Taking Democratic Differences Seriously: A Split-Population Approach*

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Abstract

Many have confirmed that the relationship between democracy and conflict is at least partially endogenous. Nevertheless, studies continue to group democracy with other variables as predictors of conflict using additive models. This is inappropriate both theoretically and methodologically, and we demonstrate this with an example from the alliance literature. We use a split-population logistic model to show that democracies in alliances are no more reliable than other regime types, once the state-development process is also modeled. Further, we find that the alliances formed during times of conflict are particularly unreliable "scraps of paper", and the general reliability of alliances is concentrated in those coordination alliances existing in already peaceful environments.

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Since the establishment of the empirical finding that democracies rarely fight each other, 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.

Nevertheless, we believe that these types of inferences may be incorrect. If there is substantial endogeneity between peace and democracy, as many believe is the case (see, for example, Russett and Oneal 2001), then the additive models that link democracies to behaviors that are different from other states will be biased as democracies face different political environments than other types of governments. Therefore, in order to understand any possible second-order effects of democracy, the peaceful or conflictual environment facing the state needs to be understood as well.

We focus here on the large body of research that suggests democracies are more likely to cooperate with each other in alliances and be more reliable alliance partners. Alliances are clearly an example of a behavior that is most likely observed when states are threatened, and the empirical work on alliances has ably demonstrated that there are substantial differences between democracies and other states. This literature, therefore, provides a fertile path for understanding how our argument can shape existing interpretations of democratic behavior.

We examine the bias we expect in this literature using a split-population logit analysis that reinterprets an existing study of alliance abrogation. We find strong support for our argument and observe that the association between democracy and reliable alliances is a product of the conflict environment facing the state. Modeling the selection of alliance types into two qualitatively distinct populations with predictors of salient threats to the state, we demonstrate that democracies are less likely than other states to be involved in the sample of often-violated alliances that are correlated with conflict. This selection effect leads to the spurious finding associating democracy with reliability in the high threat environments where alliances are most likely to be invoked. Our approach reveals that, under some conditions, there is support for the traditional alliance theory assumption that alliances may be 'scraps of paper' that form and dissolve with the rise of threats to the state.

We outline the alliance literature's growing focus on democracy in the next section and describe how multiple studies have come to the conclusions that democracies cooperate better than other regime types. We then discuss the role of endogeneity as it applies to these studies and show how this endogeneity can reinterpret the bulk of the alliance research on democratic states. The final sections present multiple empirical tests of our argument.

Democracies in Alliance

Countless studies have argued for and found a relationship between external threat and alliance making. Few, however, have connected those findings to the types of alliances formed. Instead, the focus of the alliance literature remains derivative of democratic peace scholarship: democracies in alliances behave differently than other regime types. However, if external threat is an important determinant of regime type—a common argument in peace studies—then this is problematic for inferences on the independent effects of democracy.

Establishing democratic differences with alliance formation

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 internal capability changes, 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, mostly realist theory (see also, Waltz 1979; Walt 1985), and most tests of alliance formation find that threats do matter in determining whether alliances form (Siverson and Emmons 1991; Lai and Reiter 2000). Of course, 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.

Nevertheless, not all alliances are responses to threat. Instead, alliances may be formed to neutralize potential intervenors prior to predation by major states (Schroeder 1976), and alliances may be an instrument that is used to resolve contentious issues (Gibler 1996). Or, more broadly, alliances may be used as methods of gaining control over smaller states, as major powers give some of their security to smaller states so that can have basing rights, changed foreign policy interests, geopolitical access, or other goods (Morrow 1991; Lake 2009). 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. The latter forces an assumption that some trade-off between capabilities and autonomy is the dominant reason for understanding why major states would partner with minor states that offer little fighting help.

Related to these two general alliance types has been the growth of studies associating democracies with alliance behavior that is quite different from traditional expectations of state behavior. For example, democracies may engage in deterrent alliances, but their commitments are seemingly not scraps of paper. Their alliances commitments are more likely to deter other states and, when threatened, to be honored by the democracies involved. Democracies also tend to engage in non-traditional alliances of control, and their alliances are more likely to become institutionalized and have provisions for cooperation that are different from other alliances.

The observation of a democratic difference in alliance behavior began with the early empirical work that examined whether democracies are more likely 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 during the 1920 to 1939 period. Lai and Reiter (2000) revisited this empirical claim and found a strong relationship with multivariate controls for 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.

Gibler and Wolford (2006) argued that these studies were not technically examining alliance formation but were instead identifying whether dyads were allied. Switching between alliance formation and the presence of an alliance in the dyad yielded bey different results.

Democracies were not more likely to form alliances, but states were becoming democratic *after* having formed an alliance. They argued that defense pacts reduced the threats to the state, and these helped foster demilitarization, decentralization, and eventual democratization. An analysis of the cases that controlled the democracy relationship provided further evidence for this argument since over 90% of jointly democratic alliance dyads were in three broad, regional defense pacts: NATO at 55%, OAS at 29%, and the WEU at 7%. In short, democracy was a consequence, not a cause, of alliance politics.

