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Douglas M. Gibler
University of Alabama

Mark D. Nieman
Iowa State University, mdnieman@iastate.edu

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Douglas M. Gibler[†] and Mark David Nieman[‡]

Abstract

Democracies are thought to behave differently than other states when cooperating in alliances, organizations, trade, and a host of other international institutions. We contend, however, that these democratic differences largely depend upon geopolitical environments that make cooperation possible. Though studies have demonstrated endogeneity between democracy and peace, few analyze the effects of this joint relationship on democratic differences in cooperative foreign policy behavior. We address this using the alliance literature as an example. We argue that alliances are used to either deter aggression or serve as conduits to advance other goals. Alliances that deter occur in dangerous environments, while those that serve other purposes cluster in peaceful environments. We find that alliances used to deter are particularly unreliable "scraps of paper", and that the general reliability of alliances is concentrated among those existing in already-peaceful environments, which are unlikely to be invoked. By jointly modeling regime type and political environment using data on alliance termination from 1920–2001, we show that alliance reliability is a function of the latter rather than the former. Our argument has important ramifications for a host of literatures focused on regime type, as well as current debates over the effectiveness of democratic deterrence.

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†Institute for Social Science Research, University of Alabama, Tuscaloosa, AL.

‡Department of Political Science, Iowa State University, Ames, IA.

Since the establishment of the empirical finding that democracies rarely fight one another, many studies have sought confirmation of democratic differences in other types of empirical relationships. Democracies are thought to trade more with other democracies, are more likely to form and cooperate in intergovernmental organizations, and are more likely to ally with each other and be reliable partners. These are just a few examples of the larger democratic peace research program.

We believe these types of inferences are unwarranted, however, since the explanations often miss an important point: democracies tend to be clustered across the globe in mostly peaceful regions. As democratization is more likely in peaceful environments, analyses examining any type of democratic difference must be careful to separate the independent causal effect of democracy on political outcomes from that of the political environment. Since democracy is itself at least partially determined by the political environment, a failure to model both the direct and indirect effect of the political environment on policy outcomes will incorrectly attribute the indirect effect of the political environment to democratic institutions. This, in turn, makes it easier to find statistically significant differences in foreign policy behaviors across regime types. We argue that once the political environment is accounted for—and this source of bias is eliminated—many of the differences in foreign policy behavior between regime types will be eliminated.

We focus our analysis on democratic differences in alliance behavior, and build on a recent study finding democracies to be more reliable partners. Our basic argument is that states use alliances to either (a) counter threats or (b) perform other functions, and that the former are less likely to be honored. An a-type alliance may also be re-purposed as a b-type once the intial threat has subsided. This classification of alliances has important implications on assessing the relationship between democracy and alliance violations: a-type alliances that successfully deter threats produce peaceful environments, which encourages democratization, resulting in the alliance being either re-purposed as a b-type or allowed to expire. Thus, only a-type alliance are “at-risk” of violating their alliances; since b-types do not face external threats, their obligations are unlikely to be invoked. As democracies make up a disproportionate proportion of b-type alliances, and democracies are more likely to arise in more peaceful political environments, a failure to appropriately model threat environment results in a conflation of the effect of regime type with that of the geopolitical environment.

We address this by using a split-population logit with an instrumental variable to model the likelihood of conflict during the duration of an alliance, and introduce this likelihood into the study of democratic reliability. The split-population model allows us to statistically account for the risk of alliance invocation, and subsequent potential abrogation, due to the political environment, while the instrumental variable accounts for possible endogeneity between alliance reliability and threat environment. We find that the association between democracy and alliance reliability appears to actually be a function of the political environment facing the state. Further, our analyses suggest the combined, threat-and-reliability model outperforms the reliability model alone.

Our argument should apply to other ancillary findings suggesting democratic differences as well. Any time peace affects cooperation—whether it is trade, institutions, or similar types of cooperation—the endogeneity we document will pose problems for confirming that democracies behave differently. We begin our argument in the next section with a brief review of the democracy and alliance literature.

Alliance behavior

Traditional alliance theory is replete with arguments that threats to the state cause alliance making in order to deter aggressors. Morgenthau (1960) called it external balancing: faced with threat and unable to respond quickly enough with an increase in internal capability, leaders sought partners in other states to help them avoid, or survive, against external threats to their sovereignty. Alliance-making in this manner forms a key component of traditional realist theory (see also, Waltz 1979; Walt 1985), and most empirical studies find that threats do matter in determining whether alliances form (Siverson and Emmons 1991; Lai and Reiter 2000; Johnson 2017). The implication of these arguments and findings is that alliances covary with threat, and, when threat diminishes, the need for the alliance does as well. Alliances are, as the famous phrase puts it, "scraps of paper" to be torn as situations change.

Not all alliances, however, are responses to threat. Instead, alliances may be used to facilitate a number of different tasks (Schroeder 1976; Altfeld 1984). Alliances, for example, may be an instrument used to resolve contentious issues (Gibler 1996, 1997; Weitsman 2004; Mattes and Vonnahme 2010). Alliances may also be used as a method of gaining influence over smaller states (Morrow 1991; Lake 2009; Johnson 2015). We refer to the traditional, power-aggregating alliances as *a*-types and those formed for other purposes as *b*-types.

Consistently identifying these different types of alliances *ex ante*, however, has proven difficult. The empirical focus has been on either specific issues, such as territorial settlement treaties, or the presence of asymmetric capabilities within the alliance. Classifying alliances in this way, however, negates cases where alliances are mutually implicated, at least to varying degrees. The former requires that alliances which resolve issues, such as the 1887 pact between Prussia and Russia or the 1960 USSR and China alliance, are not also, at least in part, power aggregating.[1] The latter forces an assumption that some trade-off between capabilities and autonomy is the primary reason for why major powers would partner with minor powers that offer little in the way of additional military capabilities.[2]

Democracies in alliance

Related to the two general alliance types has been the growth of studies associating democracies with alliance behavior that is quite different from traditional alliance theory expectations. Democracies may engage in deterrent alliances, for example, but their commitments

[1]The Prussia-Russia alliance addressed disputes in the Balkans and the Dardanelles, while the USSR-China alliance resolved a border dispute between the two parties (Gibler 1997).

[2]While asymmetric alliances may be an effort to "buy influence" from the perspective of major powers, they can also provide access and basing rights necessary to confront distant adversaries. From the perspective of minor powers, these arrangements do supplement their security (McManus 2018) and increase the minor power's bargaining position vis-à-vis rivals in disputes (Langlois 2012).

are seemingly not scraps of paper. Their commitments are more likely to deter other states and, when threatened, to be honored by the democracies involved.

The theoretical argument for this has focused on the idea that cooperation is more likely among similar types of states. Leeds (1999), for example, develops a model where cooperation is essentially a method of policy coordination, and leaders consider the likelihood of agreement fulfillment—foreign policy changes—when forming or proposing cooperation. Without the likelihood of fulfillment by the other actor, then there is little incentive to alter state policies when it will not be reciprocated. These audience costs seemingly make it more likely that democracies make better alliances and have longer-lasting cooperation.

A number of studies have empirically analyzed whether pairs of democracies tended to "flock together". Siverson and Emmons (1991) found some evidence that democracies were more likely to form alliances with other democracies, but there were strong period effects. The finding was consistent in the post-World War II era data, but not prior (see also Kimball 2006). Lai and Reiter (2000) revisited this empirical claim and found a strong relationship for joint threats to the dyad. Jointly-democratic dyads were more likely to be involved in defense pacts than mixed or fully non-democratic dyads, and regime similarity systematically predicted both defense pacts and other types of commitments in the dyad.[3]

Alliances composed of jointly democratic states also appear to last longer than those comprised of other types of states. Gaubatz (1996) found that jointly democratic alliance dyads lasted twice as long as other alliance dyads. There was no empirical difference, however, between mixed-regime and non-democratic dyads. This led Gaubatz (1996, 135) to conclude, "democracy by itself does not appear to either increase or decrease the ability of a state to make commitments to nondemocracies." Bennett (1997), using the same data, took the average number of liberal regimes in an alliance and found a positive, statistically significant effect increased alliance duration. The substantive effects were especially strong,

[3]McManus and Yarhi-Milo (2017) suggest that while democracies are more likely to engage in public acts of support, their cooperation with autocracies is often less public in order to avoid domestic backlash.

since all-liberal alliances increase the duration of an average alliance by almost fifteen years.[4]

Finally, democratic states have also been found to be more reliable alliance partners than nondemocracies. When confronted with threats to the alliance, democracies are more likely to honor the provisions of their alliances because their leaders risk sanction by their publics when reneging. Thus, among all the states in alliances, democracies are expected to be better partners—more reliable, less likely to terminate their alliances, and less likely to violate the terms of the agreement (Leeds 2003; Leeds and Savun 2007; Leeds, Mattes and Vogel 2009).

Do political institutions explain these differences?

Some evidence suggests that democratic differences in alliance behavior may be less well understood than it appears. For example, regarding alliance formation, Gibler and Wolford (2006) questioned the research design used by both Siverson and Emmons (1991) and Lai and Reiter (2000). Gibler and Wolford argued that these studies were not technically examining alliance formation but were instead identifying whether dyads were allied. By switching the analysis from whether a dyad was allied in a given year to focusing on a dyad at the time of alliance formation, Gibler and Wolford demonstrated that democracies were not more likely to form alliances; instead, states were becoming democratic *after* having formed an alliance.

Gibler and Wolford's (2006) analysis also showed that the peace provided from the deterring effect of large, regional defense pacts promoted the development of democracy. In fact, over 90% of jointly-democratic alliance-dyads exist within three broad, regional defense pacts: NATO (55%), OAS (29%), and the WEU (7%). The regional clustering associated with these regional defense pacts confirmed a more complicated relationship between democracies and alliance-making. It also hints that the distribution of democracies in alliance is at least partially determined by something within their political environment.

This finding raises questions about the reliability of other alliance outcomes associated with democracy. For example, given the logic of cooperation among similar regime types

[4] It is worth noting that only a handful of alliances (less than 1% of the data) were comprised solely of democratic states at the time of alliance formation.

outlined by Leeds (1999) and Lai and Reiter (2000), it is clear why democracies may be more reliable allies with other democracies, but it is less clear why democracies would unilaterally restrict their options when interacting with nondemocracies given the latter's expected higher degree of defection. While democratic states may simply be less willing than nondemocracies to break their international agreements, this commitment is not evident in other policy areas. Democracies do not, for example, honor their monetary commitments (Simmons 2000), nor their territorial treaties (Chyzh 2014), more than other regime types. Moreover, Gartzke and Gleditsch (2004) looked at whether alliance partners intervene in response to their obligations and found that democracies were actually less reliable than other states. Taken together, these additional results suggest that differences in alliance behavior often attributed to domestic institutions may instead be driven by some omitted factor.

