same effects on other versions. Here, I focus on the conventional definition that the literature most often uses, namely the escalation of disputes to war. Because I integrate conflict joining with escalation to war, I am actually linking two of their dependent variables (expansion and escalation to war).

<sup>3</sup> There is a vast literature on the relationship between power and conflict here. For more complete reviews and discussion, see (e.g.) Geller and Singer (1998). Nye (2005) further notes the difference in balance of power arguments between outcome conditions as “peace” (the absence of war) and “stability” (the independence of states, or the existence of a given system of states). Here, I am focusing on the question of how the balance of power affects peace in the sense of the outbreak of individual conflicts.

<sup>4</sup> Huth (1988) is concerned with the short-term availability of military forces and how the short-term or immediate balance of forces may differ from the long-term balance; his measures distinguish between these. He does not focus specifically on explicitly measuring changes in the balance over the course of a crisis, however.

<sup>5</sup> For example, if an unfavorable balance of capabilities matters to a state, it will never challenge the status quo in the first place, and so we will not observe a dispute which might escalate. If challenged, a state in such an unfavorable situation will react by giving a favorable bargain to the challenger.

<sup>6</sup> But see Lai (2004), who measures “private mobilization” and argues (and finds) that crises where one or both states mobilize privately are more likely to escalate to war than those where states mobilized publicly.

<sup>7</sup> Certainly, some scholars argue that third parties are important (some recent studies include Wagner 2004 and Corbetta and Dixon 2005), but the effects on the balance of power remain unstudied, and the implications of these dynamics for empirical modeling have not been addressed.

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<sup>8</sup> And so suggestive analyses such as Petersen, Vasquez, and Wang (2004) are in error in concluding a relationship exists between joining and escalation.

<sup>9</sup> See Freeman (1989) for additional discussion of the problems of systematically sampling from a “time stream” and aggregating data over time intervals.

<sup>10</sup> A similar problem arises with other variables that change over time. The number of actors in the MID (Petersen et al. 2004) has already been mentioned. Another example would be fatalities. On average, longer MIDs have more fatalities than shorter MIDs, but we cannot in current research designs explore whether the MIDs are longer because they have more fatalities, or whether there are more fatalities because the MIDs are long. If it were the case that longer MIDs also had a higher risk of war, we could not conclude whether time, fatalities, or number of actors was the driving force behind that risk. With only one observation per MID, we must reduce all data on each variable to one value per MID, and so would not be able to distinguish among different causal patterns.

<sup>11</sup> In fact, a finding that there *is* an observable relationship between power and conflict escalation within MIDs might be seen as posing more of a challenge to rationalist theory than a null finding!

<sup>12</sup> Of course, we would rather have independent variable data measured on a finer time interval than annual. While changes recorded in states’ national capabilities on January 1 of each year sometimes reflect conscious and major mobilization or demobilization actions, capabilities do not typically follow a step function with all departures and new mobilization happening on January 1. Troop strength and military expenditures actually follow a more gradual path, and major changes can sometimes occur in the middle of a year in cases of rapid mobilization (or demobilization). However, systematic capability data is available only by year, and it is typically impossible to get figures at a sub-yearly level. There are a few notable exceptions in highly studied conflicts such as WWI or WWII, but these cases are both rare and non-random. Given the frequency with which sub-annual data would ever be available, it is unlikely that augmenting the data with more dynamic capability information in this way would make a significant difference in findings, although this remains a question for possible empirical investigation.

<sup>13</sup> The model assumes that that the hazards of the events (outcomes) are independent, conditional on the covariates included in the model.

<sup>14</sup> When  $\rho=1$ , the hazard rate is independent of time, and the process has no duration dependence. When  $\rho>1$ , the distribution shows positive duration dependence, with a hazard rate that increases monotonically over time. In such

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a case termination accelerates over the course of a MID, and the termination event is more likely to end in each subsequent time interval. When  $0 < p < 1$ , the distribution shows negative duration dependence, with a hazard rate that decreases over time. This would occur here if MIDs tended to become institutionalized over time, or if positions hardened over time as actors backed themselves further and further into hard-to-get-out-of corners.