This series of studies reveals three important pieces of information for our argument. First, it confirms that, indeed, there is something different about democracies when involved in alliances. Second, though, the temporal sequencing of alliance, and then democracy, provides good preliminary evidence that democratic differences may actually be a product of the conflict environment affecting the states at the time of alliance formation. Third, and finally, the observation that joint-democracy-in-alliance arguments rests on just three alliance cases suggests that democracies may not formally cooperate with each other more readily than other types of states, at least in security matters.

Theories of democratic cooperation and alliances

Theories of democratic cooperation have not done well when trying to understand dyadic versus monadic differences in regime-type-based cooperation. This is somewhat strange considering that the dyadic nature of the democracy-peace relationship is well developed in both theory and tests. According to most democratic peace scholarship, for example, there should be an empirical difference in conflict likelihood between fully democratic dyads and other dyads in each sample. Consistent with this expectation, mixed dyads with one democracy and a non-democracy have no pacifying effect (Chan 1984; Pickering 2002; Quackenbush and Rudy 2009).[1]

Likewise, cooperation is expected to only be likely when two democracies interact. For example, the best argument for regime-based cooperation can be found in Leeds (1999) and follows similar dyad-focused explanations. In this model 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. Two variables are important in the model: accountability and flexibility. Accountability references the audience costs that democratic leaders suffer from unilateral defection. Meanwhile, flexibility is synonymous with the high level of domestic autonomy afforded non-democratic leaders. Cooperation is most likely in dyads with similar regime types, especially jointly democratic dyads according to Leeds' model: "both jointly democratic and autocratic dyads will form more agreements than dyads composed of one democracy and one autocracy" (1999, 990-991). Empirical tests using the Conflict and Peace Data Bank (COPDAB) data set (Azar 1980) confirm the expectations of the model.

Again, though, this does not match well what we know to be empirically true using existing alliance datasets (see Gibler and Sarkees 2004; Leeds et al. 2002). Though cooperation

[1]Most international conflict models use either a weak-link specification or a joint democracy dummy variable (see the discussion by Goertz 2006, Chapter 5).

may be more readily made by jointly democratic dyads, this does not translate into a greater likelihood of alliance formation. This puzzle could be explained by the higher cost associated with forming alliances, but no theory ties together why high-cost alliances are formed more readily by democratic states with non-democracies, especially given their expected higher degree of defection.

Alliance duration, abrogation, reliability, and other relationships

Dyadic theories of cooperation perform better when trying to understand alliance duration. For example, both Gaubatz (1996) and Bennett (1997) found that joint democracy predicted longer lasting alliances. Gaubatz (1996) dichotomized dyadic democracy, using data from Doyle (1986), and found that jointly democratic alliance dyads lasted twice as long as other alliance dyads. There was no empirical difference between mixed-regime and non-democratic dyads, however. This led Gaubatz to conclude, "democracy by itself does not appear to either increase or decrease the ability of a state to make commitments to nondemocracies" (1996, 135). Bennett (1997) confirmed this pattern. Again using Doyle's data, Bennett averaged the number of liberal regimes in the alliance and found a positive, statistically significant effect for increased liberalness in the alliance. The substantive effects were especially strong, since all-liberal alliances increase the duration of an average alliance by almost fifteen years. Of course, only a handful of alliances (less than 1% of the data) were comprised solely of democratic states at the time of alliance formation. If we also consider that the vast majority of jointly democratic, allied dyads are in just three alliances, then the substantive effects of democracy on alliance duration is quite small.

The move away from dyadic theories of cooperation began with the examination of alliance reliability. Here, the argument again relies on the accountability constraints of democratic leaders but makes no assumption regarding other actors in the alliance or outside. Instead, 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. For example, Leeds (2003, 813) argues:

> Because democratic leaders are accountable for their actions to a large segment of the population and can be replaced at regular intervals at fairly low cost, they tend to be particularly concerned with providing good public policy and proving their competence to their constituents... As a result, their commitments to fulfill their promises may be more credible, but they may also be very wary of making deals that will be unsuccessful, because changing policy to account for new realities may be challenging.

This approach has been used extensively, and several related studies find that democracies are less likely to terminate, violate, or abrogate their alliances (Leeds and Savun 2007; Leeds, Mattes and Vogel 2009). These studies seemingly confirm the arguments that democratic accountability leads to more credible commitments. Of course, that conclusion assumes that the conflict environment facing democratic alliance partners does not vary substantially from the conflict environments of other types of allies.

Peace and Democracy; Democracy and Peace

There are reasons to believe that democracies do not face the same threat environments as other states; rather, they tend to exist in more peaceful environments. Moreover, peace and democracy are frequently theorized to be at least partially endogenous. Russett and Oneal (2001, 37), for example, contend that "Democracy is easier to sustain in a peaceful environment," and "external threats become reasons or justifications for suspending normal civili liberties, elections, and constitutional government." They also note the long tradition of democracies worrying about Lasswell's (1941) "Garrison State" taking over and inhibiting civil and economic rights and protections (see Russett and Oneal 2001, 191). Further, their model of a Kantian peace recognizes the endogenous "feedback loops" from peace to democracy, trade, and international organization. Thus, there is an explicit recognition that peace at least partially causes democracy in their theory, even though they never develop the extent of that endogeneity.