Peaceful environments and democracy

One potentially omitted factor is the political environment around a state: specifically, how threatening a political environment is may shape a state's foreign policy behavior (Vasquez 2009). For example, alliance formation is often observed by states in order to counteract a threat (Kimball 2006; Johnson 2017). Yet, peace—or the lack of threat—encourages or even causes democracy (Gleditsch and Ward 2006; Gleditsch 2009; Gibler 2012). This creates a puzzle in terms of alliance behavior because, without a threat, democratic states should have no need for alliances. Nevertheless, democracies do make and maintain alliances.

Peace causing democracy is not a new argument, of course, and has developed over time and been integrated into the larger democratic peace project. Russett and Oneal (2001, 37), for example, contend in their seminal work that "Democracy is easier to sustain in a peaceful environment," and "external threats become reasons or justifications for suspending normal civil liberties, elections, and constitutional government." Their model of a Kantian peace recognizes the endogenous "feedback loops" from peace to democracy, trade, and international organization, so there is an explicit recognition that peace at least partially causes democracy even among some of the staunchest democratic peace advocates.

The problem for those who study democratic differences is that *any* degree of endogeneity between the political environment and democracy will introduce bias into additive models of international conflict. Even a weakly-endogenous relationship, such as that suggested by Russett and Oneal (2001), will bias estimates of the standard error of any variable that is correlated with both peace and democracy. Since properly specified models will include only variables directly related to conflict, by definition, and democracy is a common predictor of the lack of conflict, the degree of bias will turn mostly on the correlation between a variable of interest and democracy. Given the large amount of effort that has been used to determine that democracies are different from other states in their relations, this implies far-reaching concerns for studies examining the ancillary properties of the democratic peace.

A strongly endogenous relationship—or the case where democracy is a result of a peaceful environment—may have even more troubling implications for existing studies. Should democracy be dependent in some fashion on the establishment of peace (see, e.g., arguments by Thompson (1996) or Gibler (2012)), then democratic differences in a particular variable may simply underscore a more pervasive sample-selection process that made these cases observable. Alliances among democracies would, almost by definition, be more likely to be those formed or continuing after a threat has subsided. In other words, if peace is causally related to both democracy and cooperation, studies that fail to explicitly model this when looking at the effect of democracy on cooperation will suffer from a specific form of omitted variable bias: functional form misspecification, where the omitted variables represent nonlinearities—such as those introduced by selection processes—between the dependent and independent variables (Heckman 1979; Signorino and Yilmaz 2003).

We contend that this is a problem facing many studies of the axillary effects of democracy, such as the finding that democracies are more reliable alliance partners. Democracies exist within more peaceful political environments than other states (Ward and Gleditsch 2002).

Without a clear, immediate threat, alliances that democratic states participate in are unlikely to ward off potential enemies, as their are none. This implies that either these alliances are (1) formed for purposes other than deterrence, or (2) maintained even after the threats it was designed to counteract have subsided. The former indicates that some alliances are formed as b-types, while the latter suggests that a-types can transform into b-types.

Addressing alliance formation, there are a number of reasons that states seek b-type alliances that perform other functions other than power aggregation and deterrence. First, alliances can be used to resolve other contentions issues. Gibler (1996, 1997), for example, identifies a number of alliances that, rather than aggregate power, serve instead to resolve outstanding territorial disputes. Second, major powers often use alliances to consolidate their influence over minor powers (Morrow 1991; Lake 1996). Finally, alliances may be used to signal policy-alignment (Lake 2009; Nieman 2016), specifically when bandwagoning with hegemonic powers (McDonald 2015; Mousseau 2019).

Alliances that continue after threats subside raise an interesting question: why would states maintain seemingly antiquated alliances? One explanation is that such alliances are re-purposed, whether formally or informally, to serve new or additional purposes. Rather than negotiate a new treaty, cooperative agreement, or international organization, it may be easier to use the existing alliance structure to coordination cooperation elsewhere. While alliances are not costless—well-functioning alliances, in particular, require information sharing and even joint military exercises (Morrow 1994; Fearon 1997)—neither is creating new agreements and organizations. Given that an alliance already exists, states in peaceful environments may find that this existing institution can fruitfully perform tasks other than deterrence, such as facilitating coordination on counter-terrorism, border surveillance, or even encourage trade. In this way, a-type alliances transform into b-types, remaining a valuable foreign policy tool for each member, even in changing political environments.

That both democracies and b-type alliances are likely in peaceful political environments has profound implications for evaluating whether regime type affects alliance reliability. For instance, due to a lack of external threats, b-type alliances are less likely to be formally invoked. This, in turn, makes them less likely to be abrogated, more likely to last longer, and more likely to be institutionalized over time. Since states democratize in these same peaceful environments, if one ignores the role of the peaceful political environment, they may erroneously attribute the effect of the political environment to the democratic institutions of those alliance members. In other words, when we observe democracy correlated with reliable, durable, and institutionalized alliance partnerships, we are really be observing the effects of peaceful environments on both democracies and alliance behavior.[5]

That political environments vary by their threat level implies that there are two distinct ideal types of alliance within the overall sample of alliances: a sub-sample of alliances that act as traditional, power-aggregating (a-type) alliances—those within a more threatening environment—and a second sub-sample that primarily serve other purposes (b-types), such as non-military cooperation—those in more peaceful environments. Alliances drawn from the former are significantly more likely to be invoked, and constitute the sample that most accurately tests whether democracies are more reliable alliance partners, than alliances drawn from the latter. This argument leads to two hypotheses:

H1 (Political environment): States in peaceful political environments are less likely to abrogate their alliance commitments.

H2 (Democratic institutions): Once the political environment is accounted for, democratic states are no more reliable alliance partners than other states.

Research design

We test the effect of a state's conflict environment and regime type on its propensity to violate alliance agreements by analyzing all members of bilateral alliances formed during the period 1919–1989. We use data from the Alliance Treaty Obligations and Provisions dataset

[5] In quantitative studies, this means that, unless explicitly modeled, democracies are attributed the indirect effect of peaceful environments, *even if they include a measure for peaceful environment.*

(Leeds et al. 2002; Leeds and Mattes 2007), which consists of 234 bilateral alliances. The directed alliance member-year is our unit of analysis because it allows us to assess which party violated the terms of an alliance. The temporal domain of our analysis is 1920–2001. We use a split-population logit estimator to probabilistically identify and separate alliances that exist in peaceful political environments—i.e. those that are unlikely to be invoked—from those alliances in threatening political environments. We adopt an instrumental variable approach to account for any endogeneity between alliance reliability and militarized conflict.

Methodology

We expect alliances to separate into two stylized types—those that are "at-risk" of being invoked and those that are not—based each state's external threat environment. States in more threatening environments are at greater risk of having their alliances invoked, which provides opportunities to violate the alliance's terms, while states in safer environments have fewer opportunities to commit violations. If our argument is correct, then full-sample estimates of the predictors of alliance violation, which ignore these different types of environments, will recover biased estimates.

Ignoring the conditioning effect of threat environments, and treating all alliance observations as equally at-risk of entering the set of states that may violate their alliance commitments—which is true for traditional additive binary-choice estimators, such as logit or probit—is a type of model misspecification (Heckman 1979; Signorino and Yilmaz 2003). Unfortunately, we cannot definitively know *ex ante* with certainty which alliances are unlikely to be invoked; leaders do not often volunteer whether they considered violating their alliance obligations. Instead, we only have data on whether an alliance was violated, but not direct data on the degree that the alliance is "at-risk."

To address these data limitations, we use a split-population logistic regression model (Xiang 2010; see also Beger et al. 2011). A split-population logit is a type of mixture model, where an outcome variable is a function of two processes.[6] The logic of the estimator is that there are two populations in the data, and entry into each population can be estimated *probabilistically.* Though the structure of the alliance data does not let us directly observe which cases are actually in the "at-risk" pool, it can be estimated. The estimator does this by using two equations: one equation that functions as the selector, identifying *relevant* observations to include in the at-risk sample, and a second equation that estimates the outcome of interest on these relevant observations. The *relevance* equation affects the *outcome* equation probabilistically: some cases are treated as more "at-risk" than others, and this probability conditions estimates of the outcome equation.[7]

More formally, the estimator treats the outcome variable as a function of two processes:

$$Y_i = 0 \text{ with probability } (1 - R_i) + (R_i)(1 - V_i) \tag{1}$$

$$Y_i = 1 \text{ with probability } R_i V_i \tag{2}$$

where R and V are cumulative distribution functions of a binary choice model (see Xiang 2010, 487-488). R_i represents the probability that a case is *relevant* to the sample—that the observation should be in the outcome equation, i.e. an "at-risk" state[8]—and conditions V_i, which represents the probability of violating an alliance. These probabilities can be specified

[6]All selection and zero-inflated models are types of mixture models, with the familiar censored probit-types of selection models (e.g., Heckman 1979; Sartori 2003) including data on the outcome of the first stage, and more recent extensions modeling selection when there are not data available on the outcome for the first stage (Xiang 2010; Nieman 2015, 2018; Bagozzi 2016; Bagozzi and Mukherjee 2012). The relationship between selection and zero-inflated models is such that the probit variant of the split-population model is mathematically equivalent to Poirier's (1980) bivariate probit with partial observability (Xiang 2010, 488).

[7]Partial observability models, of which split-population logit and other zero-inflated model are a type, have been shown to correctly recover the sign and significance for parameters, even if variables are specified in the wrong equation, permitting accurate hypothesis testing (Nieman 2015, 438-439), though there are some criticisms of their overall reliability (Rainey and Jackson 2017). In this particular case, however, the alternative to estimating a partially observed model is to simply ignore bias induced by the two types of political environment altogether, which may result in inaccurate hypothesis testing and substantive effects (Xiang 2010). To address concerns of reliability, we formally assess model fit and conduct robustness checks in the Appendix in Tables A.2, A.3, and A.4.