<sup>15</sup> Specifically, if the originators had the same home region as defined by the COW project, then capabilities were discounted to a point equidistant between the two states (which could result in no discounting if they were contiguous). If they were in different home regions, I calculated which region was relevant for conflict as defined in Bueno de Mesquita (1981b), treated the location of the state in the relevant region as the likely point of conflict, and discounted each state to that point.

<sup>16</sup> Scholars have operationalized the balance of capabilities using the Correlates of War composite index of national capabilities (CINC, see <http://correlatesofwar.org>) scores in various ways. Measures used in various studies include 1) Larger side's CINC / Smaller side's CINC (sometimes logged); 2) Smaller side's CINC / Larger side's CINC (sometimes logged); 3) Initiator side's CINC / Total CINC, and 4) Larger side's CINC / Total CINC. I employ the initiator/total and larger/total variants because they are more direct than the smaller/larger ratios. In particular, they have intuitive meaning (a naïve probability of victory for the initiating, or larger, side), are naturally bounded at 0 and 1, and do not require an additional transformation (logging) to avoid extreme skew.

<sup>17</sup> Note that here, side A need not be thought of as “initiator” but simply as a constant identifier for one of the sides. But to ensure that shifts are always counted, we must always use a directed version (initiator/total) to measure change. If we used the nondirected (larger/total) version of the balance, we would misstate any shift where the identity of the larger and smaller side changed because one side passed the other in capabilities. For instance, subsequent intervals with a 2:1 advantage for A and then a 2:1 advantage for B would show the same larger/total balance, and hence no change across periods.

<sup>18</sup> The effect of a MID crossing January 1 is felt in the measured balance of capabilities variable, just not in the “change” variable, because we want the change variable to inform us about the effect of states joining or leaving the dispute. If states joined or left the dispute on January 1, then the value of change was not set to 0.

<sup>19</sup> It turns out to be the case that many more states join than exit disputes, and the tendency is for disputes to become larger over time. Per-interval measurement captures both instances, though, and as with the balance of power, estimates the effects of number of actors in an instantaneous (per-interval) manner.

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<sup>20</sup> Again, while the bulk of the dispute escalation literature considers escalation to be “war,” Braithwaite and Palmer (2005) point out that several different measures of conflict escalation are available and that alternative measures of escalation often yield substantively different results in empirical models.

<sup>21</sup> Changes in national capabilities from year to year are typically a small percentage of capabilities, although a few dramatic exceptions occur before and during the early stages of wars. This does not introduce autocorrelation in a problematic way, as event history analyses typically use data that is largely fixed over time, with one or two covariates changing at limited intervals (for instance, marital status, or a medical treatment). Specifically, methods for time-varying covariates treat the outcome of intervals before the last interval as having censored outcomes, and examine variation within and across MIDs to draw inference.

<sup>22</sup> Jones (1994) conducts a superficially similar analysis. However, he includes not just interventions into interstate disputes, but interventions into “disputes” with nonstate actors. He also includes interventions into ongoing wars, which I exclude because I want to focus on pre-war dynamics rather than the possibly different circumstance of ongoing combat. Finally, although it is somewhat unclear, it appears that he also uses only disputes where force has been used on both sides. The set just of pre-war MIDs appears to be a more uniform population in which to focus this investigation.

<sup>23</sup> The term “initiator” is somewhat controversial when applied to the MID dataset. In that set, side A is the side in the MID which first crossed the MID threshold, that is, threatened, used, or displayed force. However, there could have been verbal or other provocative actions before the first codeable MID incident; some argue that these would better determine who is really “at fault” in the MID. Without data on such pre-MID actions, the first state to clearly take a MID-type step is the best we can do at present when it comes to determining a conflict initiator.

<sup>24</sup> This evidence runs more strongly against balancing behavior than the evidence of Cusack and Stoll (1991). They found that the balance of capabilities in the MID tended to shift in favor of the initially smaller side, although they found no tendency for more joiners to join either the initially weaker or stronger side in the MID. The analysis here differs in several respects: I examine 339 MID joinings through 2000 (Cusack and Stoll analyzed 165 instances in 489 disputes from 1816-1976 in which at least one state was a major power); it goes beyond examination of the initial and final balance (which might conceal a variety of up and down patterns over the course of the dispute) by assessing the effect of each individual joining decision; and in keeping with the idea of dynamically evolving

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situations in this paper, it examines each joining decision in light of the immediately preceding balance of capabilities rather than the initial balance, which might be quite distant temporally.

