Responses to comments by anonymous reviewer #1 on *How much should we believe correlations between Arctic cyclones and sea ice extent?* by J.G.L. Rae et al.

We thank the reviewer for his/her comments. We have already uploaded a short response, but give more detailed responses in blue text below.

This paper has tried to correlate the sea ice anomalies with the summer cyclonic activity in Arctic and found that the correlations are highly sensitive to the model and resolution used for tracking the cyclonic activity.
While we were indeed looking for correlations between September ice extent and summer Arctic cyclone activity, we went further than this in that we also analysed the robustness or otherwise of such correlations. The sensitivity to model, resolution, and spatial and temporal sampling was the key conclusion of the paper – that the cyclone-ice correlations found by previous authors are not robust because they are sensitive to all these factors.

While the paper is generally well written and the authors explain well their methodology, I do not recommend to accept this paper for publication in TC because this subject has already been discussed in many papers (as well presented by the authors) and that this paper only shows that the variability of sea ice extent is definitely NOT driven by the summer cyclonic activity!
The papers mentioned by the reviewer, which we do indeed cite in our paper, studied such correlations in observations/reanalyses, not in a long coupled GCM run as we did. This meant that they were restricted to the ~30 years of available satellite observations of sea ice extent (as opposed to the 100 years of model output we used). In addition, they were restricted to one resolution and one tracking variable. Those studies found apparent correlations/links between summer cyclones and September ice extent, i.e. they appeared to show that the variability of sea ice extent *was* driven by the summer cyclone activity. We showed that these correlations are dependent on model, resolution, tracking variable, spatial domain, and time period used, and that they are therefore not robust. Our main conclusion was that the results of those previous studies should be treated with caution for this reason.

The correlations found (< 0.44) are not enough relevant to justify a paper in TC and obviously explains why the results are very sensitive to the model and resolution used.
As we explained in the paper, the correlations presented are all significant at least at the 90% level, and often at the 95% level.

If the correlations would be scientifically robust, the sign of the correlations should be for example every time the same which is not the case here. Correlating sea ice extent with summer T850 will give likely better correlations but it is out of the scope of this paper.
This is exactly the point we were trying to make in the paper – that the correlations depend on a range of different factors, and are therefore not robust. Therefore, the correlations which were found in the other studies we cited may also not be robust, and those studies may have drawn misguided conclusions from a limited set of results.

As we mentioned in our previous short response to Reviewer #1, we now believe we did not make the aims of the paper clear enough, leading to a misunderstanding of what those aims were. We have therefore re-worded the abstract and part of the final paragraph of the introduction, and added an extra sentence in the conclusions section. We hope that these changes make the aims of the paper clearer.