[8]This implies, of course, that the inverse, $1 - R$, is the probability of an observation selecting out of the "at-risk" subsample—i.e. being identified as "not at-risk."

as:

$$\Pr(Y_i = 0) = [1 - \Lambda(Z_i\gamma)] + [(\Lambda(Z_i\gamma))(1 - \Lambda(X_i\beta))] \quad (3)$$

$$\Pr(Y_i = 1) = (\Lambda(Z_i\gamma))(\Lambda(X_i\beta) \quad (4)$$

where Z and X are vectors of covariates associated with the relevancy and outcome equations, respectively, γ and β the accompanying parameter estimates, and Λ is the logistic link function. Equation 3 can, of course, be simplified as $\Pr(Y_i = 0) = [1 - (\Lambda(Z_i\gamma))(\Lambda(X_i\beta))]$. The likelihood function of the split-population logit is written as:

$$\mathcal{L} = \prod_{i=1}^{n}[(\Lambda(Z_i\gamma))(\Lambda(X_i\beta))]^{y_i}[1 - (\Lambda(Z_i\gamma))(\Lambda(X_i\beta))]^{1-y_i} \quad (5)$$

and estimates of β and γ are recovered via maximum likelihood estimation.[9]

In other words, the estimator treats cases where $Y = 0$ in the data as being the outcome of either (1) not at-risk, or (2) being at-risk, but not abrogated, whereas $Y = 1$ is the outcome of being both at-risk and abrogated. This modeling approach allows us to statistically separate b-type alliances members in non-threatening environments, which are unlikely to have their alliance enacted, from a-type alliance member, where threats to member states increase the possibility of alliance terms being invoked. As an example, suppose an alliance is formed during a relatively high-threat time period in which the likelihood of conflict in that dyad-year is 35%. The split-population logit would then assign 35% of the estimation to the relevance equation since it is part of the at-risk population of alliances. The remaining percentage of the estimation would be considered not at-risk and would be grouped with the alliances formed during more peaceful periods. The result of this weighting is analogous, in a sense, to including an interaction term, since the model corrects for the conditional effect of the sample-selection process. However, rather than interacting two variables, the interaction is between the full set of variables from the outcome and relevance equations.

[9] As the estimator is not included as an 'off-the-shelf' option with most statistical software, we include sample stata code on p. 13 of the Appendix for interested readers.

Though this process does weigh each observation by its political threat environment, the test remains quite conservative. The likelihood of threat in any given dyad-year is often much smaller than the 35% figure used in our example, so for each alliance we are only assigning a small portion of its effect on the overall model to the relevance equation. Conversely, the current standard approach within this research tradition is to conflate such cases as peace settlements or trade pacts that have alliance clauses with the offensive and defensive pacts formed in the years prior to major wars. As we demonstrate below, this standard assumption significantly affects whether several key variables predict alliance reliability or failure.

Data

Our dependent variable is *alliance violation*, which captures whether a state abrogates its alliance commitments. We follow Leeds, Mattes and Vogel (2009, 469-470) and code *alliance violation* as 1 if state A violated the terms of an alliance. They code an alliance as abrogated if (1) a major provision is violated and governments do not agree to continue with the alliance or (2) one government unilaterally ends the alliance prior to its terms. There are 74 instances of *alliance violation* among the 234 alliances in the data set, roughly 32% of all alliances.[10]

Next, we specify the relevancy and outcome equations of the split-population logit (Xiang 2010). Following our theory, we specify the *relevance equation* with predictors related to a state's geopolitical threat environment. We expect that the absence of a threatening environment is associated with fewer opportunities for a state to violate the terms of its alliances, as there are fewer reasons to invoke an alliance. Alliances that are formed or maintained in such environments are likely for peaceful, cooperative purposes, rather than power-aggregation. Alliances formed and maintained in more threatening environments, however, are more likely to invoked. Such alliances are also more likely to be broken, as states called upon to assist an ally may determine that it is no longer rational for them

[10] After accounting for missing data, there are 70 violations in the exact replication of Leeds, Mattes and Vogel (2009) reported in Table 1 Model 1, 68 violations in the 1920–2001 samples reported in Models 2 and 3, and 34 observations in the post-1949 samples reported in Models 4 and 5. See Table A.6 in the Appendix for the complete list of abrogated alliances.

to honor their agreed terms. We specify the *outcome equation*, with predictors that have previously been identified with affecting alliance commitment, namely the probability of a violation. We build on Leeds, Mattes and Vogel (2009) and include variables such as joint democracy and whether a state has had a change in their leader's societal coalitions, as well as dyadic- and alliance-specific features.

Relevance equation

Our primary measure of a state's threat environment is *territorial threat*. We conceptualize *territorial threat* as the maximum level of cross-border threat that a state faces. We operationalize this as the maximum predicted probability of a fatal militarized interstate dispute for state A for all contiguous neighbors. This value provides a continuous, latent measure of *territorial threat*. We construct a time-varying measure of *territorial threat* for each observation in the data set.

The predicted probability of a fatal militarized interstate dispute is estimated using data from Gibler and Tir (2014, Table 1). Gibler and Tir emphasize *territorial* predictors of conflict among contiguous neighbors, such as previous peaceful and violent transfers of territory within a dyad, the highest level of militarization of a state's neighbors, previous territorial MIDs within a dyad, and the age of the dyad's border. They include controls for whether there is a shared colonizer, the presence of a civil war for either state within a dyad, or defense pacts with neighbors. The results for the logit model used to construct *territorial threat* are presented in Table A.1 of the Appendix.

We use this measure for two reasons: first, it best captures the theoretical concept of a dangerous neighborhood. While we include other possible sources of a dangerous political environment, we expect *territorial threat* to be the best identifier of "at-risk" alliance observations. There is strong evidence that territorial disputes are the single best predictor of militarized conflict (Bremer 1992; Vasquez 1995, 2009; Reed and Chiba 2010). Yet, it is the threat of conflict—rather than just its realization—that is likely to spur participation in

a-type alliances. Neighbors with territorial disputes are likely to appear as more stressful and immediate threats than other potential sources, as neighboring states are usually able to quickly project power to their borders. Thus, we expect territorial threats to be the mostly likely source of the power aggregating alliances described by traditional alliance theory.

Second, using an instrumental variable—i.e. the predicted probability of conflict—rather than looking at observed militarized conflict is advantageous in methodologically, as it helps avoid issues related to endogeneity between alliance reliability and militarized conflict.[11] We account for uncertainty in our estimate of the instrumental variable by taking 10 draws from the estimated distribution of the maximum predicted territorial threat, and using these to calculate point estimates and standard errors, following Rubin's (1987) formula for multiple imputation (see Boehmke, Chyzh and Thies 2016, for a similar re-sampling approach).[12]

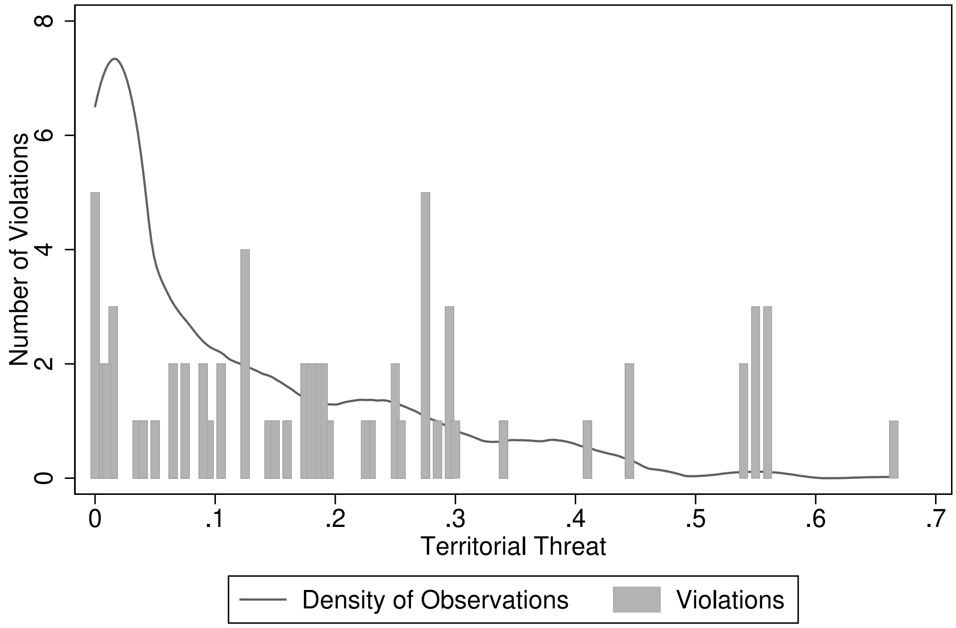
As initial evidence of a relationship, we find that 57 of the 74 alliance violations ($\approx 77\%$) have a *territorial threat* above the median territorial threat for all allied states. Figure 1 displays the kernel density of *territorial threat* for the observations within the sample. It also reports the frequency of *alliance violations* at differing threat levels. The figure shows that at low threat levels, alliances are violated less than expected by chance, while at high threat levels there are more violations than would be expected.

We also control other factors that may also influence the hostility of a state's political environment. We include the *number of borders* and *proportion of democratic borders*. States with more boarders have more opportunities for conflict (Vasquez 1995, 2009), though this is mitigated as a greater proportion of a state's neighborhood is democratic (Kadera, Crescenzi

[11]Whether an alliance is violated is unlikely to be related to the exogenous variables used to construct the instrument: the correlation between *alliance violation* and each exogenous variable to construct the instrument is $r < |0.1|$. Moreover, our instrument appears to be strong; the difference in the F-statistic between nested logits is 19.12, well above the threshold of 10 used to indicate that it does not suffer from weak instrument bias (Stock and Watson 2011). For the F-test, we estimate a logit with *alliance violation* treated as a function of the independent variables from the relevance equation.

[12]The point estimate for each parameter is the mean of the 10 draws, or $\frac{1}{10}\sum_k^{10}\beta_k$, while the standard error is the average of the estimated variances within the datasets plus the variance in the point estimates across datasets, or $\sqrt{\frac{1}{10}\sum_k^{10}s_k^2+(1+\frac{1}{10})\sigma_\beta^2}$, where s_k^2 is the standard error for dataset k and σ_k^2 is the variance in β between datasets. See Rubin (1987). As few as 5 draws from the estimated distribution is sufficient to incorporate uncertainty (Mislevy 1991).

Figure 1: Density of Territorial Threat and Frequency of Violations.



Note: The frequency of violations is overlayed with the kernel density of Territorial Threat within the sample from Table 1 Model 3.

and Shannon 2003). These variables are based on Stinnett et al. (2002) and Marshall and Jaggers (2014). We control for *rivalry*, as this indicates already heightened tensions (Diehl and Goertz 2000; Rasler and Thompson 2006), and whether a state is a *major power*, as these are more generally more active and attractive alliance partners (Chiba, Martinez Machain and Reed 2014). Data on rivalries and major powers are obtained from Klein, Goertz and Diehl (2006) and the Correlates of War Project (2016). We include an indicator variable for the *Cold War* to account for systemic effects owing to a bipolar system (Bennett and Stam 2004). Economically developed states, often clustered geographically, may have complex economies that create norms that constrain conflictual behavior (Mousseau 2003). We operationalize *economic development* as the log of energy consumption per capita (Singer, Bremer and Stuckey 1972).[13] Finally, we control for whether a state is an *oil producer*, as such states are more conflict prone (Colgan 2013).

