ISEC-19-161

"After Deterrence Explaining Conflict Short of War"

International Security

Dear Mr. Gannon,

Thank you again for submitting your paper, “After Deterrence: Explaining Conflicts Short of War,” to International Security and for your patience during the review process.

During the past six weeks, the manuscript has passed through the International Security review process, which includes internal and external evaluation. As you will see, we are attaching three external reviews. One review recommends accept, while two others recommend against publication. Unfortunately, I regret to inform you that the journal has decided not to accept the paper for publication.

Manuscript decisions are particularly difficult when reviewer feedback is split. The editors agreed, however, with many of the critical comments of the reviewers. In particular, we found Reviewer C’s comments to be particularly persuasive and incisive. From an editorial perspective, there was agreement that the paper could do more to persuade readers of the value of its findings (which are not necessarily contrary to what one would expect, including from alternative explanations), as well as how conceptualizing “gray zones” as an independent category of conflict is greater than the sum of its component parts and other related categorizations that are already well-defined.

In case you are wondering why International Security is not asking you to revise and resubmit the paper, I should explain that the journal only requests revisions when it is very likely that the revised manuscript will be accepted for publication. This policy reflects the fact that International Security receives many more publishable manuscripts than it can print. The journal receives about 300 submissions for 15-20 article slots each year. We tend to invite revisions when the external reviewers recommend publication and to accept manuscripts only when the external reviewers enthusiastically recommend publication.

To be clear, there is a lot to like in this piece and it was enjoyable to read your work on a growing subject. We hope that you are able to publish the paper in another journal and that the enclosed comments are useful if you decide to revise the paper and submit it elsewhere.

Thank you again for sharing your manuscript with us. We also very much hope that in spite of this decision you will keep International Security in mind for your future work.

Sincerely,

Morgan L. Kaplan

Executive Editor

International Security

Reviewer comments and attachments (if any):

**Reviewer A:**

I think this is a really clever article in how they flip the causal arrow about deterrence and weak actors having to use gray zone warfare strategies. I like the new dataset of Russian hostilities, and the good mixed method approach, to show how Western credibility alters Russian military actions and what role geographic proximity plays.

It is fantastic that they have proffered a theory of gray zone conflict into the deterrence literature, although I think it could benefit from several articles on the specific literature of "gray zone deterrence" and "gray deterrence." The typology of limited conflict is a great way for the authors to make their point, and show where Gray Zone conflicts lie (as are the cases to exemplify each).

The "pull their punches" expression could be better described for non-American speakers, possible relying on "restraint in employing some violence/coercion" and/or something to that effect. There is also some sloppiness of citations and the style in which they were done, which I think they should have done a better job on for submitting to such a blue-chip journal. In their concluding chapter, I think they might benefit from such writings on multi-domain operations, and what the West is doing to improve its efforts at multi-domain deterrence.

**Reviewer B:**

The authors of this article propose a "deterrence gradient" where deterrence credibility is inversely proportional to gray zone conflict intensity. Unfortunately, after reading this piece, I am not convinced of this deterrence gradient is different than competing loss-of-strength gradients and recommend International Security reject this article because of issues with the theory, dataset, qualitative analysis, and generally.

Theoretically, the deterrence gradient makes sense. Unfortunately, the choice to instrument the deterrence gradient with geography makes me question how your theory differs from Boulding's loss-of-strength gradient. An equally plausible explanation for the variation in Russian conflict intensity could be the relative strength in each country. American strength is obviously very strong at home, while Russia lacks the resources to significantly project forces. Because of Russia's loss-of-strength gradient, they are constrained in their options for interfering in American elections. In Georgia, however, Russia can easily project all manner of national power, while the United States is relatively constrained. Perhaps locating a better proxy would remedy this concern. Consider variables like American foreign direct investment, arms transfers, security cooperation activities, etc. Ultimately, I'm not sure your theory breaks new ground, which is ok, but the evidence you present further undermines your overall argument.

Both the methodology for building the new dataset and the analysis leave significant room for improvement. First, the "10 additional instances" added to the DCID and REI databases reek of cherry-picking without discussion of how you identified those instances. After flushing out your identification strategy, consider adding more detail for how you recoded each type of aggression. What sources did you consult, what rules defined each type, and who coded them?

