Dear Editor and Reviewers,

I thank you all for the opportunity to submit revisions of my manuscript "A Random Item Response Model of External Territorial Threat, 1816-2010" for possible inclusion in *Journal of Global Security Studies* (*JoGSS*). I thank the two reviewers and the editorial team for the helpful comments as well. In the remainder of this cover letter, I address the editor about changes to the manuscript that came both in light of reviewer feedback and as well additional data work on this project. Thereafter, I address the reviewers' comments individually.

Comments to the Editorial Team

The editorial team noted the general agreement from the reviewers about the utility of the project and its potential, though also concurred with the reviewers' feedback that more work needed to be done before the paper could be published in *JoGSS*. The editorial team asked that I address the reviewer feedback on all accounts, especially as they pertain to why the journal's audience should be interested in this project. I want to summarize the important changes in light of this feedback here.

First, I took care to better sell the reviewers (prominently Reviewer #1) on the importance of this project. The editorial team echoed this issue as well and both the editorial team and Reviewer #1 encouraged possibly moving some of the replications into the appendix if space considerations precluded me emphasizing the importance of the project. In light of this, I moved the replication of the militarization analysis in Gibler (2012) to the appendix and dedicated more attention to the issue of the importance of the project. Briefly: territorial threat scholarship has always used disputed territory as a stand-in for more general arguments of how external threat environments condition domestic political processes (e.g. Hutchison and Gibler, 2007; Miller, 2017). International relations scholarship notes the difficulty in identifying which foreign policy issues are salient ex ante; disputed territory historically happens to be that (e.g. Gibler, 2010; Tir, 2010). The data I provide here are useful not only for general hypotheses of how external threat influences domestic politics, but also assessing the scope and severity of the threat and over longer time periods than has otherwise been available or practical in some of these analyses (c.f. Hutchison and Gibler, 2007; Tir and Bailey, 2018).

Second, and related to the above point, the manuscript now comes with an appendix that has supplemental information about the measure and additional analyses and robustness tests. The appendix contains replications of Gibler's (2012) arguments linking territorial threat to militarization and the removal of veto players and the Kang and Kim (2020) argument linking territorial threat to women's legislative representation. It also contains a comparison of the main territorial threat measure I generate with both an alternate estimations suggested by Reviewer #2 along with the measure I had submitted for review. Finally, I offer an assessment of the territorial threat measure with other variables in the replication models and with other approaches to modeling territorial threat (i.e. by reference to spatial rivalries and territorial claims). The central takeaway here is the measure of territorial threat I generate is a valid measure of the concept, not simply duplicating existing information, and points to important variation that is not evident in binary, "there"/"not there" indicators of territorial claims and spatial rivalries.

Third, the measure underwent some changes as a result of the peer-review process and from additional consideration of the politically relevant dyad sampling frame. The appendix shows that while a politically relevant dyad sampling frame is useful overall for analyses of interstate conflict (Lemke and Reed, 2001), it will have some important limitations for understanding the full scope and severity of territorial threat. Table A.4 and A.5 also point to important spatial rivalries and territorial claims that are not involving politically relevant dyads. The appendix shows some of these are important observations as well and should be modeled. This is why I substitute the universe of directed dyad-years for the politically relevant directed dyad-years to make sure these observations are included. The distance weights will still reduce the effect of functionally irrelevant dyadic pairings (e.g. Mongolia-Nigeria) on the estimates that follow. Additionally, there has been major data innovations on the front of fatalities in militarized conflicts (c.f. Gibler and Miller, Forthcoming) and the fatality indicators are now much more informative. All told, Figure A.1 still shows a very high correlation (almost .8) with the measure I originally submitted for peer review and the measure I submit with this revision.

Enclosed are the revised manuscript and appendix. I thank the editorial team and the reviewers for their helpful comments and for further consideration of my revisions.

Best regards,

The Author

Reviewer #1's Comments

Reviewer #1 (R1) had the following comments on the original version of the manuscript. Overall, I identified three major issues that R1 raised. R1 starts with a minor note that I could be clearer what I am ultimately doing in my estimation procedure. The biggest issue R1 raised is ultimately a quite helpful one; the "who cares?" question was not adequately answered and I could better sell the importance of what I am doing here. R1 also asked for more information about how correlated the territorial threat measure is with some other indicators available in the models I replicate. I discuss these in turn.