<sup>25</sup> It is possible that this pattern could result from many states sequentially reacting against one state on some side. However, many states reacting *simultaneously* against a state are not counted as sequential bandwagonings.

<sup>26</sup> It is certain that some of these variables are related, empirically or theoretically. For instance, the conflict interaction level measure is based on past disputes (not current); if dispute behavior is related to democracy, then democratic pairs may have lower conflict interaction scores than non-democratic pairs. Similarly, some variables might be correlated with the balance of capabilities. While it is not the intent of this paper to develop a theoretical model of how all of these factors interact, it is useful to reiterate that the findings concerning the balance of power are quite robust to modifying this specification. Moreover, the point of including these factors in one model is to control for the empirical overlap (shared variance) between them.

<sup>27</sup> Because variables must be measured at the beginning of the MID, a few more cases are lost than in later models; 5 cases are not included because there is no data on the international conflict score variable in the first year that the dyad exists in the international system.

<sup>28</sup> Creating a selection model from dyad-year data requires a number of research design choices that have not always been clearly identified in the literature (e.g. Reed 2000). In particular, at the first stage we must identify which dyad-years are marked as having a dispute onset in order to then model whether the dyad escalates. The key choices include first, whether we mark dispute joiners or only originators as having a dispute onset, and second, whether we mark all dyads of all originators, or only pick out one dyad. We can only mark “escalation” for those dyads marked as experiencing a MID onset at the first stage. If we mark all dyads of originators or joiners as “onset” dyads, then we would code escalation in many dyads, even though the “escalation” is actually the escalation of only one MID. But if we mark only one dyad as an “onset,” then we can code only one escalation, neglecting the fact that many other states also ended up at war (we cannot include escalating dyads without onsets). In the analysis in Table 3, I coded onsets for any pair (originators or joiners) that got into a dispute. This keeps all possible “escalating dyads” in the sample at stage 2. It does lead to an apparently-larger number of disputes in the escalation to war stage in Table 3 than in the other analyses, but this n in the bivariate probit is actually “disputing dyads” rather than individual MIDs.

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<sup>29</sup> Running a Cox hazard model yields similar results on the variables of interest. Similarly, the results are robust to variations on the exact combination of independent variables included; in particular, dropping the “change in balance” or “absolute change in balance” variables makes little difference, and results using distance-adjusted capability measures are very similar. In terms of case inclusion, the results are robust to dropping 1 day MIDs (740 intervals), and dropping unreciprocated uses of force (900 intervals). The core results are also similar if the terms to deal with nonproportional hazards are excluded.

<sup>30</sup> An alternative explanation per earlier discussion would be that the measure does not always reflect the initiator’s advantage, because Side A may or may not be what we would consider the initiator.

<sup>31</sup> For purposes of estimating duration, other variables are set at their mean or modal values in the data set. Expected duration in a Weibull hazard model is  $E[t|x_i] = \exp(\beta'x_i) \Gamma(1/\rho + 1)$ , where  $\Gamma$  is the gamma distribution, and  $\rho$  is the duration dependence parameter.

<sup>32</sup> Given an expected duration to war (or settlement) of  $n$  days, I compute the fixed daily probability  $p$  that yields a cumulative probability of 0.5 after  $n$  [expected duration] days pass. The code for the routine used to compute this is available with the replication data.

<sup>33</sup> The average duration a dispute has already been ongoing across intervals is 108 days. The data is quite skewed, with a standard deviation on that duration of 340 days.

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**Table 1: Hypothetical Competing Risks Data Setup for Two MIDs**

<b>MID #</b>	<b>Interval</b>	<b>Start</b>	<b>End</b>	<b>Time</b>	<b>Side 1</b>	<b>Side 2</b>	<b># States</b>	<b>BOP</b>	<b>Settlement</b>	<b>War</b>
1	1	1/1/1900	1/9/1900	9	A	B	2	0.45	0	0
1	2	1/10/1900	1/25/1900	16	A, C	B	3	0.75	0	0
1	3	1/26/1900	2/7/1900	13	A, C	B, D	4	0.55	1	0
2	1	3/1/1901	3/4/1901	4	A	B	2	0.8	0	0
2	2	3/5/1901	3/6/1901	2	A	B,C	3	0.4	0	1