[13]Economic consumption per capita is available for a broader time frame than GDP/capita and is are correlated at $r = 0.7$.

Outcome equation

We rely on Leeds, Mattes and Vogel (2009, Table 1, Model 1) to specify our outcome equation. Leeds, Mattes and Vogel (2009) find that changes in a leader's core constituency and (the absence of) democratic institutions are highly correlated with alliance violations. Their conclusion affirms previous studies that find democratic governments to be seemingly more reliable alliance partners. *Change in leader's societal coalition* is measured as a binary variable that is 1 if there is a change in the core domestic supporting coalition of state A in a year. *Democracy* is coded 1 if state A has a score ≥ 6 on the -10 to 10 Polity IV index (Marshall and Jaggers 2014).

Leeds, Mattes and Vogel (2009) include several dyadic measures expected to decrease the reliability of international commitments. *Change in international power* is a binary variable measured 1 if there is a change of >20% in either state since the alliance was formed. *Change in political institutions* is a dichotomous variable coded as 1 if either state experiences a change in political institutions since the alliance was formed. *Change in external threat* is a binary variable coded as 1 if the level of external threat between the current year and the start year of the alliance changed by 30%.[14] *Formation of new outside alliance* is a binary measure coded as 1 if state A formed a new alliance.

Leeds, Mattes and Vogel (2009) also include four alliance-specific variables in their analysis. Each of these are expected to reduce the risk that an alliance is abrogated. *Asymmetry* is a dichotomous variable equal to 1 if an alliance includes a major and minor power. *Non-military cooperation* is a binary variable coded as 1 if an alliance has provisions linking nonmilitary issues to the alliance. *Ratification* is a dichotomous variable measured as 1 if an alliance was formally ratified. *Military cooperation* is a binary variable coded as 1

[14]This measure differs substantially from our measure of *territorial threat* in terms of both composition and by focusing on whether there is a *change* in environment. The measure used by Leeds, Mattes and Vogel is based on a variable from Leeds and Savun (2007, 1127), which represents the sum of the capabilities (Correlates of War CINC scores) for politically relevant states (neighbors and major powers) that do not share an alliance and have a foreign policy affinity score (S score, a similarity score based on alliance portfolio) below the median value in their sample (median = .775). Our measure of *territorial threat*, however, is a latent measure focused on territorial determinants of conflict by contiguous neighbors.

if an alliance includes provisions related to peacetime military cooperation. Lastly, cubic polynomials are included to account for temporal dependence (Carter and Signorino 2010).

Empirical analysis

Table 1 presents the results comparing logit and split-population logit models. We estimate five models: the first two are estimated with a traditional logit and the third is the full model estimated with a split-population logit. Model 1 provides an exact replication of Leeds, Mattes and Vogel (2009) for estimating alliance violations. Model 2 re-estimates Leeds, Mattes and Vogel, but restricts the sample to only those observations that are common to both the original and full model. This ensures that parameter estimates in the outcome question are comparable once we account for the relevance equation, as both samples contain the exact same cases. Model 3 reports the estimates of the full model using the split-population logit, which includes both *relevance* and *violation* equations. Finally, Models 4 and 5 reports estimates of the 1950–2001 period for when relevance is ignored and accounted for, respectively.[15] The top of the table reports the *outcome* (violation) equation, and the bottom of the table reports the *relevance* (the degree an observation is "at-risk") equation.

The results are interpreted in a relatively straightforward way: positive coefficients indicate that increases in a variable make the outcome for that equation more likely. Hence, positive coefficients for variables in the *relevance* equation indicate an *increase* in the probability of being in the "at-risk" sub-sample of violating an alliance, while negative coefficients indicate a *decreased* likelihood of being in the "at-risk" sub-sample.[16] Similarly, positive coefficients for variables in the *outcome* equation indicate an increased likelihood of an alliance violation.

[15] We look at a sub-sample since alliance compliance rates decline dramatically pre- and post-WWII (Berkemeier and Fuhrmann 2018).

[16] We focus on "opting into," rather than "opting out of," the at-risk sample. Our focus on observations being treated as "at-risk" or "opting in" to the outcome equation, of course, is the mathematical inverse of identifying the "zero-inflated" observations that "opt out." Reporting our results this way is consistent with previous studies using this estimation technique (e.g., Xiang 2010, 2017; Bagozzi 2016; Nieman 2015, 2018).

Table 1: Political Environment, Democracy, and Alliance Violations.

Time	LMV Original 1920-2001	LMV Reduced 1920-2001	Full 1920-2001	LMV Reduced 1950-2001	Full 1950-2001
Outcome Equation					
Change in Leader's Societal Coalition	0.889*	0.910*	5.477	1.772*	11.247
	(0.436)	(0.444)	(5.055)	(0.528)	(9.586)
Democracy	-1.322*	-1.341*	1.336	-1.813*	0.856
	(0.393)	(0.401)	(1.841)	(0.556)	(2.517)
Change in International Power	0.803*	0.877*	3.018	0.727	2.222*
	(0.330)	(0.340)	(2.247)	(0.464)	(0.928)
Change in Political Institutions	0.131	0.195	1.703	0.010	2.651
	(0.308)	(0.308)	(1.294)	(0.465)	(2.365)
Change in External Threat	0.421	0.425	0.765	-0.069	-0.673
	(0.270)	(0.278)	(1.261)	(0.445)	(1.859)
Formation of New Outside Alliance	1.070*	1.064*	2.452	1.145*	2.985*
	(0.253)	(0.261)	(1.628)	(0.380)	(1.298)
Asymmetry	-0.408	-0.503	-1.625	-0.510	-1.696
	(0.259)	(0.265)	(0.909)	(0.424)	(1.329)
Non-military Cooperation	-0.746*	-0.746*	-2.684*	0.055	-2.784
	(0.257)	(0.261)	(1.190)	(0.498)	(3.363)
Ratification	-0.083	-0.134	1.513	-0.265	1.172
	(0.354)	(0.355)	(2.299)	(0.466)	(4.741)
Military Cooperation	0.557*	0.575*	4.978*	1.043*	5.794*
	(0.180)	(0.186)	(2.224)	(0.280)	(2.552)
Time	-0.086	-0.069	-0.124	-0.000	-0.065
	(0.077)	(0.076)	(0.276)	(0.114)	(0.526)
Time Squared	0.001	-0.000	-0.003	-0.002	-0.002
	(0.004)	(0.004)	(0.010)	(0.006)	(0.022)
Time Cubed	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-4.448*	-4.464*	-3.210*	-5.660*	-3.412
	(0.440)	(0.455)	(1.447)	(0.901)	(3.267)
Relevance (At-risk) Equation					
Territorial Threat			4.198*		6.412*
			(1.126)		(2.620)
Proportion of Democratic Borders			-0.299*		-0.072
			(0.121)		(0.246)
Number of Borders			0.119		0.012
			(0.062)		(0.125)
Major Power			0.446		-0.291
			(0.511)		(1.087)
Rivalry			-0.009		-0.140
			(0.399)		(0.477)
Cold War			0.424		2.195*
			(0.309)		(1.043)
Economic Development			-0.150		-0.163
			(0.083)		(0.137)
Oil Producer			0.675		1.333*
			(0.513)		(0.540)
Constant			-4.564*		-5.178*
			(0.403)		(1.406)
Log-likelihood	-352.394	-339.988	-307.564	-177.293	-156.730
Observations (Alliances)	6612 (223)	6395 (223)	6395 (223)	4952 (139)	4952 (139)

Note: *$p < 0.05$, two-tailed. Standard errors in parentheses. Point estimates and standard errors in models 3 and 5 were calculated from 10 draws using Rubin's (1987) formula for multiple imputation to account for uncertainty in the *territorial threat* instrumental variable.

Again, the first model is an exact replication of the original Leeds, Mattes and Vogel (2009, Table 1, Model 1) study, and the results are consistent with their earlier findings: all coefficients and standard errors remain the same as in the original study. Model 2 restricts the Leeds, Mattes and Vogel (2009) sample of cases to only those observations included in both the exact replication and the full split-population model. All of the parameter estimates are approximately the same, and all relationships are in the same direction and have the same level of significance as the original analysis. Model 2 thus provides a baseline from which to compare our full split-population model.

Model 3 estimates a split-population model where territorial threat and other factors related to the political environment are treated as part of the *relevance* equation, which identifies observations that select into the pool of cases at-risk of violating their alliances in the *violation* equation. As expected, the coefficient on *territorial threat* is positive and statistically significant in the *relevance* equation. This result indicates that states are more likely to enter the at-risk population for alliance abrogation when external territorial threats are high and is consistent with H1.

Turning to the *violation* equation, we see that, after accounting for the underlying sample-selection process, the sign on *democracy* is now positive, though statistically insignificant. This result is suggestive that the previous finding of a negative and significant effect associated with *democracy* in Models 1 and 2 may, in fact, have indicated that democracies were less likely to violate their alliances not because they are more reliable, but because they exist in peaceful neighborhoods and were less likely to have military provisions invoked. Thus, the endogeneity between peace and democracy seems to explain why democracies behave differently when engaging in alliance politics. Similarly, *changes in leader's societal coalition* also fails to reach any traditional level of statistical significance.

To more formally assess whether *democracy* exerts a null effect once the political environment is accounted for, we use a technique introduced to political science by Rainey (2014). The idea is to identify the smallest 'meaningful effect', and then determine whether the estimated quantity of interest meets this threshold. If the estimate (and its 90% confidence interval) fails to meet the threshold identified as the smallest 'meaningful effect', then the effect is marginal; i.e. there is statistical evidence that the variable has little or no effect on the outcome of interest. If the estimate (and its 90% confidence interval) is equal to or surpasses the threshold, then the effect is appears to be non-marginal.

Given that estimated parameters in logit-based models are difficult to interpret directly, we follow Rainey's advice and focus on assessing whether the independent variable affects the predicted probability of the outcome of interest. In our case, we evaluate whether *democracy* exerts a meaningful effect by seeing if it reduces the likelihood of alliance violations by at least one-half of one percent (0.5%); i.e. the effect size should be less than -0.005. To do this, we take the first difference of the parameter of interest, holding all other parameters at the their mean or modal values. The 90% confidence interval *democracy* [-0.001, 0.007] is *greater* than -0.005, failing to meet the threshold, indicating that the effect of *democracy* on reducing alliances violations is negligible. This result is consistent with H2 and indicates that the influence of political institutions may actually be attributable to political environment rather than the institutions themselves.