A Minor Note: Make Clearer What the Approach Ultimately Is

R1 started with a minor note that I could have made it clearer what the modeling approach ultimately is. My discussion early in the manuscript had emphasized that scholars will typically use spatial rivalries (Thompson and Dreyer, 2012) or territorial claims (Frederick, Hensel and Macaulay, 2017) as a functional, if inherently limited, indicator of the concept of "territorial threat." As I mention, the problem with this is not that these indicators are invalid, just that they are information-poor, "there"/"not there" measures that can be expanded and augmented with other information that is also in orbit of the concept in question. R1 recommended I make this clearer in the first paragraph that starts to explain the estimation process (p. 4), which I believe I do in this revision.

Expand the Appeal; the "Who Cares?" Question Needs More Work

R1 indicated the biggest shortcoming of the manuscript as I originally submitted it was that it was not evident to the potential *JoGSS* reader why this project is important. In other words, the first version of the manuscript was more invested in describing the estimation and its validity, assuming the reader understood the project's importance in the absence of my explanation of the project's importance. A "big picture" discussion was absent and the first version of the manuscript misses the figurative forest for the trees. R1 recommended I take this seriously even if the restrictions of the research note format means some of the replications should go into an appendix. I took this opportunity seriously and want to emphasize here the major value of this measure. Overall, what I offer in this manuscript is important for researchers (and *JoGSS'* readers) for the following reasons.

The biggest contribution of this new measure of territorial threat might even be less about "territorial threat", per se. The scholarship that I review all share a unifying interest in how international relations influences domestic politics. Some will even cite Gourevitch (1978) to argue for the international sources of domestic politics. Thus, the scholarship on territorial threat's effects on domestic politics is almost always built on general arguments. Hutchison and Gibler (2007) advance a *general* argument about how external threat creates conditions for political intolerance at home. Their application is external, territorial threat but their inspiration also clearly connects to Cold War scholarship on the threat of international communism to Americans (e.g. Stouffer, 1955).[1] Miller (2017) advances a *general* argument about how external threat creates conditions that draw individuals to executive authority. His application is also external, territorial threat but his inspiration explicitly connects to terror management theory scholarship at the time looking at the "mortality threat" of international terrorism (e.g. Landau et al., 2004). Both echo a point made more explicit by the likes of Gibler (2010) and Tir (2010) in

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[1] The threat of international communism to Americans would be more of a general "policy" threat, at least in the crude Correlates of War issue typology introduced in Jones, Bremer and Singer (1996). It happened to be a particularly salient external threat to Americans, however.

their respective analyses linking territorial threat to centralization and diversionary conflict. Arguments about the effect of external, territorial threat double as general arguments for the international sources of domestic politics. The greater difficulty has always been identifying which types of external threats are going to be salient, ex ante. The territorial dispute literature happened to have an answer the extent to which it identified disputed territory as a root cause of war (e.g. Vasquez, 1995; Senese and Vasquez, 2003). Thus, scholarship in this vein that operates at the intersection of international relations and comparative politics will find this data set useful for testing their more general arguments anchored to Gourevitch's (1978) "second image reversed" perspective.

