

## ***Interactive comment on “Evaluation of numerical models by FerryBox and Fixed Platform in-situ data in the southern North Sea” by M. Haller et al.***

### **Anonymous Referee #2**

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The authors present a thorough comparison between data obtained from two operational numerical models of the North Sea and observational data (in particular, Ferrybox data and data from one platform of the MARNET network). The work is certainly relevant as such a comparison can be an invaluable tool for the evaluation and improvement of the numerical models. For the case at hand, it was found that the simulation of sea surface temperature is in general satisfactory. For salinity, the results are worse particularly close to the coast. To explain the discrepancies, the authors present several hypothesis (the quality of the fresh water discharge data, the grid nesting, . . .), and hence, they present also solutions to improve the models. Proving the hypothesis is out of the scope of the paper. However, this is already an important step to improve operational models in the North Sea (and probably, elsewhere).

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Although, the work has merit, the paper needs to and can be improved drastically. Firstly, I list some overall remarks, and later, I make some minor remarks. However, I do skip some of the smallest issues.

1. The introduction is long and goes into details that are not so relevant for the paper. In addition, the description of the residual circulation is not very clear. I find this to be particularly true when describing the connection between the Baltic Sea and the North Sea. It is also important to note that the description of the circulation is actually that of the residual circulation, and that tides are actually of high importance in the region. Tides are not even mentioned as one of the drivers for the residual circulation. Other smaller details in the introduction deserve attention: the isobaths mentioned in the introduction are not really visible in Fig. 1; although the North Sea is part of the Coastal Ocean, it is probably not correct to refer to it as "a costal ocean".

2. The use or lack of units for salinity is not consistent. In p. 361, l. 25 it is said that "salinity values are quoted in dimensionless numbers, even though Ferry Box measurements are performed in practical salinity units". First note that in principle the practical salinity units do not really exist and that the practical salinity scale is already dimensionless.

3. Section 2.6 is unclear. In general, it is not clear how the data was extracted from the models and observations. In addition, there are a couple of repetitions that add to the confusion. I find this section to be particularly important because the data extraction is the basis for all future analysis. Therefore, it is difficult to evaluate the validity of the paper as a whole if this is not clear.

4. It is said that the data of the AMM7 model are available for 2011 and 2012. However, the analysis covers only the period from April 2011 to April 2012. This has to be made clear in the text.

5. I would suggest that a large portion of the "Summary and conclusions" section goes into another section of discussion about perspectives for the combined use of FerryBox

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data and numerical models. In this way the conclusions can be more concise. I would really appreciate a concise conclusion in terms of the lessons learned to improve the models.

6. It is not clear why it is mentioned that BSHcmod v4 contains general vertical coordinates. They are not used, right? Are only z coordinates used? This is confusing.

Minor details:

- The expressions "accounting to" and "accounts to" are used several times. Do the authors mean "amounts to"?
- The authors use the preposition "at" in several places such as "the stde is at 0.8 K". In this cases, "at" is not necessary.
- The fonts in most figures a too small.
- It is probably of use to rename the subfigures as a), b), c), ... in order to refer to a specific plot.
- In several places the authors use "according" when they probably mean "corresponding"
- The explanation of the cost function cf is unclear.
- p. 366, l. 19: ... frontal zones form in this region. ...
- p. 368, l. 3: rephrase "it would be suggestive"
- p. 370, l. 19: the limiting values 0.7 and 1.2 are not observed in Fig. 6. They seem to be 0.8 and 1.1.
- p. 372, l. 22: the fact that there is a minimum of 0.15 at  $0.5^\circ$  E is not reflected in the Fig. 8.
- p. 376, l. 22: Do the authors mean that "the observed variability" cannot be reproduced by either model"?

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- p. 377, l. 16: the sentence on the freshwater eddies should be made clearer.
- p. 378, l. 21-23: It is said here that the salinity values are underestimated however it seems that they are actually overestimated. The drop, however, is underestimated. In addition, it is difficult to see this in the figure since the model data is plotted at the back.
- p. 378, l. 26: remove "should both"
- Fig. 1, caption: A green line is mentioned, but it does not appear in the figure.
- Fig. 5, it might be useful to plot a rectangle on the left figure, denoting the time covered by the AMM7 data. Also, the caption has to be corrected: change psi to K.

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