

Re-Review of “Intraseasonal prediction of monthly storminess in the North Sea with the ACE2 atmospheric emulator and Random Forests” by P. Aziz et al.

Overall Assessment

The authors have done a very good job in addressing both my concerns and that of the other reviewer. This latest iteration is much improved, and I thank the authors for undertaking my additional analysis suggestions in exploring the ACE2 results in more detail, which in my view augments the study impact. I would also remark that the story is a lot clearer in this version, as they have made considerable effort in more thoroughly linking their results to the wider body of published literature.

I can now recommend publication of the work in *Weather and Climate Dynamics*. However, following such major revision, I do have a handful of further comments/minor suggestions that I feel should still be addressed first. These should be easily actionable, requiring relatively little additional effort on the part of the authors. I note just one exception of a glaring error concerning that of the values in Table 3, which still requires (further) correction.

I include outstanding comments below, with associated line numbers (where applicable) in accordance with the revised manuscript (without track changes).

Specific Comments

L81-86: I would suggest moving this addition (‘Previous work suggests...’ onwards) to earlier in the paragraph, right after the sentence citing Bulter and Polvani (2011) on L77, for improved readability.

L128-135: It is possibly worth noting I think that the zonally symmetric component of the stratosphere circulation ought to be less relevant for the three SNAPSI cases looked at in Hitchcock et al. (2022), in the context of studying the effects of strengthened polar vortex conditions. That is because weak/SSW events can have greatly contrasting vortex geometries/zonal asymmetries.

That’s not to dispute the authors’ inference based on their results, but a strong, stable polar vortex does tend to be more zonally symmetric. So, it may be worth the authors revisiting/revising the text here.

Fig. 1c: Does it really make sense to show this correlation map for the whole globe? Only the NH mid to high latitudes are mentioned and the area of significance, in connection with North Sea storminess, would appear only to be confined to the North Atlantic region.

Therefore, it is perhaps worth the authors refining the plot to just the North Atlantic sector in the final version.

L243: ‘...corresponding ensemble of simulations...’ → As only one simulation is performed for control and perturbed individually, should the word ‘ensemble’ be changed?

Or perhaps at least change to ‘...corresponding ensemble of years...’, so it’s clear that initial conditions weren’t tweaked and multiple forecast experiments were generated for a given season.

Fig. 8: Although I think its good the authors show the signal for November initialisation start dates (Fig. 8); to illustrate the lack of signal between an artificially strengthened polar vortex and North Sea wind speed increase 7-60 days into the forecast, this result is not central to the main story in my view. I strongly suggest the authors move it to the Appendix for the final version.

Fig. 9: Maybe the authors could add quantification of the difference in 90th percentile of the PDF distributions, perhaps as a percentage value (perturbed – control/control)*100, due to the strengthened polar vortex?

Also of consideration might be calculation of risk ratios of the upper tail end shift (e.g., using Eq. 4 in Zhang and Wang, 2019)? It appears that for some years, the probability of days exceeding control 90th percentile might perhaps be doubled or even trebled, so calculating such metric would provide a clear, quantitative indication of the risk enhancement in surface extreme winds.

Table 2: I suggest moving this until after Fig. 10, to avoid any confusion by the reader that the analysis pertains to the new Fig. 9 analysis.

Fig. 11: Would it make more sense to display the years in chronological order? Or better yet perhaps, order them as a function of increasing Storm Index?

Table 3: The R² values still do not appear to correspond to the r values shown (e.g., Nov-Dec is 0.12 to 0.1 for R² but 0.31 to 0.52 for r?). Also, R² by definition can only ever be positive! Requires fixing.

L403: ‘...when the polar vortex is weak, the atmosphere becomes less active and stormier...’ → Worth double checking this is correct. From the abstract in Zhang et al. (2024), the suggestion is the opposite as ‘synoptic-scale eddies tend to be suppressed’ following weak polar vortex events.

Unless the authors mean to say, ‘less stormy’ and it is perhaps just poorly worded?

Technical Corrections

L174: ‘...grid near Hull...’ → ‘...grid cell near Hull...’

L175: ‘...The second grid is chosen...’ → ‘...The second grid cell is chosen...’

L186: ‘...basis grid cell.’ → ‘...basin grid cell.’?

Table 1 Caption: ‘None of these values is statistically significant.’ → ‘None of these values are statistically significant.’

Additional References

Zhang, J., & Wang, F. (2019). Changes in the risk of extreme climate events over East Asia at different global warming levels. *Water*, 11(12), 2535, <https://doi.org/10.3390/w11122535>.