Applying the same test to *changes in leader's societal coalition*, we find different results. We expect a meaningful effect to increase the likelihood of alliance violations by at least one-half of one percent when there is a change in a leader's societal coalition. In this case, the 90% confidence interval [-0.001, 0.020] includes 0.005, indicating that, though it is not statistically significant, we cannot rule out a meaningful effect for *changes in leader's societal coalition*.

We re-estimate the reduced and split-population models for the period 1950–2001 in Models 4 and 5. Once again, *territorial threat* is positive and statistically significant, and *democracy* is no longer statistically significant once the sample-selection process of identifying "at-risk" observations are modeled jointly with the likelihood of abrogation. Applying the more formal test of the smallest 'meaningful effect', we again expect a 0.5% reduction in alliance violations as the threshold. We again find that the 90% confidence interval for *democracy* [-0.001, 0.004] is larger, rather than smaller, than the smallest meaningful effect size. Each of these results is supportive of H1 and H2. Taken together with the previous results, there is evidence that the negative and statistically significant results associated with *democracy* in Models 2 and 4 are spurious, and their effect is instead attributable to the political environment in which democracies form and exist. Lastly, and in contrast to estimates for time period 1920–2001 reported in Model 3, *change in leader's societal coalition* is statistically significant at the $p < 0.10$-level in the period after 1950, even after accounting for the sample-selection processes of identifying the "at-risk" observations. This difference suggests that the effect of changes in a leader's societal coalition are stronger in the 1950–2001 period than before.

We formally compare and evaluate the model fits for the reduced and split-population models, for each temporal sample, using the Vuong and Clarke distribution-free tests, in Table A.2 in the Appendix. These tests indicate a strong preference for the split-population models over the reduced model. We also assess the reliability and robustness of the estimates from the split-population logit in Tables A.3 and A.4 in the Appendix. The stability of the estimates in the partially observed (relevancy) equation indicate the split-population logit is appropriate. Finally, we assess whether the initial threat environment in which an alliance is formed, rather than the current threat environment, determines the risk of future abrogation in Table A.5 of the Appendix. We find that our time-varying measure of the current threat level is robust to this specification as well.

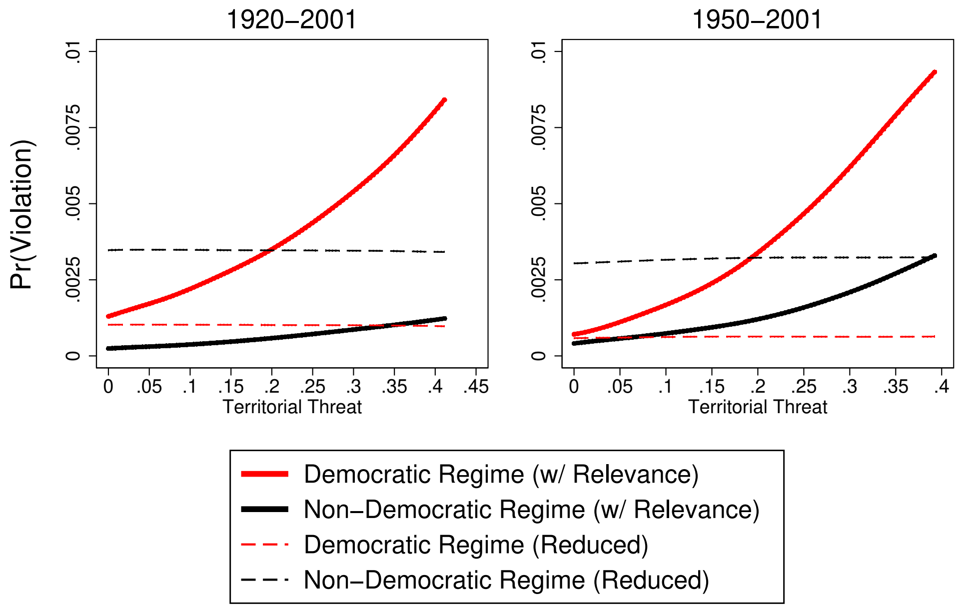
Overall, the results are consistent with our theoretical expectations. Threatening environments affect the underlying propensity of states to enter the sample at-risk of violating their alliance terms. Moreover, once the political environment is accounted for, democratic institutions do not appear to exert a significant impact on whether an alliance violation occurs. States in non-threatening environments appear less likely to be involved in the traditional capability-aggregating, a-type alliance associated with international conflict. Rather, alliances that continue after their members' external threats have subsided are often repurposed as b-types, used to deepening economic ties (Gowa and Mansfield 2004; Li and Vashchilko 2010) or to demonstrate political affinity (McManus and Nieman 2019). As states are more likely to democratize in peaceful environments, b-type alliances are more likely to consist of democracies.

Substantive effects

To illustrate the substantive effect of territorial threat and political institutions on alliance abrogation, we report predicted probabilities of alliance violations in Figures 2 and 3. In the first figure, we report predicted probabilities for two conditions: for democracies [red line] and for non-democracies [black line], after accounting for the level of territorial threat affecting the state [solid lines]. As a point of reference, we also compare these predicted probabilities to those from the reduced model [dashed lines], which does not account for the effect of territorial threat on an observation's probability of being part of the at-risk subsample. In the second figure, we repeat this procedure, reporting a change in the leader's winning coalition [red line] and when there is no change in the leader's winning coalition [black line], after accounting for territorial threat [solid lines]. The reduced model is again provided as a reference [dashed lines]. To make the substantive results more realistic, and to ensure that outliers are not skewing our interpretation, we visualize predicted probabilities of alliance abrogation for the middle 95% of values of *territorial threat* from the estimated sample. Finally, we report predicted probabilities for both the full time period and the 1950–2001 period.

Figure 2 shows that, while democracy is associated with a lower likelihood of alliance violation in the reduced model [red dashed line is below the black dashed line], in the full model democracies are associated with an increased probability of an alliance violation [red solid line above black solid line]. Moreover, increases in territorial threat raise the probability of an alliance violation regardless of whether regimes are democratic [both solid lines increase], suggesting that the threat environment is driving the change. Comparing the sub-figure for the whole sample to that for the 1950–2001 sample, it is clear that the effects for territorial threat are similar, though the difference in regime types is weaker in

Figure 2: Predicted Probabilities of an Alliance Violation, Democracy, and Territorial Threat.



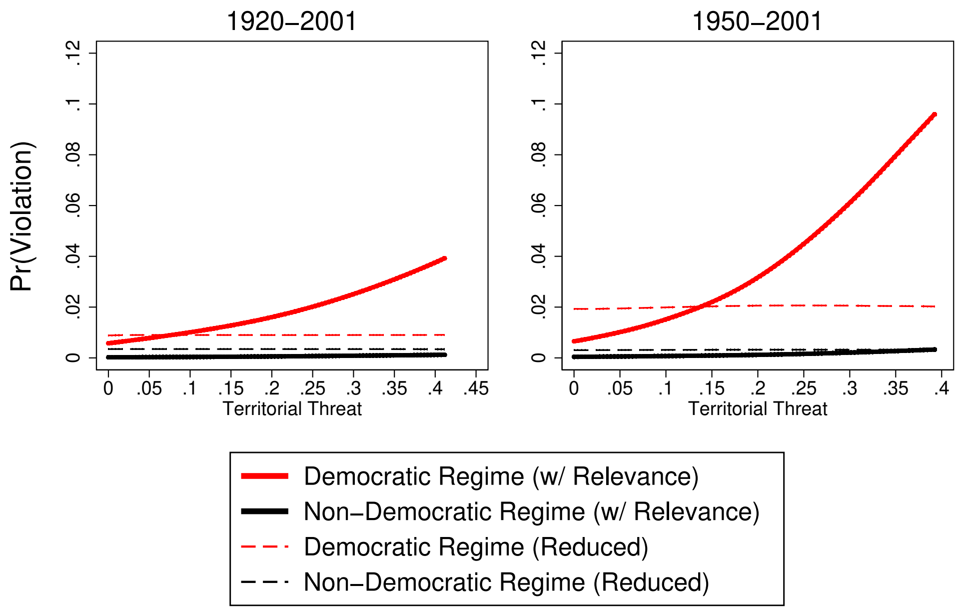
Note: Estimates for the full time period from Table 1, Models 2 and 3. Estimates for the post–1945 period from Table 1, Models 4 and 5. All variables held at mean or median. Figure reports the predicted probabilities for the middle 95% of values of Territorial Threat from the samples.

the 1950–2001 period. It is worth keeping in mind, of course, that the difference between democracies and non-democracies were shown to be negligible (or in the wrong direction), according to the test suggested by Rainey (2014).

Figure 3 looks at the impact of a change in the a leader's societal coalition. The figure demonstrates that a change is associated with increases in the probability of an alliance violation [red lines are above corresponding black lines] and also that a change in the winning coalition increases the likelihood of a violation as the degree of territorial threat rises [solid red line]. Territorial threat also increase the probability of an alliance violation when there is no change in a leader's winning coalition [solid black line], though this effect is much smaller.[17] The figure shows that threat environment significantly influences the effect of changes in the

[17]The predicted probability of an alliance violation in a democratic state, which experiences a change in their leader's winning coalition, is almost identical to the probability of a violation when there is a change in the leader's winning coalition in a non-democracy, at every territorial threat level. This suggests that the interaction of the two variables exerts little substantive impact, once territorial threat is accounted for.

Figure 3: Predicted Probabilities of an Alliance Violation, Change in Leader's Winning Coalition, and Territorial Threat.



Note: Estimates for the full time period from Table 1, Models 2 and 3. Estimates for the post–1945 period from Table 1, Models 4 and 5. All variables held at mean or median. Figure reports the predicted probabilities for the middle 95% of values of Territorial Threat from the samples.

leader's winning coalition on the probability of an alliance violation. Moreover, comparing the sub-figure for the full sample to that for the 1950–2001 sample, it is clear that this effect is stronger in the 1950–2001 period.

Notably, the territorial threat matters at all points along the spectrum for Figures 2 and 3: in low threat environments, the presence of a democracy exerts only a small risk of an alliance violation. Similarly, in the absence of an external threat, a change in a leader's winning coalition has very little effect on alliance abrogation. Instead, it appears abrogation becomes much more likely for democracies as the level of territorial threat increases. The same holds for a change in the leader's winning coalition: alliance abrogation is more likely as territorial threat increases. For the latter, these effects appear to be even stronger in the 1950–2001 period. A clear implication from examining the substantive effects is that ignoring threat environment significantly *overestimates* the effect of both democracy and changes in a leader's winning coalition at low levels of threat and significantly *underestimates* these effects at high levels of threat. That is, accounting for territorial threat, and the political environment more broadly, improves our understanding of the roles of political institutions and leadership changes in substantively meaningful ways.