Others have gone further. Thompson (1996) argued that leaders who give up the pursuit of regional hegemony contribute to both peace and democracy. The "cart before the horse" was the contention that peace was a necessary antecedent for democracy to take hold among the states in a region. Follow up analyses confirmed this systematically as Rasler and Thompson (2004) found a reciprocal relationship between domestic power concentration, non-democracy, and peace among major powers since 1816. However, the relationship was not strong enough to mute the pacifying effects of joint democracy among major states. This differs from the work of James, Solberg and Wolfson (1999), who found that simultaneously modeling peace–to–democracy and democracy–to–peace made the latter statistically insignificant, suggesting that peace is is more likely to lead to democracy than its converse.

Our position is similar to both sets of these arguments, but it also argues for an even higher level of endogeneity between peace and democracy. We posit that external threats to the state effectively control whether states democratize in the first place. As developed in Gibler (2012), states centralize in response to threats to their territorial homelands. The population becomes more nationalistic (Gibler, Hutchison and Miller 2012) and less tolerant of dissent (Hutchison and Gibler 2007). Institutions centralize as well. Opposition leaders are muted by the difficult political environment created by threat since deviation from regime policies are likely to be viewed as traitorous by the population. Meanwhile, ruling elites are doubly advantaged by both national sentiment and the rise of the military within the state, and domestic political bargaining becomes much easier, making checks on executive power difficult to maintain as the regime defends against the state's rival (Gibler 2010). These causal mechanisms have been confirmed in multiple ways, and the general relationship between territorial peace and democracy has held using mutliple research designs (Gibler and Braithwaite 2013; Gibler and Hutchison 2013; Gibler and Tir 2010, 2014).

Together, these behavioral and institutional changes render a correlation between external territorial threats and non-democratic, often authoritarian states across international borders. Or, put differently, external threats split the population of regime types between decentralized democracies within peaceful regions and centralized, non-democratic states in conflict-prone environments.

Democracy-peace endogeneity and selection bias

The endogeneity between peace and democracy introduces selection bias into most additive models of international conflict. A weakly-endogenous relationship, such as that suggested by Russett and Oneal (2001), may 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 likelihood 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—trade, IGO membership, rivalry, and alliances, for a few examples—this implies far-reaching concerns for studies of the ancillary properties of the democratic peace.

A strongly endogenous relationship—our suggestion of populations of regime types split by external threat—may have even more troubling implications for existing studies. Should democracy be dependent in some fashion on the establishment of peace, then democratic differences in a particular variable may simply underscore a more pervasive sample-selection process that made these cases observable. In the example we chose for this paper, the alliances with democracies are likely to be those that were formed or continue after a threat has subsided. These alliances face different political environments than most alliances, and are likely to serve purposes other than just power aggregation. These alliances are unlikely to be tested in peaceful environments and are, therefore, less likely to be abrogated, more likely to last longer, and more likely to be institutionalized over time. When we observe democracy correlated with these tendencies, we are really observing the effects of peaceful environments on alliance behavior.

Consider again the many findings associating democracies with behaviors that are often different from other types of states. First, we noted how democracies are unlikely to form alliances but tend to be allied after large, regional defense pacts take hold. As Gibler and Wolford (2006) point out, this is consistent with the argument that alliances are removing threats to the states, and political decentralization of the state follows demilitarization. These findings also confirm that joint democracies are unlikely to form an alliance with each other and, given the connection between threat and alliance formation, also imply that democratic alliances continue in peaceful regions during peaceful times.

When democracies do involve themselves in alliance making, they are more likely to do so during peaceful periods and use alliances for different purposes. If our argument is correct, then democracies form alliances that last longer and are more reliable—with fewer violations, abrogations, and longer durations—not because of anything inherent in their system of government but because they are part of the subsample of states at peace. Without the threat of conflict affecting alliance members, their alliances will less often be tested by potential aggressors. Too, these alliances are more likely to be formed for cooperative purposes, such as resolving specific coordination issues or recognizing independent state status, and will behave differently in the international system when compared to the sample of capability-aggregating alliances formed during dangerous times.

This type of logic applies to the ancillary findings suggesting democratic differences as well. Since the alliances are formed in the absence of threats, their different, non-traditional purpose suggests an increased likelihood of finding trade pacts, institutionalization, and other

factors associated with closer cooperation between states. Again, rather than democracies necessarily being somehow different, these regimes may simply more likely to be in the subset of states at peace who are able to use alliances as tools for cooperation.

Scraps of paper

Note there are actually two selection effects present in the observation of a democracy in an alliance. We describe the regime-based selection above, but there is also the process that determines whether an alliance should be formed in the first place. Traditional alliance theory argues that alliances are most often a response to dangerous environments as leaders pursue external balancing to counter immediate threats, and this has often been confirmed empirically. This is also why alliances are correlated with conflict at such a high level—as much as 25% of the alliance data have formation or termination dates related to war (see Kang and Gibler 2013).