Then, I need to know what kinds of variables these are. As the article reads, they seem like binary variables. However, you then put them on a linear scale in Figure 1. Your linear scale implies a "5" intensity includes information, cyber, paramilitary operations, air/naval, and ground forces. However, I'm not sure the Syria case really includes significant IO / Cyber operations or that the Georgia campaign included cyber options. A better intensity measure might sum the number of types of aggression present.

The data analysis is also crude. At a minimum, I'd expect you to show a linear regression with the dependent variable of conflict intensity (1-5) with the independent variable of distance from Washington. This is the core of your thesis and I'm not sure that your map and Figure 2 provide strong evidence. You might also consider a multinomial logit regression with your aggression types as the dependent variable and distance from Washington as your independent variable. I'd expect to see evidence that IO was more likely closer to Washington, while ground forces were more likely further away. Both of these regressions, though basic, would provide support for your theory without overburdening your small statistical power. Your cases could then build on that evidence.

The cases cry out for a more rigorous analytical framework. If these cases seek to add evidence that deterrence credibility is inversely related to conflict intensity, I'd expect you to explicitly and obviously address the deterrence credibility and conflict intensity of each case. Each case should open with 1-2 sentences outlining whether the case supports your theory and the status of deterrence credibility / conflict intensity in each. Instead, each case begins with a different meander through the case.

Consider the Ukraine 2014 case (p26-28). As a reader, I'm looking for a succinct summary of whether and how the Ukraine case does or does not support your theory. Instead, the case spends two paragraphs talking about other cases and the historical background before telling me anything about deterrence. Bookend each case with what the reader should take away and how this information relates to the theory.

Beyond the data and case analysis, the paper needs to answer important questions about external validity, policy implications, and political science implications. The paper sets out a theory for all gray zone conflict and then focuses on Russia. This is fine. However, the authors must address whether this theory applies only in the West and Middle East, or whether China, Iran, and others respond to their deterrence gradient. This paper might also comment on whether current reassurance efforts in Europe are enhancing American deterrence credibility, how this paper builds on existing research, and what gaps remain.

Finally, the writing throughout this paper is weak. The authors mix metaphors and need to improve their concision throughout. I'm fine with the authors use "pulling punches" at least a dozen times, but don't then invoke the "full court press" on page 29. The paper generally integrates a significant amount of colloquial language ("pull out the stops", "pull punches", "take the gloves off", "full court press") where more precise language would improve the paper. In a few places, the authors fail to define terms, like "spiral model" on page 15. The paper has a limited number of mechanical errors, such as at the bottom of page 15 (if -> it, also -> a).

The paper has other issues, but these comments should help the authors as they revise the paper. Gray zone conflict is an important issue, and we need better ways to understand whether deterrence is working below the level of open war.

**Reviewer C:**

This articles reviews "grey zone conflict," argues for its merit as a concept, and theorizes that appearance of this behavior and variation in its intensity by reference to the strength of deterrence. They test this empirically against Russian activity since the Cold War and find that, consistent with the theory, Russian activity is more ambitious the further away from the core deterrence location of NATO.

One strength of the article is anti-alarmism. The piece makes the very important point that resorting to grey zone tactics may be good news for those responding to them. This is an intervention worth making in Washington and has real promise. Unfortunately, this isn't the heart of the article as a piece of social science. Instead, the authors (will use plural for convenience) spends most of its time making the case that the severity of grey zone activity declines with distance.

The contribution of this finding is far less clear. One reason is that there are obvious reasons to expect it to decline with distance even if deterrence is irrelevant. More on that below. The other reason is more basic: what's the significance of learning that grey zone techniques will be more mild 1000 miles from China's shores vs. its immediate neighborhood? There simply isn't as much at stake in understanding the severity-geography link as there is in other things, such as whether grey zone behavior is really worthy of a new label (more below) and whether it reflects strength or weakness. Thus, the movement of the article's center of gravity toward the severity/geography issue is a mistake.

Within these parameters, the biggest hole in the article is one that, to the authors credit, is acknowledged explicitly: "One might argue that Russia values the stakes differently in each conflict and thus the correlation with the deterrence gradient observed in Table 2 is spurious." That is precisely what struck me and, I fear, will strike many other readers. Simply put, there's lots of reasons to expect grey zone behavior to be more circumscribed far from the capital of its sponsor.