Conclusion

We began this paper by pointing out that endogeneity between peace and democracy will bias additive-model estimates of many other democracy-related arguments, and we have shown that to be the case with regard to international alliances. Democracies in alliances have been thought to be more reliable, but we demonstrate that this result is likely to be spurious. Democracy is more likely to take hold in peaceful international environments, and peaceful environments seldom provoke the type of alliance making associated with aggregating capabilities to defend the state. In other words, democratic alliances are different from other types of alliances, but this has little to do with regime type.

Also noteworthy is our finding that, under some conditions, traditional alliance theories may be correct. Quantitative analyses of alliances and conflict generally pool the sample of all cases to assess conflict-proneness and reliability. Our findings suggest, however, that their are two qualitatively distinct types of alliances: power-aggregating alliances formed in hostile environments and alliances serving other functions formed in more peaceful environments. The former type of alliance is a reaction to threats to the state and is manifestly different in their behavior. These alliances are shorter statements of intentions attempting to ward off potential aggressors, and the commitments expressed in these treaties are much more likely to be abrogated. Alliances that correlate with conflict may not be more than the scraps of paper traditional theories expect. Accounting for the threat environment, therefore, may help to explain why alliances are correlated with peace in some periods, and with conflict, or even the diffusion of conflict, in others (e.g., Levy 1981; Kadera 1998; Senese and Vasquez

2008).

Finally, our argument and results have implications beyond the alliance literature, raising concerns about a number of second-order findings associated with the democratic peace research program more broadly. Current scholarship suggests that democratic states trade more often with other democracies, and democracies may also be more active in international governance. Each of these literatures, however, tend to pool samples without regard to threat environment, potentially biasing results by attributing sole explanatory power to an outcome—political institutions—rather an (at least partial) underlying causal process—peaceful political environments. This criticism, moreover, extends to almost all studies that find some type of democratic difference in state behavior. Ultimately, democratic institutions may still affect state behavior once peaceful environments take hold, but we just do not yet know. Our paper presents an important set of questions for these long-accepted relationships: without control for the effect of dangerous environments, current estimates of the effect of democracy on behavior are biased and may be spurious.

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Online Appendix

This appendix includes additional information to that in the main manuscript. First, we present the estimates of the model used to construct the instrumental variable. Second, we compare the model fits of the logit and split-sample logit for both temporal samples using the Vuong and Clarke test criteria. Next, we conduct a series of robustness checks, including reduced models and alternative specifications, as well as an alternative theoretical account where the initial threat level conditions whether cases are probabilistically treated as "at-risk." The models presented in the main text outperform, and are robust to, each of these alternative specifications. Finally, we present Stata code for the split-sample logit, and report a table of all alliance violations.

1 Construction of the Instrumental Variable, *Territorial Threat*

Table A.1 reports the estimates of the variables used to construct the primary independent variable, *territorial threat*. Data and model specification are from Gibler and Tir (2014, Table 1). Our model differs from Gibler and Tir (2014), however, in that while they use the period from 1816–1999, we use the period 1900–2001. We make this change because the factors that affect conflict in the nineteenth century may not be the same as those in the twentieth (Bennett and Stam 2004). All results are consistent with Gibler and Tir's (2014) findings, with the exception of *civil war in either state*, which is significant at the p <0.1-level in our replication, compared to the p <0.05-level in the original analysis.

Table A.1: Predicting Fatal MIDs in Contiguous Dyads, 1900–2001.

	β	S.E.
Same Colonial Master	0.235*	(0.113)
Peaceful Territorial Transfer in Dyad	-0.568*	(0.184)
Violent Territorial Transfer in Dyad	0.514*	(0.116)
Defense Pact with All Neighbors	-0.908*	(0.213)
Civil War in Either State	0.169	(0.102)
Highest Militarization Level Among Neighbors	14.023*	(1.619)
Previous Territorial MID Against Either State	0.424*	(0.091)
Border Age (logged)	0.181*	(0.036)
Peace Years	-0.434*	(0.024)
Peace Years (Squared)	0.012*	(0.001)
Peace Years (Cubed)	-0.000*	(0.000)
Constant	-2.753*	(0.134)
Log-likelihood	-1877.638	
Observations	15058	

Note: *p <0.05, two-tailed. Replication of Gibler and Tir (2014, Table 1) for 1900–2001.

2 Model Fit Comparisons

We demonstrate that inclusion of the *relevance* equation improves our model fit and the quality of our estimates. To do this, we compare the model fit of models 2 and 3 from Table 1, using Vuong's (1989) and Clarke's (2003, 2007) tests for non-nested models. We use the Vuong and Clarke tests, rather than an F-test or likelihood ratio test because models 2 and 3 are non-nested due to their differing functional forms: model 2 assumes an additive non-linear logit function while model 3 is a mixture of two logistic distributions (for a discussion on differing types of non-nested models, see Clarke 2001).[1] We also compare models 4 and 5 from Table 1, which provided analogous estimates during the 1950–2001 period, as a robustness check.

The Vuong test compares the mean log-likelihood ratios of two models. If the first model is closer to the true specification, then the mean log-likelihood ratio is positive and statistically significant. As is common practice, we apply the Schwarz's correction to the Vuong test. The correction penalizes for the inclusion of additional parameters in a model. That is, the models that include the *relevance* equation are penalized because they estimate more parameters than the reduced model. More formally, the corrected Vuong test is:

$$LR_n\left(\tilde{\theta}_n, \tilde{\gamma}_n\right) - \left[\left(\frac{p}{2}\right)\ln n - \left(\frac{q}{2}\right)\ln n\right] \tag{1}$$

where LR is the log-likelihood ratio, $\tilde{\theta}$ and $\tilde{\gamma}$ are the model estimates, and p and q are the number of estimated parameters for model f and g, which are the two models being compared (Vuong 1989).

Clarke's distribution-free test, meanwhile, tests whether the median logged ratio of the likelihood for the individual observations of two models are equal. If the first model is closer to the true specification, more than half of the individual logged ratios of the likelihoods will

[1]Neither AIC nor BIC are appropriate as they do not include information from the rival theory, nor do they permit probabilistic statements regarding model selection (Clarke 2003).

be greater than zero. More formally:

$$H_0 : Pr_0 \left[\ln \frac{f\left(Y_i|X_i;\beta_*\right)}{g\left(Y_i|Z_i;\gamma_*\right)} > 0 \right] = 0.5 \tag{2}$$

where the numerator is estimated model f, which predicts Y_i from a set of covariates, X_i, and estimated parameters, β_*; the denominator is estimated model g, which predicts Y_i from a set of covariates, Z_i, and estimated parameters, γ_*. The null hypothesis is that the median logged ratio of the likelihoods between the two models is equal to 0, i.e. the probability that the median logged ratio of the likelihoods of f is greater than g is 0.5. If d_i is set equal to $\ln f\left(Y_i|X_i;\beta_*\right) - \ln g\left(Y_i|Z_i;\gamma_*\right)$, the test statistic is:

$$B = \sum_{i=1}^{n} I_{(0,+\infty)}\left(d_i\right) \tag{3}$$

where I is a dichotomous indicator equal to 1 if $n_i > 0$ in Equation 2, and 0 if $n_i \leq 0$. Equation 3 is the sum of positive differences and is distributed according to a Binomial distribution with n trials and a mean equal to 0.5. We apply the average Schwarz correction to Clarke's distribution-free test, adjusting the individual log-likelihoods for model f by a factor $[(p/2n)\ln n]$ and those of model g by a factor $[(q/2n)\ln n]$ (see Clarke 2007, 350).

Table A.2 reports the results of our non-nested model comparisons. The test statistic for the Vuong test is 40.02 for the full model (Table 1, Model 3) compared to the reduced model (Table 1 Model 2). This returns a p-value of <0.001, allowing us to reject the null that the models are equivalent.

Using Clarke's test, we find that the split-population model returns a positive log-likelihood ratio for 4468 of the 6395 observations, which generates a p-value of <0.001. We are thus able to reject the null that the models are equal and again find empirical support for the full models.

We find similar support looking in the 1950–2001 sample. The full split-population model (Table 1, Model 5) again outperforms the the reduced model (Table 1, Model 4).

Table A.2: Comparison of Model Fit.

	Full	post-1945
Vuong Test		
Vuong	71.86	58.85
SE	1.80	3.23
t-statistic	40.02	18.23
p-value	$< .001$	$< .001$
Clarke Test		
$\sum_{\mathrm{i}}^{\mathrm{n}}(\mathrm{ll}_{\mathrm{M}_{\mathrm{Full}},i} - \mathrm{ll}_{\mathrm{M}_{\mathrm{LMV}},i} > 0)$	4468	3559
$\sum_{\mathrm{i}}^{\mathrm{n}}(\mathrm{ll}_{\mathrm{M}_{\mathrm{Full}},i} - \mathrm{ll}_{\mathrm{M}_{\mathrm{LMV}},i} < 0)$	1927	1393
Positive, one-side test (p-value)	$< .001$	$< .001$

Note: Clarke distribution-free test uses binomial distribution ($p = .5$).

In sum, the results indicate that the two split-population models outperform the models which assume all states are initially equally "at-risk" of violating an alliance.

3 Robustness Checks

The following reports several robustness checks, divided into two parts. First, we report several reduced models and models with alternative specifications of some variables, to demonstrate that our variable choice and specification do not drive our main results. Second, we estimate a model based on an alternative theoretical account of our argument, where the initial threat level conditions whether cases are probabilistically treated as "at-risk." The results of these various specifications are consistent with those from our main model.