**Table 2: Logit Model of Militarized Dispute Escalation to War**

	Model 1	Model 2	Model 3	Model 4
Bal of Capabilities (Side A / Total)	0.292 (0.98)			
Bal of Capabilities (Max/Total)		-0.638 (0.91)		
Bal of Capabilities (Side A / Total) (distance adjusted)			0.351 (1.15)	
Bal of Capabilities (Max/Total) (distance adjusted)				-1.071 (1.55)
At least 1 Major Power (Originators)	0.827 (3.86)**	0.829 (3.89)**	0.895 (3.94)**	0.926 (4.12)**
Contiguity (Any Type, Originators)	0.724 (2.83)**	0.728 (2.85)**	0.698 (2.72)**	0.696 (2.69)**
Joint Democracy (Originators)	-2.157 (2.14)*	-2.155 (2.14)*	-2.169 (2.15)*	-2.168 (2.15)*
Alliance (any type) (Originators)	-0.182 (0.62)	-0.178 (0.61)	-0.207 (0.71)	-0.218 (0.75)
S (originators)	0.328 (1.61)	0.321 (1.56)	0.356 (1.71)	0.360 (1.72)
Conflict Interaction Level (Originators)	-0.004 (0.02)	-0.013 (0.05)	0.073 (0.27)	0.109 (0.4)
Constant	-4.218 (13.40)**	-4.255 (13.21)**	-3.529 (5.90)**	-3.208 (5.42)**
Observations (MIDs)	2257	2257	2257	2257

Absolute value of z-statistics in parentheses

\* significant at 5% level; \*\* significant at 1% level

**Table 3: Bivariate Probit Mode of Dispute Onset and Escalation to War**

	Estimations: <u>Separate</u>		<u>Combined</u>
	MID Onset	Escalation to War	MID Onset
Bal of Capabilities (Max/Total)	-0.398 (4.39)**		-0.357 (5.15)**
At least 1 Major Power	0.639 (20.70)**		0.636 (20.06)**
Contiguity (any type)	0.593 (16.60)**		0.618 (16.34)**
Joint Democracy	-0.342 (8.21)**		-0.345 (5.45)**
Alliance (any type)	0.031 (0.90)		0.048 (1.12)
S	-0.289 (2.88)**		-0.290 (2.01)**
Hostility Score	-1.193 (20.70)**		-1.206 (28.69)**
ln(Distance)	-0.157 (11.87)**		-0.155 (8.67)**
Difference in S score with system leader	-0.100 (1.86)		-0.097 (0.65)
Systemic Power Concentration	0.415 (1.76)*		0.100 (0.17)
Peace Years	-0.073 (12.36)**		-0.069 (8.95)**
Peace Years Spline 1	-0.000 (7.80)**		-0.000 (4.28)**
Peace Years Spline 2	0.000 (6.31)**		0.000 (3.18)**
Peace Years Spline 3	-0.000 (2.07)*		-0.000 (0.86)
Constant (onset)	-0.815 (4.65)**		-0.807 (2.73)**
			<b>Escalation to War</b>
Bal of Capabilities (Max/Total)		0.790 (1.09)	0.454 (0.87)
At least 1 Major Power		0.619 (2.27)*	0.653 (2.97)**
Contiguity (any type)		1.079 (3.61)**	1.068 (5.00)**
Joint Democracy		-1.541 (3.33)**	-1.236 (3.32)**
Alliance (any type)		0.677 (2.09)*	0.484 (1.83)*
S		-0.223 (0.55)	-0.061 (0.16)
Conflict Interaction Level		-0.141 (0.52)	-0.447 (1.92)**
Constant		-2.760 (.763)**	-4.066 (7.32)**
p (selection effect)			0.757 (5.04)**
Observations	522865 (dyad years)	3094 (dispute dyads)	522827 (dyad years)