The implications of this relationship for alliance theory are noteworthy. Thus far, the literature has seemed to focus on how democratic alliances are different and how democracy can solve commitment and information problems inherent in international relations. If we are correct, though, democracy in alliance is most often just a proxy for alliances made during or continuing into peaceful times, and the search for democratic differences has obscured the confirmation of a major expectation in alliance theory. Alliances without democracies, or those in dangerous neighborhoods, are likely to constitute the alliances expected by traditional international relations theorists. Thus, once we separate the sample of alliances made in peaceful environments, alliance-making during crises may be seen as the "scraps of paper" so famously described by the German Chancellor prior to World War I.

What we describe here is the observation of two separate samples of alliances—the traditional, capability-aggregating alliances and alliances formed for other purposes, during peaceful periods. In the next sections we explore this argument by using an existing model of how threat affects state development and then apply it to a study that finds democracies are less likely to violate their alliances. This pairing demonstrates well the endogeneity bias we suspect. Democracies are much less likely to take part in the capability-aggregating alliances, and this is why they are less likely to violate their alliance obligations. Democratic differences are due to selection effects.

Research Design

We replicate and extend two recent studies that explore the determinants of democratization and alliance abrogation to test our hypotheses: Gibler and Tir (2014) and Leeds, Mattes and Vogel (2009). The Gibler and Tir (2014) study demonstrates that external territorial threats to the state are key predictors of the likelihood of democratization. In fact, the addition of territorial threat to their model renders the democratizing effects of neighboring and regional democracies statistically insignificant. This finding is especially useful for our purposes because it establishes a baseline prediction of democratization, based on external threat, that does not rely on variables associated with regime type. Thus, by substituting this measure of the likelihood of democratization, we are able to describe the potential

conflict environment for each state independent of regime type. We apply the existing model of external threat to the Leeds, Mattes and Vogel (2009) study, which finds 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.

Our sample of cases are those observations that are in both studies which provides a sample that spans 1951–1999. Our primary dependent variable is *Alliance Violation*, coded as 1 if there is a violation and 0 otherwise. Our primary variables of interest are territorial threat and democracy. *Territorial Threat* is the maximum level of cross-border threat that a state faces, which is operationalized as the maximum predicted probability of a fatal militarized interstate dispute for state A across all of its dyadic relationships (Gibler and Tir 2014, 33).[2] We use the Leeds, Mattes and Vogel (2009) data for *Democracy* which is coded as 1 if a state has a score ≥ 6 on the -10 to 10 Polity IV index, and 0 otherwise (Marshall and Jaggers 2008). All other control variables remain the same as defined in these two studies.

Methodology

We expect alliances to separate into two general types—reliable and unreliable—based on the level of external threat affecting the states. States in more threatening environments are more likely to violate their alliances terms, while states in safer environments have fewer opportunities for alliance violation. If our argument is correct, then full-sample estimates of the predictors of alliance violation will be biased. Introducing controls for threat environment should demonstrate this.

Table 1 provides some initial support for our expectations. We divide states into high and low *territorial threat* environments, with high *territorial threat* defined as those cases above median threat levels in the sample. Sixty-eight percent of alliance violations occur in high threat environments, and the difference between conflict environments is statistically significant ($\chi^2 = 3.901, p = 0.048$). External threats affect the probability of whether a state honors its alliance commitments.

Ignoring the conditioning effect of threat environments and treating all alliance observations as equally at risk of entering the population of states that may violate their alliance commitments—which is true for traditional binary-choice estimators such as logit or probit—

[2]The predicted probability of a fatal militarized interstate dispute is estimated using Gibler and Tir (2014, Table 1). Leeds, Mattes and Vogel (2009) include a measure of external threat in their analysis. Their measure, however, differs substantially from Gibler and Tir's (2014) on the *territorial* nature of the threat facing each state. The external threat measure used by Leeds, Mattes and Vogel (2009) is from Leeds and Savun (2007, 1127) and represents the sum of the capabilities (Correlates of War CINC scores) for politically relevant states (neighbors and major powers) with whom a state does not share an alliance and with whom its foreign policy affinity score (S score, a similarity score based on alliance portfolio) is below the median value in their sample (median = .775). Leeds and Savun consider there to be a change in threat levels if a state outside the alliance weakens or its affinity score increases. They code a change in external threat as a binary variable equal to 1 if the level of external threat between the current year and the start year of the alliance decreased by 30%. Leeds, Mattes and Vogel (2009) extend this variable to include cases where there is an increase of 30% in an ally's threat. These measures differ substantially in their focus on territorial versus all threats and also in terms of measurement, continuous versus dichotomous.