Reduced Models

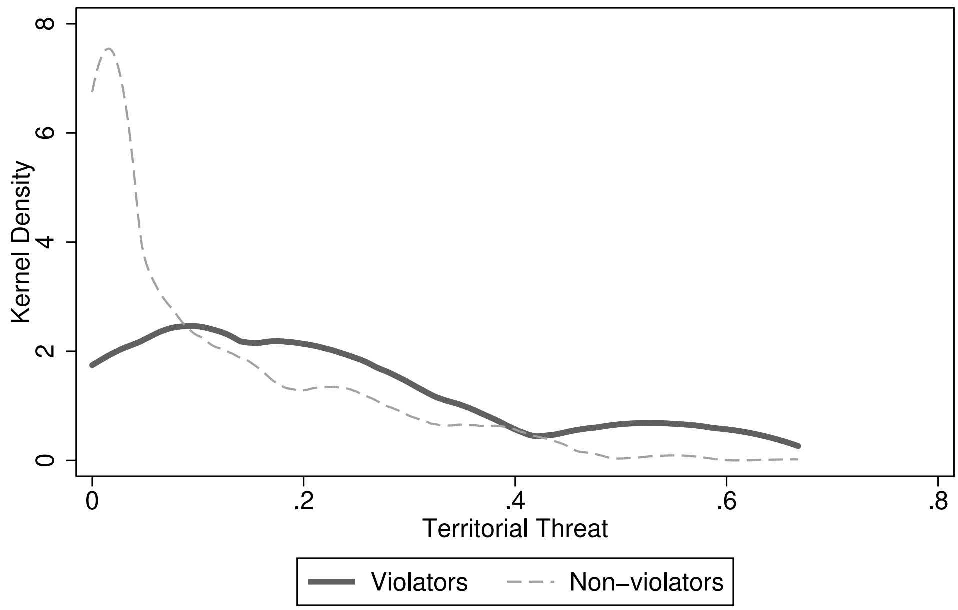
We begin our robustness checks by visualizing the difference in *territorial threat* between states that violate an alliance and the full sample (from Table 1). The mean territorial threat for alliance violators is 0.215 with a standard deviation of 0.193 and $N = 74$, while the mean for non-violators is 0.108 with a standard deviation of 0.127 and $N = 6768$. A difference of means between the two samples is statistically significant with $p < 0.001$. As Figure A.1 demonstrates, a large swath of states at low levels of territorial threat comprise a large proportion of the non-violators.

Next, we report several additional models in Tables A.3 and A.4. These are various reduced models that examine the sensitivity of the original model when controlling for the level of external threat. We also run a model that uses *GDP per capita* in place of *energy consumption per capita* in the 1950–2001 period.[2] Note in the relevance equation that *Territorial threat* is stable across all models and is consistently able to differentiate among alliance types—i.e. territorial threat is more likely to lead to alliance violations.

The outcome equation, which is the original model conditioned by relevance, demonstrates that the two primary variables of interest—change in leader coalition and democracy—are not consistent predictors of alliance violation. Democracy is only statistically significant in one of the models, and it has a positive coefficient, meaning that democracies are more

[2] *GDP per capita* data are only available from 1950 onward.

Figure A.1: Comparison of Kernel Density of Alliance Violators and Sample Average.



Note: The mean territorial threat for alliance violators is 0.215 with a standard deviation of 0.193, while the mean for non−violators sample is 0.108 with a standard deviation of 0.127. A difference of means between the two samples statistically significant at p < 0.001.

likely to violate their alliances. These results, of course, strongly contradict the argument that democracies are less likely to abrogate their treaties. The generally negligible effects (with the only significant result running in the wrong direction) only emphasizes the conclusion that democracy is not an accurate predictor of alliance violators once the territorial threat environment is considered.

We report results in the manuscript that support the idea that leader change matters for alliance violation, at least among states in the 1950–2001 sample, but our additional models suggest this result may be dependent upon model specification. Once we consider the threat environment affecting the alliance member, the effect of leader change disappears. The original model suggests leader change is statistically insignificant, with an effect size that is quite large, but our re-analyses imply statistical significance at the traditional $p < 0.05$-level in less than half of the conditioned models in Tables A.3 and A.4.

Table A.3: Political Environment, Democracy, and Alliance Violations, Robustness Checks.

Model	Simple	Democratic Development	Full Development	Change in Alliance	Alliance Terms
Outcome Equation					
Change in Leader's Societal Coalition	1.149*	12.506*	13.555*	12.482	15.839*
	(0.475)	(6.005)	(2.552)	(23.077)	(7.443)
Democracy	-0.508	0.566	0.568	1.491	2.311*
	(0.458)	(0.830)	(0.854)	(1.226)	(1.133)
Change in International Power				1.980*	
				(0.725)	
Change in Political Institutions				0.252	
				(0.637)	
Change in External Threat				0.903	
				(0.847)	
Formation of New Outside Alliance				1.578*	
				(0.693)	
Asymmetry					-2.204*
					(0.735)
Non-military Cooperation					-2.204*
					(0.735)
Ratification					2.676
					(1.456)
Military Cooperation					18.621*
					(2.152)
Time	0.028	0.051	0.114	-0.119	0.108
	(0.073)	(0.109)	(0.116)	(0.140)	(0.154)
Time Squared	-0.005	-0.006	-0.009	0.001	-0.010
	(0.005)	(0.006)	(0.007)	(0.007)	(0.009)
Time Cubed	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-1.700	-0.392	-0.703	-2.078*	-1.224
	(0.981)	(0.752)	(0.699)	(0.672)	(1.209)
Relevance Equation					
Territorial Threat	4.686*	4.312*	3.849*	3.664*	4.509*
	(0.791)	(1.025)	(1.045)	(1.014)	(0.969)
Number of Borders		0.083	0.046	0.067	0.124*
		(0.066)	(0.069)	(0.067)	(0.056)
Proportion of Democratic Borders		-0.347*	-0.303	-0.334*	-0.281*
		(0.153)	(0.157)	(0.148)	(0.112)
Major Power			0.574	0.345	0.756*
			(0.381)	(0.387)	(0.374)
Rivalry			0.248	0.171	0.110
			(0.372)	(0.386)	(0.385)
Cold War			-0.048	0.178	0.466
			(0.263)	(0.275)	(0.287)
Economic Development			-0.230*	-0.216*	-0.189*
			(0.078)	(0.075)	(0.075)
Oil Producer			0.374	0.422	0.887*
			(0.362)	(0.376)	(0.372)
Constant	-3.097*	-4.345*	-4.509*	-4.447*	-5.454*
	(0.925)	(0.385)	(0.464)	(0.522)	(0.444)
Log-likelihood	-385.975	-377.079	-364.525	-350.253	-321.929
Observations (Alliances)	6842 (234)	6811 (234)	6618 (234)	6543 (231)	6470 (226)

Note: *p <0.05, two-tailed. Standard errors in parentheses. Point estimates and standard errors were calculated from 10 draws using Rubin's (1987) formula for multiple imputation to account for uncertainty in the *territorial threat* instrumental variable.

Table A.4: Political Environment, Democracy, and Alliance Violations, Additional Robustness Checks.

Model	Democratic Development	Democracy Directly	Exclude Democracy	Alt. Econ Dev. Measure
Outcome Equation				
Change in Leader's Societal Coalition	4.443	7.290	5.173	7.535
	(2.821)	(6.123)	(2.774)	(6.987)
Democracy	0.756	2.497	0.542	0.808
	(1.607)	(2.047)	(1.073)	(2.044)
Change in International Power	3.794	2.740*	3.256*	2.281*
	(5.669)	(1.146)	(1.570)	(0.835)
Change in Political Institutions	1.861	1.609	1.598	2.478
	(2.434)	(1.002)	(1.236)	(2.058)
Change in External Threat	1.073	0.896	0.607	-0.637
	(2.608)	(1.087)	(1.128)	(1.488)
Formation of New Outside Alliance	3.069	2.330*	2.491*	2.997*
	(4.513)	(0.833)	(1.174)	(1.311)
Asymmetry	-1.287	-1.376	-1.866*	-1.617*
	(1.034)	(0.803)	(0.937)	(0.756)
Non-military Cooperation	-2.790*	-2.798*	-3.372*	-2.347
	(1.112)	(1.239)	(1.085)	(2.940)
Ratification	0.851	2.358	1.662	0.063
	(3.319)	(1.682)	(1.944)	(2.517)
Military Cooperation	5.010	3.479*	4.853*	6.792*
	(3.556)	(1.104)	(1.524)	(2.462)
Time	-0.174	-0.197	-0.105	-0.085
	(0.435)	(0.253)	(0.267)	(0.390)
Time Squared	-0.002	0.001	-0.005	-0.001
	(0.011)	(0.011)	(0.011)	(0.015)
Time Cubed	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-3.199	-3.722*	-2.616	-3.082
	(1.938)	(1.869)	(1.371)	(1.979)
Relevance Equation				
Territorial Threat	3.946*	4.292*	4.973*	6.045*
	(1.091)	(0.986)	(1.057)	(2.057)
Number of Borders	0.145		0.044	-0.005
	(0.100)		(0.050)	(0.137)
Proportion of Democratic Borders	-0.329*			0.010
	(0.121)			(0.252)
Democracy		-1.129*		
		(0.561)		
Major Power		0.512	0.215	-0.010
		(0.413)	(0.409)	(0.842)
Rivalry		0.030	0.036	-0.492
		(0.376)	(0.374)	(0.493)
Cold War	0.416	0.449	0.473	1.892
	(0.520)	(0.271)	(0.290)	(1.092)
Economic Development		-0.125	-1.598*	-0.528*
		(0.090)	(0.075)	(0.202)
Oil Producer		0.485	0.642	1.598*
		(0.415)	(0.477)	(0.553)
Constant	-4.773*	-4.376*	-5.051*	-2.441
	(0.617)	(0.512)	(0.468)	(1.804)
Log-likelihood	-316.297	-314.076	-311.835	-151.948
Observations (Alliances)	6582 (223)	6424 (223)	6395 (223)	4896 (137)

Note: $*p < 0.05$, two-tailed. Standard errors in parentheses. Point estimates and standard errors were calculated from 10 draws using Rubin's (1987) formula for multiple imputation to account for uncertainty in the *territorial threat* instrumental variable.

Alternative Models

We explore an alternative theoretical and empirical specification of our theory here. As a reminder, our basic argument is that states form alliances to (a) counter threats or (b) perform other functions. In addition, alliances that were formed as an a-type can transform into a b-type once the threat has subsided. This argument is especially important when looking at the relationship between democracy and alliance violations; after states form an a-type alliance that successfully deterred a threat, their now peaceful environment encourages democratization and their alliance can either be re-purposed into a b-type alliance or allowed to expire. In terms of alliance violations, only a-type alliance are "at-risk", as b-type alliances do not face external threats and are unlikely to be invoked.

While our theory and empirical specification suggests that regardless of original intent, all alliances are likely to transform into b-types in the absence of current threats, it is possible that the threat level at the time of alliance formation is the driving factor in subsequent behavior. As alliances are written documents with specified obligations, the latter argument would indicate that the initial threat environment is "baked in" to the bilateral document. This argument, then, suggests that we should examine the threat level at formation to probabilistically estimate which alliances are "at-risk" in the *relevance equation*y. Conversely, our argument, suggests that the alliance's purpose is more flexible, and its purpose (whether a- or b-type) alliances are often *re-purposed* once the political environment has become peaceful. Thus, the time-varying measure of threat that we use best captures our theoretical argument.