The second major contribution returns to the limitation of binary, "there"/"not there" indicators. This in part motivated the construction of this metric, as I mention in the manuscript, and it becomes more apparent in the updated replication I provide of Hong and Kim (2019). Absent the type of measure I provide here, scholars will lean on dummy variables of spatial rivalries or territorial claims to approximate the concept of "territorial threat." However, those dummy variables cannot communicate much in terms of "severity" of territorial threat. Thus, Hong and Kim (2019) will report a statistically significant interaction between spatial rivalries and exclusionary ideology on mass killing onsets in cases of state failure. They will reasonably attribute the mass killing episodes initiated in Cambodia in 1975 to the threat the Khmer Rouge regime perceived from Vietnam (p. 534 in Hong and Kim, 2019). After all, Cambodia in 1975 had an exclusionary regime, a spatial rivalry, and, consistent with the argument, a mass killing onset. However, the simulations I do of their model (substituting spatial rivalries for my measure of territorial threat) and report in Figure 3 in the revised manuscript will paint a more complete picture about how exclusionary regimes behave under conditions of territorial threat with respect to mass killing episodes. The differences between exclusionary regimes (like the Khmer Rouge regime) and non-exclusionary regimes only become discernible at higher levels of territorial threat, and levels higher than what Cambodia had in 1975 when those mass killings started. Looking through the data they compile, I make the case that scholars who want to understand this relationship should instead think of the case of the Cultural Revolution in China. This was a particularly severe case. The mass killings began in 1966 amid a backdrop of the emerging Sino-Soviet split (and onset of a China-Soviet spatial rivalry), a recent invasion by India (the Great Northern Push over that disputed territory), and a still tense relationship with Taiwan that resulted in a series of crises in the mid-1950s that took place in the Taiwan Strait. China's regional threat environment at the time came from all sides and it should be unsurprising that the Cultural Revolution happened (per their argument). That was a particularly severe case. Consider too the nature of the problem that Hong and Kim (2019) want to explain: mass killing episodes (a severe and mercifully rare event) under conditions of state failure (another severe and rare event). It makes sense such a grisly episode is only likely to happen under particularly severe conditions, and that assessment of how severe territorial threat must be for those distinctions to emerge between exclusionary regimes and non-exclusionary regimes cannot be gathered when using a binary indicator like spatial rivalries.

R1 was prescient that it might make sense to move some of the replication material into an appendix if space restrictions would interfere with elaborating the project's importance. I moved the Gibler (2012) replication into the appendix in order to best address this feedback. Doing this allowed me more space to illustrate how useful the measure is for understanding important phenomenon and how it can be used more generally. I think this makes for a better paper for *JoGSS*' readers.

Explore Potential Collinearity in the Hong and Kim (2019) Replication

R1 expressed an interest in whether there was a potential collinearity issue in the Hong and Kim (2019) replication. The rationale here is straightforward. A measure of territorial threat that includes information about

disputes and fatalities in the context of spatial rivalries or claimed territory is indirectly going to include information about militarization. The Hong and Kim (2019) replication that I submitted for review offered interesting departures from the analyses the authors published, which might have been attributable to that issue.

Curious about this myself, I created correlation matrices for all four replications that I do as part of this project. These are the Hong and Kim (2019) analysis of mass killing onsets, the Kang and Kim (2020) analysis of women's legislative representation, and the Gibler (2012) analyses of changes in military personnel and changes in the composition of veto players. This information is available as Figure A.3 and Figure A.4 in the appendix. Overall, there is no problematic correlation between the territorial threat variable and any other right-hand side variable in any of these replications. Indeed, the territorial threat variable is not even highly correlated with the spatial rivalry variable in the Kang and Kim (2020) analysis of women's legislative representation. This is surprising, prima facie, but it is perfectly intuitive. Spatial rivalries are only "there" or "not there." In the kind of data that Kang and Kim (2020) assemble (i.e. state-years from 1985 to 2010), states that have them rarely end them and states that do not have them typically do not enter them. Meanwhile, the territorial threat variable varies for each state, each year, contingent on the indicators that inform them. Overall, these figures suggest no major collinearity concern in any of the replications and suggest the territorial threat measure I generate is indeed a distinct measure that is not ultimately just some other readily available indicator.

Reviewer #2's Comments

I thank Reviewer #2 (R2) for their interest in the research note I submitted for review. R2's feedback focused on two categories. The first is a conceptualization issue about whether I think of "territorial threat" as objective or subjective. The second class of comments focused on the empirics, in part raising questions about how ambiguous the first draft was. I discuss these in turn.

Is "Territorial Threat" Subjective or Objective?

R2 pointed to some ambiguity in my language regarding territorial threat. Namely, some of the language I had used in offering a conceptual definition of territorial threat suggested I think of territorial threat as subjective (e.g. "level of concern", "perceived threat"), though the indicators I use are all implying an objective estimate.

