Ref.:  Ms. No. APSR-D-20-01487  
The Shadow of Deterrence: Why capable actors engage in conflict short of war  
AMERICAN POLITICAL SCIENCE REVIEW  
  
Dear Professor Gannon,  
  
Thank you for submitting your manuscript, "The Shadow of Deterrence: Why capable actors engage in conflict short of war" to the American Political Science Review. We appreciate your interest in the journal.  
  
We have now received sufficient feedback from our reviewers to reach a decision. Unfortunately, we are unable to publish your manuscript. This ends the review process.  
  
Like the reviewers, we admire and see as timely both the use of a formal model to understand gray zone conflicts and the empirical examination of cases of Russian incursions. However, we also share their concerns about whether the paper is poised to make a sufficiently distinctive contribution. Most notably, they do not see the central predictions as particularly surprising (R1); worry that is does not sufficiently engage deterrence theory or examine pre-crisis stages that characterize general deterrence (R2); would like the model to be more sophisticated in terms of making the level or type of force used a continuous parameter, incorporating bargaining, incomplete information, or repeated play (R3); and push you to develop an empirical test that better speaks to your particular causal mechanism and rules out others (R3). All reviewers suggest that with some revisions, the paper might be better suited to a subfield journal like the Journal of Conflict Resolution.   
  
As you know, we receive an exceptionally high volume of submissions and are only able to publish a small fraction of that work, which forces us to be very selective and only invite papers for resubmission that have a high chance of being accepted after revision.  
  
The reviewers' comments appear at the foot of this email and/or in the attached files. We hope you find them helpful as you continue your work on this project.  
  
We are grateful to you for giving us the opportunity to consider your research for publication in the APSR, and we hope you will continue to submit your best work to the journal.  
  
Sincerely,  
The Editors  
American Political Science Review  
  
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Reviewers' comments:  
  
Reviewer #1: This paper develops a formal model of gray zone conflict and pairs it with a quantitative analysis of Russian incursions into surrounding regions. The main formal result presented is that increasing a defender's deterrent threat decreases the intensity of a challenger's provocation. The main empirical result is that NATO countries (those that reasonably have a better deterrent threat) see fewer Russian military actions.  
  
At one level, I don't see anything wrong with the analysis, either from a formal or quantitative standpoint. My central criticism of the manuscript is that the results are not surprising. From the formal side, the central claim is that if one side can better threaten the other, the other is more inclined to give up. That is a common argument, and the model is not providing much additional nuance to the subject. This might be fine on the empirical side of things if there were a very well-identified result that we would want to know the magnitude of the effect on. But (to their credit), the authors are careful to note the limits of the empirical study --- i.e., it is not causally identified because countries do not join NATO as-if randomly.  
  
I can see two ways the authors might wish to go forward. First is to focus on the novel aspects that the model covers but the authors do not engage much with. The narrative surrounding Observation 1 basically says that the challenger does not alter the status quo if its opponent has a low war cost but will engage in gray zone conflict if the opponent has a high war cost. I understand why the authors are doing this --- it is the narrative the authors want to pair with the empirical analysis. But this is suppressing the more interesting result from the observation: if the challenger has a lower cost of war, it might just fight instead. The authors could then spend more time on a case study of the more interesting dynamics --- assuming that those effects would hard to be parse out in a quantitative analysis.  
  
(Aside: I think footnote 7 has a typo. It should be $k\_C \leq 0.41$, not be $k\_C =0.41$, right? Otherwise, this is just a knife-edge condition.)  
  
The second option is to submit the paper to a field journal. Going down that route, I would still encourage the authors to emphasize the more novel aspects of the theoretical results.  
  
A few other things. Does it matter that C goes first? First movers usually have the advantage in Stackelberg-like competition, but this is not discussed. Is there any reason not to use general loss functions instead of using squared terms? Is it weird that if D fights, C pays for the gray zone investment but does not get anything out of it? Does that drive any results?  
  
Finally, I found the discussion that straddles pages 11 and 12 to not be compelling. The paper makes an assumption about how to structure the utility functions to avoid situations where medium-resolve challengers go to war but high-resolve challengers choose gray zone conflict. At minimum, the authors should formalize within the text what the assumption is. Regardless, though, the assumptions should be driving the results, not the results driving the assumptions. If this actually is a weird outcome, then the authors need to give a compelling argument as to why the assumptions on the utility functions that generate are strange. That argument cannot use what the outcome of the equilibrium would be as justification. (I think the authors understand this, but it is not coming across in the text.)  
  
  
Reviewer #2: In this manuscript, the authors develop a game-theoretic model of gray zone deterrence, and provide empirical tests of their argument. Gray zone deterrence is certainly an increasingly important and popular topic to examine. However, I have a variety of concerns with the manuscript.  
  