Robust z-statistics in parentheses

\* significant at 5% level; \*\* significant at 1% level

**Table 4: Weibull Competing Risks Duration Model of War vs. Settlement Outcomes**

	Model 1		Model 2	
	Time to War	Time to Settlement	Time to War	Time to Settlement
<b>Time-varying</b>				
Bal of Capabilities (Side A/Total)	0.172 (0.41)	0.152 (1.12)		
Bal of Capabilities (Max/Total)			1.902 (1.86)	0.699 (2.22)*
Change in BOP	0.466 (0.51)	-0.263 (0.44)	0.444 (0.48)	-0.248 (0.41)
Abs[Change in BOP]	-2.722 (2.60)**	0.571 (0.90)	-2.598 (2.43)*	0.604 (0.95)
Total # Participants	-0.234 (3.98)**	0.176 (1.93)	-0.250 (4.17)**	0.167 (1.87)
<b>Static</b>				
At Least 1 Major Power (Originators)	-1.229 (3.48)**	0.419 (3.63)**	-1.305 (3.69)**	0.373 (3.22)**
Contiguity (Any Type, Originators)	-0.964 (2.32)*	0.179 (1.53)	-0.845 (2.06)*	0.218 (1.83)
Joint Democracy (Originators)	2.677 (1.42)	-0.655 (3.71)**	2.709 (1.40)	-0.663 (3.75)**
Alliance (any type) (Originators)	-0.339 (0.88)	-0.456 (3.95)**	-0.271 (0.71)	-0.447 (3.87)**
S (originators)	-0.795 (0.98)	0.895 (3.33)**	-0.886 (1.12)	0.875 (3.26)**
Conflict Interaction Level (Originators)	-0.375 (0.93)	-0.811 (6.66)**	-0.521 (1.27)	-0.854 (6.93)**
<b>Non-proportional Hazard Interactions</b>				
Joint Democracy * Time	0.036 (2.16)*	0.005 (2.73)**	0.037 (2.08)*	0.005 (2.75)**
Conflict Interaction Level * Time	-0.001 (0.94)	0.000 (0.79)	-0.001 (0.85)	0.000 (0.87)
At Least 1 Major Power * Time	-0.000 (0.25)	-0.002 (3.67)**	-0.000 (0.23)	-0.002 (3.68)**
Constant	11.181 (11.77)**	2.488 (7.33)**	9.726 (8.20)**	2.025 (5.04)**
p (duration parameter)	.698 (0.079)	.486 (.0068)	.703 (.054)	.487 (.0069)
Subjects (MIDs)	2262	2262	2262	2262
Observations (Intervals)	3429	3429	3429	3429

**Notes:**

Absolute value of z-statistics in parentheses

\* significant at 5% level; \*\* significant at 1% level

Coefficients shown in Accelerated Failure Time Metric - (positive = longer time; negative = shorter time)