Table 1: Territorial Threat and Alliance Violations

	No Violation	Violation	Total
High Territorial Threat	2,344 (50%)	21 (68%)	2,365 (50%)
Low Territorial Threat	2,349	10	2,359
Total	4,693	31	4,724

$\chi^2 = 3.901, p = 0.048$. Unit of analysis is directed dyad year. Observations are cases in both Gibler and Tir (2014) and Leeds et al (2009). *High Territorial Threat* is coded as threat levels above the median.

is a type of model misspecification (Heckman 1979; Signorino and Yilmaz 2003). Ordinarily, when there is a distinct process or condition that must be present to select into the "at risk" group, one would estimate a selection model. Unfortunately, the structure of the alliance data does not permit this, since 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 the earlier selection stage. This means the data are partially observed since observing the outcome variable is dependent on an unobserved joint process (Poirier 1980).[3]

To address these data limitations, we use a split-population logistic regression model to test of our hypotheses. A split-population logit is a type of mixture model, where an outcome variable is a function of two distributions (Xiang 2010). The logic of the estimator is that there are two distinct populations in the data, and entry into each population can be estimated probabilistically. The estimator does this by using two equations: one equation that functions as the selector, identifying *relevant* observations, and a second equation that operates as a traditional binary model estimating outcome.

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

$$\begin{aligned} Y_i &= 0 \text{ with probability } (1 - G_i) + (G_i)(1 - F_i) \\ Y_i &= 1 \text{ with probability } G_i F_i \end{aligned}$$

where G and F are cumulative distribution functions (CDF) of a binary choice model following logistic distributions.[4] In our case, G represents the probability that a state is *relevant* to the sample—the "at risk" states—and conditions F, which represents the likelihood of alliance abrogation. In our empirical analysis, we use variables obtained from the model described by Gibler and Tir (2014, Table 2, Model 1) to specify the G equation, and variables described in Leeds, Mattes and Vogel (2009, Table 1, Model 1) to specify the F equation. This allows us to statistically separate alliance members in non-threatening environments, which are unlikely to have their alliance invoked, from the more traditional conception of alliance membership, where threats to member states will lead to the possibility of alliance terms being invoked.

[3]See Nieman (2015) for a discussion of data structure and selection processes.

[4]Consistent with both Gibler and Tir (2014) and Leeds, Mattes and Vogel (2009), we assume G and F are follow logistic CDFs. The model could also adhere to other binary distributions, such as a normal.

Empirical Analysis

Table 2 presents the results of our replications and our original analyses using a split-population logit. We estimate five models: we begin by replicating the Gibler and Tir (2014) and Leeds, Mattes and Vogel (2009) studies in models 1 and 2. We then re-estimate Leeds, Mattes and Vogel (2009) using only observations that are common to both studies. Next, we estimate a split-population model, using Gibler and Tir (2014) to specify the *relevance* equation and Leeds, Mattes and Vogel (2009) as the basis for the *violation* equation. Finally, we re-estimate the split-population model, using a more sparsely specified model that excludes monadic- and system-level democracy variables from the *relevance* equation. The top of the table reports the *relevance* (selection) equation, and the bottom of the table reports the *violation* equation.

The first two models are exact replications of the original Gibler and Tir (2014, Table 1, Model 1) and Leeds, Mattes and Vogel (2009, Table 1, Model 1) studies and are consistent with their results. All coefficients and standard errors are the same as in the original studies. Model 3 restricts the Leeds, Mattes and Vogel (2009) sample of cases to only those observations included in both studies. The only substantive change caused by the restricted sample is for the variable *alliance agreement includes nonmilitary cooperation*; this control is no longer statistically significant. Notably, all other variables demonstrate *stronger* substantive effects in the restricted sample and provide an excellent baseline from which to compare our split-sample model.

Model 4 estimates a split-population model where external threat is treated as the *relevance* equation, which identifies cases that select into the cases at risk of violating their alliances in the *violation* equation. As expected, the coefficient on *territorial threat* remains positive and statistically significant in the *relevance* equation. Further, its introduction into the estimate of alliance violation renders *democracy* statistically insignificant, with a standard error that is more than five times larger than the coefficient. Model 5 provides another estimate with a restricted set of variables in the *relevance* equation. The results remain substantively the same as in the previous model: *territorial threat* remains positive and statistically significant while *democracy* is no longer a predictor of alliance violation.

To ensure that our approach is a better representation of the true data generating process, we also compare the model fits between models 3 and 4, 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 3 and 4 are non-nested due to their differing functional forms: model 3 assumes a non-linear logit function, while model 4 is a mixture of two logistic distributions (Clarke 2001).[5] 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. Clarke's distribution-free test, meanwhile, tests whether the the median log-likelihood ratio of two models are equal. If the first model is closer to the true specification, more than half of the individual log-likelihood ratios will be greater than zero.

Table 3 reports the results of our non-nested model comparisons. The test statistic for

[5]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).

Table 2: Development, Democracy, and Alliance Violations.