My primary concerns about the manuscript are about the set up of the game-theoretic model, which is the central purpose of the manuscript. At the outset of the discussion of the model, the authors state that they assume challenger and defender "are in a crisis" (p. 9). I find that to be surprising and disappointing for several reasons. First, general deterrence is more important than immediate deterrence (i.e., deterrence within crises) because if general deterrence succeeds, then crises are avoided. Further, when we are talking about gray zone conflict, that seems like something that would be an attempt to avoid a crisis, not something that a state only considers once a crisis has already started. So I think that the model should start before the crisis, not after the crisis has already started.  
  
Also, the authors state that gc = 0 means "accepting the status quo" (p. 9), while gc > 0 means "challenging the status quo" (p. 10). But if they're in a crisis already, then a challenge to the status quo has already been made. So how can they accept it now? And what does a further challenge at this point mean? At least from the standpoint of standard deterrence theory (e.g., Zagare and Kilgour 2000), if they are in a crisis now, then challenger has already initiated a challenge, and then defender didn't back down right away, so if challenger sets wc = 0 and gc = 0, then that would indicate that challenger is backing down, probably because their initial challenge was a bluff.  
  
It also isn't clear whether the authors are talking about direct or extended deterrence. Is defender only trying to deter challenger from attacking defender (direct deterrence), or is defender trying to deter challenger from attacking a third state (extended deterrence)? The model only has two states, but the various examples talk about deterring Russia from challenging in Ukraine, Estonia, and elsewhere, suggesting that extended deterrence is the focus. This is an important distinction that isn't even addressed.  
  
Part of the problem is that the manuscript seems to be quite disconnected from deterrence theory. Which is strange, because it seems to be trying to make a contribution to deterrence theory. But I don't see how it does so in its present form.  
  
The authors also describe the case when wD = 1 as "D declares war" (p. 10). What does this represent? Although there have been many wars, there have been few declarations of war since World War II. So why incorporate a declaration of war into the theory?  
  
In the section on data, the authors cite Singer, Bremer, and Stuckey (1972) for the International Crisis Behavior dataset. This doesn't make any sense, as the two have nothing to do with each other.  
  
Also, in a few places the authors refer to section numbers, but the sections are not numbered.  
  
Frankly, I spent little time on the last half of the manuscript; since the model seems to be so disconnected with reality and with deterrence theory, what is the point of going any further with the manuscript? Overall, I believe that there are too many issues with this manuscript for publication in APSR to be appropriate. Following revisions, I would recommend submitting it to a journal such as ISQ or JCR.  
  
  
  
Reviewer #3: This manuscript studies the dynamics of limited use of force or "gray zone conflict" among two adversaries. It introduces a simple model of deterrence where there are different levels of conflict and the credibility of the deterrent threat varies. It includes statistical and case analyses of crises involving Russia post-1990 to support the theoretical arguments.  
  
While there is much to like in this manuscript, in my opinion, the theoretical and empirical contribution is not large enough for APSR. There are also a few issues, which I list below. A revised version of the manuscript could be a good fit for a subfield conflict journal.  
  
On the model:  
  
It is not clear what is gained by treating war and gray zone operations as qualitatively different uses of force in the model. This needs to be discussed further. Why can't we define a single, continuous parameter that captures different levels of conflict, ranging from lowest to all the way to total war? The empirical analysis implicitly makes this assumption through the ordered probit model and its underlying latent variable structure.  
  
The model has no bargaining. Given that both war and gray zone conflict are costly, there should be peaceful revisions to the status quo that the states prefer in the existing model. This also reduces the applicability of the model to the cases discussed in the manuscript, in terms of explaining why bargaining failed.  
  
The model does not incorporate any uncertainty about the defender's deterrence behavior, either through incomplete information about resolve or mixed strategies.  Uncertainty likely plays a big role in deterrence interactions, including the cases discussed.  
  
The model is single round, and there is not much discussion on how the implications would change when there is repeated interaction. For instance, all three analyzed cases and the data set are about Russia, and arguably there should be learning from each crisis about Russia's future behavior, and NATO's likely response.       
  
On the empirics:  
  
There seems to be some mismatch between the model and the empirical analysis. Overall, what the empirical analysis suggests is that NATO deters Russian aggression, and Russia is more likely to show hostility to its immediate neighbors. This is not necessarily a strong support for the model itself. These results could be consistent with many alternative explanations, which are not explored in detail.  
  
If there are any quotes from key decision makers consistent with the theoretical expectations, they would strengthen the overall empirical analysis. Given that these cases are relatively recent, it may be difficult to access relevant documents, so perhaps these are not the most useful cases to analyze. Other historical cases, for instance those involving CIA covert operations in Latin America during the Cold War, about which there are lots of declassified documents, could be more promising alternatives for direct tests of the theory.