I thank R2 for highlighting these issues and I have tried to be clearer in my language and have altered the conceptual definitions (and overall language) to reflect that. To R2's point, I think of "territorial threat", conceptually, as a latent phenomenon concerning a state's territorial integrity and how prone it is to violent conflict and attempts at revision/contraction by other states. The indicators I use, and the past scholarship I review, all treat this as something more "objective" than "subjective." In other words, territorial threat is estimated based on recent/current indicators of conflict, rivalry relationships, violent territorial contractions, or targeted claims. However, these indicators are noisy in their own ways though I contend they can be appropriately modeled to devise an informative estimate of this phenomenon in question.

The Starting Point for the Data is Dyadic; So Why is the Measure Monadic?

R2 suggested that a reader is going to be thrown for a loop in how I weave the manuscript around two different levels of analysis. For example, I discuss the limitations of using a dyadic estimate as a stand-in for a monadic concept, but the data around which I ultimately derive a monadic estimate are entirely dyadic. Under those conditions, would it not make sense to add in monadic indicators? After all, the first draft of the manuscript I submitted for review mentioned I had been using monadic indicators in the measure.

On that note, I did want to clarify that I had accidentally misled the reviewer about the use of monadic indicators. Verbatim, I had written "the second (territorial change indicator) is the number of violent transfers for Side A a state-level input." This was incorrect, and from it I see R2's point about adding even more monadic indicators if this is what I had been doing. What I did (and should have written) is add the number of violent transfers for Side A in the directed dyad. This would have avoided R2's understandable confusion, and I thank R2 for alerting me to this before a wider set of eyes (i.e. *JoGSS*' readership) saw it. The language in the revised manuscript corrects this.

With this in mind, I want to reiterate there are no monadic indicators whatsoever in the construction of the measure. However, the estimate that emerges from it is monadic because of the random item response modeling procedure. I thank R2 for understanding that, ultimately, the estimate of state-level threat is the estimate across all directed dyads and that the modeling procedure is a way of leveraging the available dyadic information into a monadic indicator. The random item response modeling procedure and the construction of the data do not make it necessary to have monadic summaries based on these dyadic data.

Why Use the Categorical Distance Scale?

R2 noted that the categorical distance scale I construct is ultimately arbitrary and unique to this project. It is seen nowhere in any conflict scholarship and, thus, it would be fair to wonder why I did this and if the results from it are sensitive to this peculiar scale. After all, we have the minimum distance data (c.f. Weidmann and Gleditsch, 2010). Why not use that instead?

An important development happened in the time between the first submission of this manuscript for review and the decision from *JoGSS* to extend an opportunity to revise and resubmit the paper. I was prepared to reply that the minimum distance data available at the time included coverage only to 1946, and I had wanted to maximize the temporal coverage in this measure as much as I could. This would be a major boon to conflict researchers to explore hypotheses of territorial threat's effects on domestic politics into the 19th century. Fortunately, Schvitz et al. (2021) released an update to the minimum distance data that push the left bound of the temporal domain to 1886 in the time between the submission of the first draft and the receipt of the invitation to revise and resubmit the paper. This provides at least some 19th century coverage (and a fair bit of pre-world war coverage). It also allowed me to re-estimate the models of territorial threat with an alternate distance weight (i.e. the inverted minimum distance between states in the dyad).

The appendix contains ample information about the effect that my (admittedly arbitrary) distance category weight has on the estimate of territorial threat relative to a better measure like minimum dyadic distance. Namely, there are no differences. Figure A.1 shows the Pearson's *r* between the territorial threat measure I offer for wide use (with coverage from 1816 to 2010) and an alternate measure using the minimum distance weights (with coverage from 1886 to 2010) is .97. Figure A.2 re-estimates all four replications I provide with this project with this alternate territorial threat measure. In three of the four replications, the results are functionally identical. In the one case where there are differences (i.e. the effect of territorial threat on changes in military personnel), the differences are attributable to the changes in temporal domain (i.e. territorial threat seems to have stronger effects on changes in military personnel from 1886 to 2010 than they have on changes in military personnel from 1816 to 2010). Both effects are clearly discernible from zero.