Model	Gibler and Tir Replication	Leeds et al Replication	Leeds et al Cases in both	Split-Population Cases in both	Split-Population Cases in both
DV	Democratic Transition	Alliance Violation	Alliance Violation	Alliance Violation	Alliance Violation
Equation				Relevance	Relevance
Territorial-threat Level	-13.236**			14.413**	14.658**
	(3.331)			(7.152)	(6.160)
Territorial-threat (2-year Change)	-11.758**			8.355	8.372
	(3.675)			(5.973)	(6.016)
Number of Democracies on Border	0.361**			0.083	0.100
	(0.147)			(0.287)	(0.255)
Number of Borders	-0.132			-0.070	-0.072
	(0.093)			(0.184)	(0.182)
Island State	-0.209			-0.440	-0.269
	(0.599)			(1.052)	(0.967)
Number of Democratic Transitions, region (lag)	0.365**			-0.042	
	(0.154)			(0.451)	
Proportion Democratic, System	-6.167			-1.850	
	(5.466)			(9.937)	
Cold War Year	-0.959			1.787	1.928
	(0.811)			(1.823)	(1.183)
GDP per Capita (lag)	0.282*			-0.883**	-0.842**
	(0.151)			(0.278)	(0.237)
Polity IV Score (lag)	0.136*			0.043	
	(0.027)			(0.145)	
Oil State	-0.538			2.180*	2.140*
	(0.488)			(0.746)	(0.727)
Number of Previous Transitions in State	1.491*			0.002	
	(0.393)			(0.816)	
Years as Non-democracy	0.011*			0.001	
	(0.003)			(0.012)	
Constant	-2.531			0.578	-0.568
	(2.711)			(5.282)	(2.562)
Equation				Violation	Violation
Change in Leader's Societal Coalition		0.889**	1.771**	3.011*	3.351**
		(0.436)	(0.592)	(1.557)	(1.406)
Democratic State		-1.322**	-2.227**	-0.599	0.170
		(0.393)	(0.660)	(3.109)	(1.638)
Change in International Power Since Alliance		0.803**	1.018**	1.409*	1.463*
Formation in Either State		(0.330)	(0.502)	(0.807)	(0.770)
Change in Political Institutions Since		0.131	-0.176	-0.310	-0.190
Alliance Formation in Either State		(0.308)	(0.512)	(1.024)	(0.910)
Change in External Threat Since Alliance		0.421	-0.061	-0.313	-0.202
Formation		(0.270)	(0.487)	(1.034)	(0.815)
Formation of New Outside Alliance		1.070**	1.400**	1.545**	1.628**
by this State		(0.253)	(0.388)	(0.725)	(0.648)
Alliance Includes One Major Power and		-0.408	-0.470	-0.480	-0.445
One Minor Power		(0.259)	(0.436)	(0.722)	(0.690)
Alliance Agreement Includes		-0.746**	0.278	0.679	0.616
Nonmilitary Cooperation		(0.257)	(0.490)	(1.029)	(0.906)
Alliance Formed Through Treaty		-0.083	-0.310	-0.346	-0.213
Requiring Ratification		(0.354)	(0.483)	(0.975)	(0.825)
Alliance Agreement Requires Peacetime		0.557**	1.166**	1.462*	1.438**
Military Coordination		(0.180)	(0.292)	(0.821)	(0.558)
Time		-0.086	0.006	0.002	-0.014
		(0.077)	(0.124)	(0.291)	(0.246)
Time Squared		0.001	-0.003	0.001	0.001
		(0.004)	(0.006)	(0.014)	(0.011)
Time Cubed		<0.001	¡0.001	>-0.001	<0.001
		(<0.001)	(<0.001)	(<0.001)	(<0.001)
Constant		-4.448**	-6.228**	-4.756**	-4.853**
		(0.440)	(0.942)	(1.389)	(1.520)
Log-Likelihood	-280.109	-352.394	-156.969	-143.747	-143.875
N (alliances)	3884	6612 (223)	4724 (136)	4724 (136)	4724 (136)

Note: $^{**}p$ <0.05, $^{*}p$ <0.10, two-tailed.

the Vuong test is 58.27, with 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 greater log-likelihood for 3110 of the 4724 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 model 4. We find similar support for the restricted split-population model in model 5. Overall, the results indicate that the two split-sample models outperform the model which assumes all states are initially equally "at risk" of violating an alliance.

These results are consistent with our primary theoretical expectations. Threatening environments affect the underlying propensity of states to enter the sample of states at risk of violating their alliance terms. Moreover, once the endogenous effects of threat environment are

Table 3: Non-nested Model Comparison.

	Full	Restricted
Vuong Test		
Vuong	82.13	57.39
SE	1.41	1.72
t-statistic	58.27	33.40
p-value	$< .001$	$< .001$
Clarke Test		
$\sum_{\mathrm{i}}^{\mathrm{n}} \mathrm{ll}_{\mathrm{M_{Alt}},i} - \mathrm{ll}_{\mathrm{M_3},i} > 0$	3110	3063
$\sum_{\mathrm{i}}^{\mathrm{n}} \mathrm{ll}_{\mathrm{M_{Alt}},i} - \mathrm{ll}_{\mathrm{M_3},i} < 0$	1614	1661
Positive, one-side test (p-value)	$< .001$	$< .001$

Note: Model 3 is the reference category in each test. Clarke distribution-free test uses binomial distribution ($p = .5$).