Figure 1: Effect of Balance of Forces on Expected Duration to War/Settlement

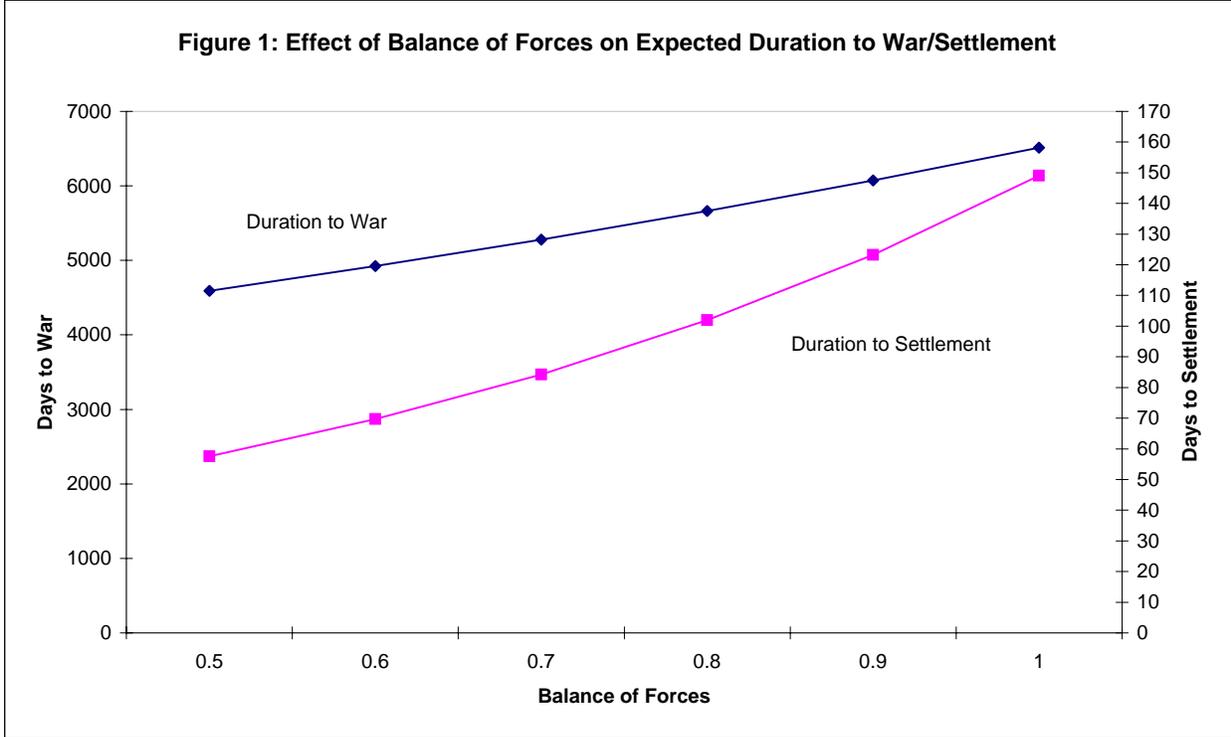
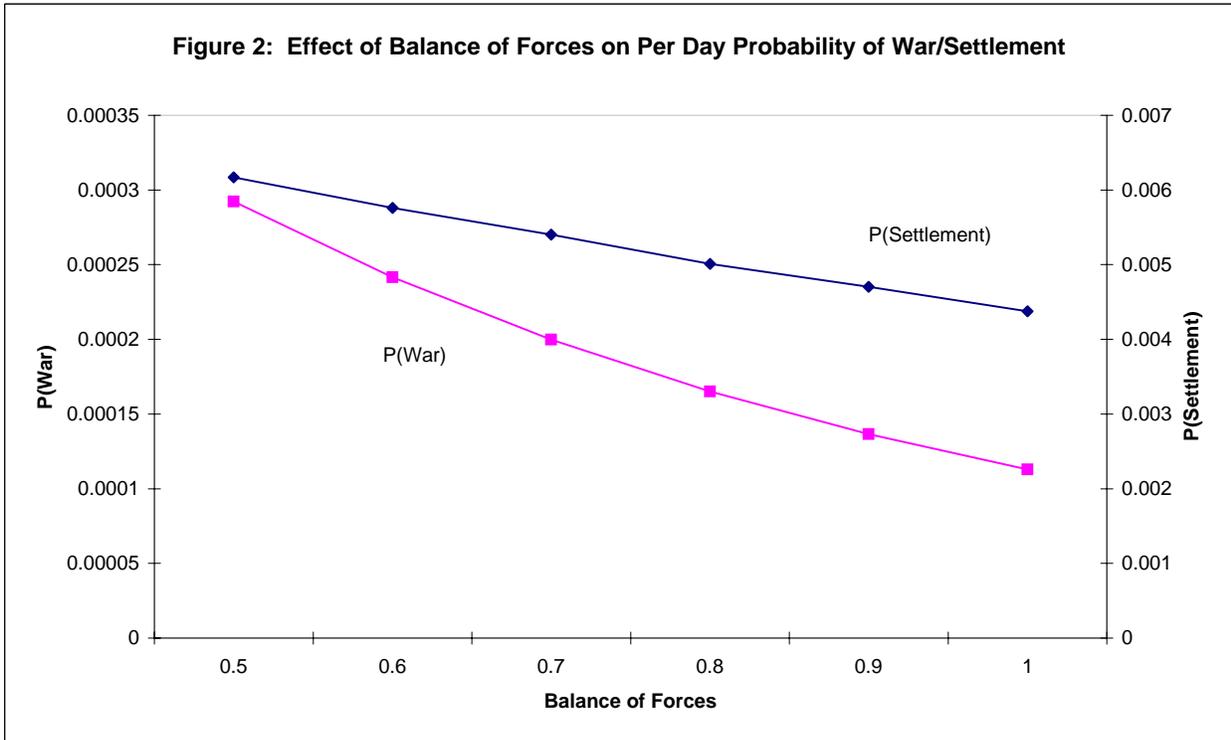


Figure 2: Effect of Balance of Forces on Per Day Probability of War/Settlement



**Figure 3: Effect of Democracy over Time on per day Probability of War/Settlement**