Thus, one would expect countries to fight harder in their immediate sphere of influence and for conflicts that, say, feature co-ethnics. Further away should feature more caution and also more limited interests. All of this is monadic reasoning devoid of a story about the state of deterrence. The authors respond by saying that Ukraine bucks that trend but is consistent with a deterrence story. Rather than fight that issue, I think there's a bigger problem here: the authors have conceded that an obvious and intuitive alternative explanation explains the pattern in 3 of the 4 cases they analyze. By their own admission, the deterrence story only helps us clean up a puzzle about Ukraine. While that is an important puzzle and an important case of grey zone techniques, does it merit an article in IS? Another way of putting this: the severity of grey zone activity by Russia in 75% of the cases the authors select themselves is overdetermined, making the deterrence gradient concept sufficient but not necessary for understanding most of the empirical content they present.

More broadly, the paper could do a better job attacking the logic and explanatory value of alternative explanations. I found even the broader claims about whether "efficiency" explains the pattern - which sometimes seem like the same point as lower stakes and sometimes not - a bit underwhelming. The authors claim that "Limited war that is motivated by efficiency, by contrast, should be less correlated with geography." I'm not sure why they state this so confidently. We might think that states simply counting up the costs of various forms of grey zone activity might be more likely to use cheaper methods further from home. That would follow the internal logic of BdM's loss of strength gradient noted by the authors, and it \*would\* be correlated with geography. Thus, regardless of declining stakes \*and\* leaving aside deterrence, wouldn't we expect Russia to be less ambitious further away? Why do we need to know about deterrence, if stakes and cost of logistics both give us reasonably clear reasons to expect Russia or China to be more ambitious closer to home?

Another weakness of the paper is its definition of grey zone tactics and its justification for adopting this label. Most readers familiar with grey zone discussions will know that many question the validity of this label. Many see it as old wine in new bottles and/or Pentagon-speak to hype a threat that is inconsistently defined. This background means that any paper on this topic in a journal of the stature of IS must tread carefully in defining "grey zone behavior" and in defending it as a conceptually and strategically distinct thing.

I'm not sure the paper does either. First, it does not have a full throated response to the idea that the component parts of "grey zone behavior" were just fine on their own. If it's cyber attacks + covert/deniable military operations + reliance on proxies … well that's basically just Cold War competition plus cyber. The authors are refreshingly honest about this, listing on p. 4 that a range of items like proxy wars is related. But the paper could use a stronger defense of why a new unifying label is needed. The 2x2 on p. 14 is useful in telling us what grey zone \*isn't\* but that isn't a justification for using the "grey zone conflicts" label for what is in the lower left box.

Compounding the problem is some curious categorizations made by the authors. On the one hand they call it an "emerging phenomenon" (abstract). On the other hand, they retroactively classify conflicts which seem to suggest it's been here all along. They choose to include overt military activity in their research design, implicitly expanding their conceptualization of grey zone to include acknowledged, public Russian interventions in Chechnya and Georgia. Granted, both included other Russian activities like cyber attacks. But I'm not sure others would classify conflicts in which the intervener overtly uses military force (air or land) as grey zone. Was American bombing of North Vietnam in 1965 now "grey zone," since it only used air power and covert action? In fact, the authors almost argue exactly this. They go so far as to suggest containment of Iraq between the two wars was all grey zone activity: "Between 1991 and 2003, the United States engaged in a continuous gray zone contest to contain Saddam Hussein with air policing, economic sanctions, covert intelligence, and occasional air strikes" (p. 13). Now that too is included? Historical revisionism like this may be merited - perhaps we \*should\* go back and recode a lot of modern conflict history - but one needs to make this case very carefully.

A final reason I don't support moving forward with this manuscript: the paper begins with a title and intro of broad interest, including noting China's use of what some call grey zone techniques. But it quickly becomes a paper about post-Cold War Russia. One wonders how much of the theory is an overfit set of claims specific to Russian behavior vs. a broader framework. The scope pares back the significance of the findings, not to zero, but to a paper on grey zone & Russia (vs. a more broadly cast and appealing paper on grey zone across states).

Smaller issues to consider as the authors move forward.

--What do we make of Syria? Isn't this case, in which Russia does quite a bit in a theater far from Moscow, a deviant case for the theory? If so, how do you make sense of it? They note on p. 21 that it is an exception to the pattern but lamely note the presence of a forward operating base. That's not part of the theory, which would require a deterrence-oriented explanation.

--If the authors are identifying a type separation problem, aren't there other ways to separate whether grey zone actors are responding to deterrence, cost of logistics, or stakes? Could we look at other behaviors, besides variation in the intensity/scope of grey zone activity, which might be indications of one or the other?