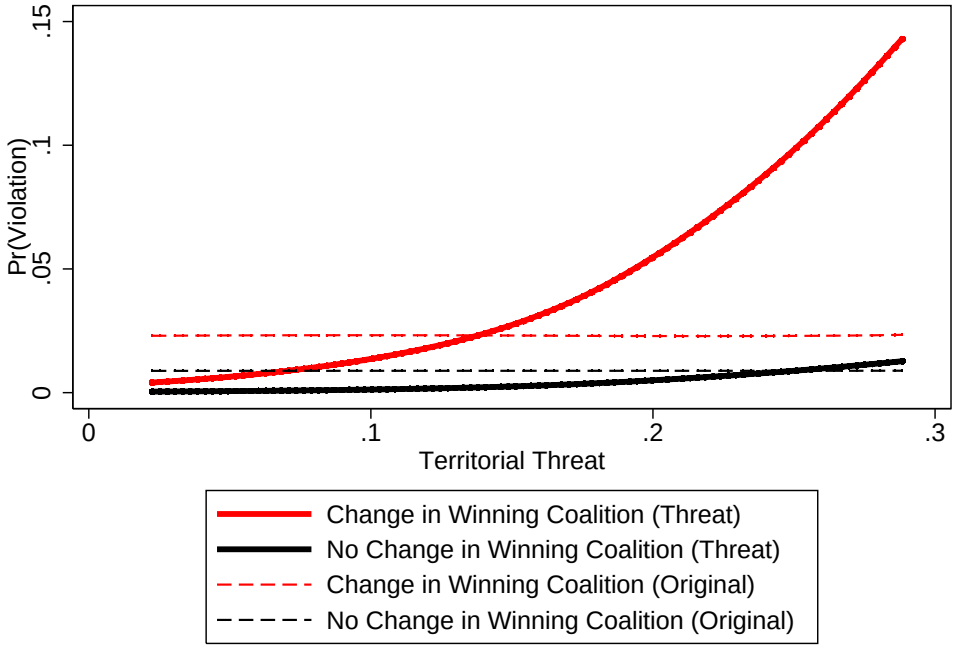
accounted for, democratic institutions do not appear to exert a significant impact on whether an alliance violation occurs. States are more likely to democratize in non-threatening environments, and, thus, they are less likely to be involved in the traditional, capability-aggregating alliances associated with international conflict. Instead, alliances signed in peaceful environments appear to have been signed for other reasons, such as deepening economic ties (Gowa and Mansfield 2004; Long 2003) or signaling embeddedness within a security network (Lake 2009).

To assess the substantive effect of accounting for threat environment on alliance abrogation, we report predicted probabilities of an alliance violation in Figure 1. We report predicted probabilities for two conditions: (1) 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 the level of territorial threat affecting the state [solid lines]. As a point of reference, we also compare these predicted probabilities to those from Leeds, Mattes and Vogel's original analysis [dashed lines]. Changes in a leader's winning coalition lead to an increase in the probability of an alliance violation [red lines above corresponding black lines]. The effect of a change in a leader's winning coalition is significantly increased as the degree of territorial threat rises [solid red line]. Increases in territorial threat, however, 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.[6] Taken together, the results suggest that territorial threat acts as a primary factor, perhaps even a necessary condition, for the occurrence of alliance violations.

Our results suggest that the rate at which democratic states honor their alliance terms may be inflated. Once we account for the two distinct populations of alliances—those signed to aggregate capabilities during times of threat and those signed in peaceful environments for

[6]The predicted probability for alliance violations in a democratic state that experience a change in their leader's winning coalition at every territorial threat level is almost identical to the probability of a violation when there is a change in the leader's winning coalition in non-democracies.

Figure 1: Predicted Probabilities of an Alliance Violation, Change in Leader's Winning Coalition, and Territorial Threat.



Note: Estimates with Territorial Threat from Table 2, Model 5. All variables held at mean or median.

other reasons—it appears that the rate of alliance reliability is much lower. It is only those alliances at relatively high risk of territorial threat—at least above the median value—that select into the population of states that abrogate an alliance. In fact, there is only one alliance violation (3% of total violations) in the observations with the lowest 10% of *territorial threat* scores and only five (16% of total violations) in the lowest 25% of *territorial threat* scores in the sample. In comparison, there are 13 (42% of total violations) violations in the highest 25% of *territorial threat* scores in the sample.

Alliances signed in threatening environments also appear to differ from those signed in less threatening environments in other ways. Looking at the full sample from the Leeds, Mattes and Vogel (2009) study, the mean length of time that an alliance survives is 14.2 years. Alliances signed when territorial threat is very high (>.25 in the first or second year of the alliance), however, survive for an average of only 7.9 years, which is a decrease of over 40%. This is even less than the violations that occur after a change in a leader's winning coalition, when alliances survive for an average of 9 more years.

Analyzing the texts of alliance treaties

We have demonstrated that external threat affects the likelihood of an alliance violation. We believe external threat should also affect the types of alliance signals that are sent to other states in the system. To test this, we examine the actual language used by the alliance

Table 4: OLS Estimates of Alliance Text Content

	Word Count	We-ness	Tentativeness
Territorial threat	-1708.321*	0.515*	3.503*
	(820.732)	(0.213)	(1.535)
Both democratic	-21.731	-0.020	0.380
	(198.088)	(0.051)	(0.370)
One democratic	302.279*	0.014	0.262
	(137.907)	(0.036)	(0.258)
Asymmeteric	-175.524	-0.003	0.321
	(115.755)	(0.030)	(0.216)
Constant	1149.714***	-0.029	1.558***
	(136.504)	(0.035)	(0.255)
N	114	114	114

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

partners when making their agreements. We use a LIWC Dictionary (Linguistic Inquiry and Word Count) that can compare the English-language treaty texts to a dictionary of words commonly associated with several different psychological concepts (Pennebaker et al. 2015). Statistically significant comparisons are derived from texts that differ substantially from the average use of each concept in normal language use. Obviously, alliance language is not normal language use, but we are comparing predictors using alliance texts only—not alliance texts and other types of writing.