Table A.5 reports two models—model 1 looks at just the threat level at the time of alliance formation, and model 2 includes both the initial threat level at alliance formation and the time-varying threat level. Model 1 allows us to evaluate whether the threat conditions present when an alliance is formed drives future behavior, while model 2 controls for this initial threat level while allowing for the theorized re-purposing of alliances in peaceful environments.

Starting with Model 1, and focusing on the *relevance equation*, it is clear that *threat* *at Formation* is not statistically significant, suggesting that the level of threat at alliance formation is not a good predictor of whether an alliance is "at-risk" of violating their alliance. Turning to Model 2, we see that *territorial threat* remains positive and statistically significant even after accounting for the initial threat level at the time of alliance formation. Consistent with our argument, this result indicates that alliances initially created to aggregate power and deter a threat (a-types) are later re-purposed (made into (b-types)) once the political environment becomes more peaceful. This result is consistent with outcomes identified by previous research, such as the result that democratization takes place *after* alliances are formed (e.g., Gibler and Wolford 2006). In other words, it is not that the threat level at formation alone which determines how an alliance will be used throughout its existence, but the level of threat currently facing the alliance.

It is also worth emphasizing that these results do not affect our primary substantive take-aways in any way. Neither *democracy* nor *change in Leader's Societal Coalition* are statistically significant. Using Rainey's negligible effect test, we continue to find that political institutions exert no meaningful effect on whether an alliance is violated, once the political environment is accounted for. The current political environment appears to primary factor of whether a state is "at-risk" of violating an alliance, while political institutions have little if any direct effect on their own.

Table A.5: Political Environment, Democracy, and Alliance Violations.

Time	Formation 1920-2001	Formation and Over Time 1920-2001
Outcome Equation		
Change in Leader's Societal Coalition	5.039	5.327
	(12.735)	(5.829)
Democracy	1.034	1.305
	(4.914)	(2.483)
Change in International Power	6.801	3.105
	(14.272)	(4.243)
Change in Political Institutions	1.839	1.796
	(1.762)	(1.983)
Change in External Threat	1.946	0.882
	(3.267)	(1.916)
Formation of New Outside Alliance	6.071	2.569
	(14.251)	(3.230)
Asymmetry	-0.984	-1.575
	(1.349)	(1.091)
Non-military Cooperation	-2.364	-2.584*
	(1.445)	(1.271)
Ratification	-0.342	1.371
	(2.531)	(3.175)
Military Cooperation	6.824	5.108
	(8.592)	(2.680)
Time	-0.172	-0.136
	(0.408)	(0.370)
Time Squared	-0.005	-0.002
	(0.022)	(0.011)
Time Cubed	0.000	0.000
	(0.000)	(0.000)
Constant	-5.275	-3.242*
	(17.622)	(1.444)
Relevance (At-risk) Equation		
Territorial Threat		4.452*
		(1.308)
Threat at Formation	0.085	-1.166
	(1.370)	(1.464)
Proportion of Democratic Borders	-0.388*	-0.292*
	(0.130)	(0.119)
Number of Borders	0.187*	0.139
	(0.062)	(0.077)
Major Power	0.413	0.398
	(0.408)	(0.616)
Rivalry	0.294	0.045
	(0.434)	(0.412)
Cold War	0.125	0.468
	(0.263)	(0.363)
Economic Development	-0.175	-0.159
	(0.093)	(0.088)
Oil Producer	0.336	0.620
	(0.418)	(0.653)
Constant	-4.701*	-4.997*
	(0.443)	(0.674)
Log-likelihood	-315.373	-307.144
Observations (Alliances)	6395 (223)	6395 (223)

Note: *p <0.05, two-tailed. Standard errors in parentheses. Point estimates and standard errors in models 3 and 5 were calculated from 10 draws using Rubin's (1987) formula for multiple imputation to account for uncertainty in the *territorial threat* and *Threat at Formation* instrumental variables.

4 *Stata* Code to Implement the Split-population Logit

We include *Stata* code of the program we wrote to estimate the split-population logit. For shorthand, *DV* represents the binary outcome variable, *IVO* represents the regressors in the *outcome* equation, and *IVR* represents the regressors in the *relevance* equation. We outline what each line of code does below.

Stata code:

```
program define spl_lf, rclass
    args lnf beta gamma
    tempvar rel violate
        quietly gen double ‘rel’ = 1/(1+exp(-‘gamma’))
        quietly gen double ‘violate’ = 1/(1+exp(-‘beta’))
        quietly replace ‘lnf’ = ‘lnf’ = ln((1-‘rel’)+(‘rel’*(1-‘violate’))) if $ML_y1==0
        quietly replace ‘lnf’ = ‘lnf’ = ln((‘rel’)*(‘violate’)) if $ML_y1==1
end
ml model lf spl_lf (DV = IVO) ( = IVR)
ml maximize
```

The first line defines that we are creating a program; the second and third line specifies the arguments (coefficients to be specified), while the fourth and fifth lines creates two temporary variables (i.e. the two equations). Next, the sixth and seventh lines specify the likelihoods to be summed for $Y = 0$ and $Y = 1$, while line eight ends the program. Note that line six, specifying the likelihood when $Y = 0$, treats the outcomes as coming from two distinct processes, i.e. a mixture model, as $Y = 0$ can occur because either (a) the observation is not relevant (“1-‘rel”’) or (b) the observation is relevant but there is no violation (“ ‘rel’*(1-‘violate’)”). $Y = 1$ occurs only if the observation is relevant and a violation occurred.

Lines nine and ten implement the program and maximize the likelihood. The variables are specified in line nine, with the equation from the first set of parentheses providing the independent variables (on the right hand side of the equal sign) for the ‘beta’ argument and the second set of parentheses providing the independent variables for the ‘gamma’ equation. Any desired options, such as estimating clustered standard errors or choosing an alternative maximization algorithm, can be specified after the parentheses. Finally, the likelihood is maximized in line ten.[3]

[3] As with other mixture models, it may be helpful to specify initial conditions to help identify the global maximum.

5 Alliance Violators

Table A.6 provides information about the cases of alliance violations. Alliance violation data are from Leeds, Mattes and Vogel (2009). In addition to the State A (the violator), State B (the state with whom the agreement is violated), and the year, we also report three other pieces of information: State A's level of *territorial threat*, whether there was a *change in leader's societal coalition*, and whether it is a *democracy*. *Territorial threat* is the mean predicted probability from 10 draws of the estimated distribution of the maximum predicted territorial threat from the model reported in Table A.1. *Change in leader's societal coalition* and *democracy* are from Leeds, Mattes and Vogel (2009).

Table A.6: List of Abrogated Alliances.

State A	State B	Year	Territorial Threat	Δ in Leader's Societal Coalition	Democracy
Germany	Russia	1933	.0134181	1	0
France	Italy	1935	.0194196	1	1
Turkey	Italy	1935	.0438603	0	0
Greece	Italy	1935	.0658665	0	1
Belgium	France	1936	.0074095	0	1
Italy	Spain	1936	.0549720	0	0
Russia	Czechoslovakia	1938	.2272077	0	0
France	Czechoslovakia	1938	.1936811	0	1
Germany	Austria	1938	.1865337	0	0
Russia	Poland	1939	.279217	0	0
France	Germany	1939	.2953013	0	1
Russia	Finland	1939	.279217	0	0
Italy	Albania	1939	.1462826	0	0
Russia	France	1939	.279217	0	0
Russia	Lithuania	1940	.5524079	0	0
Yugoslavia	Romania	1940	.2755803	0	0
Russia	Estonia	1940	.5524079	0	0
Germany	Denmark	1940	.6677183	0	0
Thailand	United Kingdom	1940	.0000000	0	0
Russia	Latvia	1940	.5524079	0	0
Italy	Russia	1941	.5609003	0	0
Germany	Russia	1941	.5609003	0	0
Russia	Iran	1941	.5609003	0	0
Italy	Germany	1943	.259393	0	0
Russia	Japan	1945	.5433966	0	0
Russia	Turkey	1945	.5433966	0	0
Russia	Yugoslavia	1949	.4135729	0	0
Albania	Yugoslavia	1949	.0933776	0	0
Hungary	Yugoslavia	1949	.0779969	0	0
Bulgaria	Yugoslavia	1949	.0941740	0	0
Poland	Yugoslavia	1949	.015158	0	0
Czechoslovakia	Yugoslavia	1949	.0170141	0	0
Romania	Yugoslavia	1949	.0794406	0	0
Pakistan	Turkey	1950	.1850240	0	0
Egypt	United Kingdom	1951	.2974145	0	0
Russia	France	1955	.2527492	0	0
Russia	United Kingdom	1955	.2527492	0	0
Egypt	United Kingdom	1956	.3046337	0	0
Jordan	United Kingdom	1957	.2971661	0	0
Iraq	Jordan	1958	.176082	1	0
Iraq	United Kingdom	1959	.1818257	0	0
Mali	France	1960	.0829011	0	0
Egypt	Yemen Arab Republic	1961	.2875846	0	0
Saudi Arabia	Yemen Arab Republic	1962	.1485351	0	0
Nigeria	United Kingdom	1962	.0385331	0	1
Saudi Arabia	Egypt	1962	.1284466	0	0
France	Morocco	1966	.0029804	0	1
Egypt	Yemen Arab Republic	1967	.3355091	0	0
Libya	United Kingdom	1970	.0069660	0	0
Madagascar	France	1973	.0000000	1	0
Tunisia	Libya	1974	.0015907	0	0
United Kingdom	South Africa	1975	.0149923	0	1
Egypt	Russia	1976	.1617086	0	0
Iraq	Egypt	1977	.1838835	0	0
Somalia	Russia	1977	.1971040	0	0
Uganda	Sudan	1979	.1853800	1	0
Russia	Pakistan	1979	.4478039	0	0
Iran	United States	1979	.3403858	1	0
United States	Taiwan	1980	.0682471	0	1
Syria	Libya	1980	.1067835	0	0
Niger	Libya	1981	.0030048	0	0
Chad	Libya	1982	.1290733	1	0
Algeria	Libya	1984	.0460337	0	0
Sudan	Egypt	1985	.1514078	0	0
Morocco	Libya	1986	.0980619	0	0
Malta	Russia	1987	.0000000	1	1
Senegal	Gambia	1989	.0008766	0	0
Jordan	Saudi Arabia	1990	.1075147	0	0
Russia	Iraq	1990	.2336497	0	0
Poland	Russia	1991	.1297963	1	1

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