Ocean Science
Discussions

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Interactive comment on "An approach to the verification of high-resolution ocean models using spatial methods" *by* Ric Crocker et al.

Anonymous Referee #1

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The paper untitled "An approach to the verification of high-resolution ocean models using spatial methods" described a really interesting method to quantify benefit from high resolution model. The paper describes in detail methodology and apply it to compare two ocean circulation forecast models on the Nordic Sea. Scientific results obtain comparing the two forecast system are poorly commented and explained but this scientific analysis is not the main topic of the paper which is really dedicated to the description, implementation of this methodology that was not already applied for ocean forecast. That could be frustrating for readers, authors can certainly add analysis of some results, some suggestions are provided below. Nevertheless, the paper is clear and objectives are well presented and I recommend the publication of this paper if authors take into account few following remarks and comments.

1. Section 2, Figure 1 : this figure presents the domain and the difference of coastline between the two models. Difference of coastline is an important point discussed also latter in the paper and illustrated on fig 4. To be really interesting, I recommend to highlight the differences between the two SST fields on this figure. A more contrasted color bar, for example, can highlight difference of spatial scale, intensity of SST fronts . . . which are the main reasons to apply the HiRA method in this context.

2. Section 3, line 187. Reference to WMO manual is useful but Authors should explained that this guide refers to Atmosphere and that ocean scales are really different. In this paragraph specificities of ocean should be described as difference of scale depending of the areas, open ocean vs shelf, rossby radius . . . This is briefly discussed later in the section (line 245) but it should appear before in the introduction of the method to justify to use it for ocean application.

3. Section 3, fig 3 and 4. Figure 3 and 4 are useful to understand the method and the neightbourhood concept. But it could be really useful to have, on these figures or with a new figure, a clear description (with an example) of how is computed the probability/density function especially in the coastal cases, how the observations are selected in a neightbourhood, where the coastline is different between the two models and when observations are removed from the statistics. A schematic view of this process should be really useful to understand easily some non-intuitive results as for example why there is less observation in a larger domain.

4. Section 4, line 290. I suggest to use zonal and meridional instead of horizontal and vertical.

5. Section 4, figure 4. Unclear or a mistake in the legend. Why a) is 7x7 neightbourhood (NB4) and b) NB5? Comparison should be done between similar neightbourhood.

6. Section 5 and 6, fig 5, 7, 9, 10. It's really difficult to identify differences between each line, probably too much lines on the same figures or more important line should be highlighted (in bold or with darker color?) NB1 and NB2 are the more important, it

is really difficult to distinguished them especially on fig7,9,10. Uncertainty, computed for each line, is difficult to associated to the right line. Is it useful to have the "1" line for AMM15, there is no comparison with AMM7? It's also difficult on these figures to have clear relationship between the uncertainty vertical bar and the difference bar. It will be useful to have on the figure or in a table the information where the difference bars are smaller than the uncertainty. This is discussed in the text (paragraph line 420) but it is difficult to verify what is described on the figures.

7. Section 5. Discussion on the different results obtained on-shelf and off-shelf is really interesting, but in the paper it appears as a mix between feasibility and useful methodology to compare several forecasts and a clear difference due to dynamics, physical ocean process and seasonal cycle. I suggest adding more quantitative information concerning the impact of the number of observation to compute robust statistics. The sentence (line 460) explains that the model are better to forecast open ocean, but is there any impact of the number of observation in the statistics? Do you compute statistics with the same number of observations in the two domain (off-shelf and on-shelf)? Fig 12 and 13 seems to exhibit larger uncertainty in the statistic on-shelf in comparison to off-shelf. On fig 12 and 13, it's clear that main differences between the two models appears in summer. That's not really discussed in the paper, is there clear explanation, is it due to physical seasonal processes or mainly due to the number of observations?

8. Section 5, line 479. Conclusion of this paragraph is not clear. What do you really mean by "closer look at the data"?

9. Section 5, line 508. Last sentence concludes on differences at NB2 scale, could you add comment on this conclusion about significance and robustness of this result.

10. Section 5, fig 14. On this figure lack of observations seems to appear end of May and in the text (line 487) authors indicate that missing data are in April.