Here, we examine the sense of "we" elicited by the text; this variable can serve as a proxy for the level of togetherness and commitment alliance members are trying to portray. The dictionary for "we" includes, for example, first person plural words such as we, our, and us and other similar words. We also examine the tentativeness of the language in the text which portrays the conditions under which the alliance partners will act and their intentions should those conditions manifest.

We estimated several OLS regressions using a similar set of independent variables as possible predictors of alliance-text differences. Our predictors include the territorial threat variable described earlier (averaged across participants), the number of democracies in the alliance (dummy variables for mixed dyads or jointly democratic dyads in alliance), and whether the alliance was asymmetric, as coded by Leeds, Mattes and Vogel (2009). All of these are measured at the time of alliance formation. We have complete English language texts for 228 alliances; however, the Leeds, Mattes and Vogel (2009) study examines only bilateral pacts and limits their examination to alliances ending prior to 1989. This provides a sample of 114 alliance texts for analysis, which is presented in Table 4.

Using our predictors, we do find differences in the types of agreements that are made between states under threat and those states at relative peace. For example, a simple count of the number of words in a treaty text is strongly determined by the average level

of territorial threat facing the participants. A move from least-threatening environment to most-threatening environment reduces the word count of the treaty text by over 1700 words; meanwhile, the presence of one democracy increases the word count by over 175 words on average. For comparison, the longest treaty contains 3444 words, the shortest treaty has 148 words, and the average treaty has almost 950 words. Neither status asymmetry or the presence of two democracies was statistically significant.

The content of the treaties made in the shadow of threat also changes. The common model suggests that alliance texts imply a sense of togetherness, or "we"-ness, when states are threatened. As we know from our previous analyses, the alliances formed during territorial threat are more likely to be violated, so this result is consistent with the argument that alliances are often being used as a means of bluffing potential aggressors.

The language of alliances formed in the shadow of threats is also more tentative. High-threat alliances are likely to have explicit conditions that targeted states must meet if certain events occur. For example, three of the highest-threat alliances occurred during World War II. An alliance between the Soviets and Yugoslavia in 1941 had 362 words and carried the stipulation, "Should either of the contracting parties be subjected to attack from a third State, the other contracting party undertakes to observe a policy of friendship toward it." Similar language is in the 1941, 370-word treaty between the Soviets and Japan: "Should one of the Contracting Parties become the object of hostilities on the part of one or several third powers, the other Contracting Party will observe neutrality throughout the duration of the conflict." Finally, the Soviets and France agree to the following in 1944: "In the event either of the high contracting parties finds itself involved in military operations against Germany, whether as a result of aggression committed by the latter or as a result of the operation of the above Article III, the other party shall at once render it every aid and assistance within its power." None of the other predictors was statistically significant for we-ness or tentativeness of language.

Combined with our earlier analyses, we have confidence that threat effectively divides the types of alliances that are made in the system. High-threat predicts both the likelihood of violations and also the content of the agreements that are made. Threat also tends to select democracies out of the alliances we observe at a rate higher than what we would expect given the distribution of regime types. Previous attempts to distinguish among alliance types do not fare as well.

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. However, our findings suggest that their are two qualitatively distinct types of alliances: those in peaceful environments and those in hostile environments. The latter type of alliance are reactions to threats to the state and are 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 other periods (e.g., Senese and Vasquez 2008; Levy 1981).

Our results also point out a need for more research on those alliances that are formed or exist in peaceful environments, as these types of alliances have been largely ignored in the existing literature that focuses on alliances as a means to power aggregation. If states do not face a need to deter a potential adversary, why are such alliances formed? Once the issues that produce threatening environments are resolved, why do such alliances continue, or, even more puzzlingly, why are they often further institutionalized? Additionally, several broad alliance networks formally call for states with little, if any, power projection to come to the aid of a state. The OAS, for example, includes Uruguay, Haiti, and Costa Rica. It seems unlikely that any of these states would be able to actively participate in the defense of the others. Even if each of these states' primary motivation to join the alliance was to secure a pact with the US, why include formal obligations on the part of the these minor powers in the agreement? By focusing primarily on alliances in threatening environments, scholars have largely ignored other potential purposes and effects of such alliance ties.

Finally, our argument has potential implications for the many 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. However, both of these literatures tend to pool samples without regard to threat environment. This is actually true for almost all studies that find some type of democratic difference in state behavior. Ultimately, democratic institutions may still somehow 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 selection effect of dangerous environments, current estimates of the effect of democracy on behavior are biased and may be spurious.

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