Interestingly, the Pearon's *r* between the different values of dyadic minimum distance and the distance category is -.919. That is a very strong negative correlation (i.e. my dyadic distance categories increase as dyadic minimum distance decreases). This suggests the distance category is I construct is a useful stand-in for minimum distance until the International Conflict Research team that maintains the CShapes data ultimately extend the temporal domain of their data further into the past. No matter how arbitrary the distance categories are, they approximate well the information communicated in the minimum distance data and have no implications for the measure I construct relative to the more informative minimum distance data.

Rethink the Discussion of the Interactive Effect in Hong and Kim (2019)

R2 noted that an insignificant interactive term is not necessarily evidence against an interactive effect (e.g. Brambor, Clark and Golder, 2006) and that I should rethink what I am doing here before this potentially goes to print in *JoGSS*. I thank R2 for raising this issue and I want to emphasize what I have done here in light of this feedback and with other changes to the measure I clarify in the letter to the editor.

First, and to R2's point that the insignificant interaction term was not necessarily definitive that Hong and Kim's (2019) findings were incorrect, Model 2 of Table 1 reports a statistically significant interaction. This would be consistent with the authors' original findings.

Second, I took up R2's invitation to more fully explore what the interactive effect looks like, especially as it may relate to the novelty of the measure I create relative to what Hong and Kim (2019) use in the absence of it. This is Figure 3 in the manuscript. Figure 3 uses an informal/empirical Bayesian approach (c.f. Gelman and Hill, 2007) to estimate the probability of a mass killing onset for exclusionary and non-exclusionary regimes across the range of the territorial threat measure, as a quantity of interest (King, Tomz and Wittenberg, 2000). What emerges is a caveat that may not otherwise be evident from Hong and Kim's (2019) analysis. For example, the author's reference the mass killing of Khmer Rouge dissidents in 1975 as illustrative of their findings and hypothesis since Khmer Rouge officials made explicit reference to the threat from Vietnam as pretense for these grisly human rights atrocities (Hong and Kim, 2019, 534). My simulations do not discount this, but they add a caveat that the Cambodian case is not necessarily informative of what kind of territorial threat is necessary to observe a distinction between exclusionary and non-exclusionary regimes. Consider that mass killing onsets in episodes of state failure are rare events, and that what the authors want to explain is itself a particularly grisly kind of human rights abuse.[2] Under those conditions, it makes sense that my simulations suggest that much higher levels of territorial threat than Cambodia in 1975 are needed to observe differences between exclusionary regimes and non-exclusionary regimes. I suggest that my simulations point to a case like the Cultural Revolution in China as more informative. These mass killings began in 1966 amid the emerging Sino-Soviet split, a recent invasion from India, and in the backdrop of two deadly confrontations with Taiwan in the late 1950s. This is useful information for scholars interested in understanding and preventing mass killing onsets. A binary measure like Hong and Kim (2019) use in the absence of a more informative measure cannot answer what kind of threat severity is necessary to observe differences in exclusionary and non-exclusionary regimes of interest to their research question.

Clarify the Meaning of Zero in the Measure

R2 pointed to Figure 1 in the manuscript that the reader could benefit from a discussion of the meaning of zero in the measure. I took up this opportunity to expand on what zero would mean in this concept. Basically, zero plays an important role in model identification in this application because the random intercepts are assumed to have a mean of zero and the particular categories of the random effects are seen as deviations from this mean and thus from the intercept (e.g. De Boeck, 2008; Choo et al., 2014). The simulations I do on top of this incorporate the fixed term of the global intercept, which communicates the global mean across all observations in the model (i.e. the universe of observations from 1816 to 2010). One way of interpreting this is to think of zero as communicating a kind of reference point for average territorial threat observed across time and space. As a yearly mean exceeds zero and becomes negative, it indicates greater an average level of territorial threat, in a given year, that is below that global mean observed across all observations.

That Figure 1 shows we observe this in the 1990s (and a clear downward trend in territorial threat after World War II) is certainly consistent with some important IR scholarship. Goertz, Diehl and Balas (2016) argue that the grisly nature of World War II resulted in norms discouraging territorial conquest. Gleditsch (2002) argues that zones of peace resulted in Europe after World War II, which is going to play a large role in this trend as well.

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[2]In the data Hong and Kim (2019) construct, there are 991 state failure episode-years from 1951 to 2010. Of those, 34 episode-years included a mass killing onset. This is just under 4% of observations.

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