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## ***Interactive comment on “Validation of the NEMO-ERSEM operational ecosystem model for the North West European Continental Shelf” by K. P. Edwards et al.***

**Anonymous Referee #2**

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[12pt,a4paper]article

**Validation of the NEMO-ERSEM operational.....  
by K.P.EdwardsNeumann *et al.***

The manuscript focus on validation of an update of the operational model run at UK Metoffice. The updated system is compared both to the old system and to several different kind of data. The main change between the old and new system is the physical part of the model. The biochemistry appers to be the same in both systems. The physics have been validated in another publication, thus the present work only

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Discussion Paper



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Comment

focuses on the biochemistry. The paper is well written, and clearly demonstrates how the system has been improved after the update. However, there are several limitations in the manuscript as a validation paper. From these (see below) my recommendation would be reject. It is important to document that the new operational system is better than the old one, but as the old was very bad, and the validation is incomplete with a strong focus on correlations that is irrelevant as the seasonal signal are so strong, the manuscript is not enough for publication. However, I recommend the authors to bring their work further in a proper way for submission of a new paper showing the real quality of the new system. The authors also raise a very interesting discussion on satellite versus in situ data that should be brought forward.

My main concern is that the paper only focus on the biochemistry and that some of the results are impossible to discuss without saying anything about the physics. As an example take Figure 3. The POLCOMS-ERSEM results (upper panels) for the winter situation are so bad, that all other results discussed in the ms. becomes uninteresting. On the other hand looking on Figure 1, it is only the fine grid that includes the biology, thus my first thought was: Why do they have different initial fields for the nutrients. Of course they don't have that (?), and since the biology are the same (?), the question is: why do these high values appear. My answer would be that there must be a bug in the POLCOMS model, or that there are some physics that are resolved VERY different in NEMO and POLCOMS, but I guess the authors can add some more educated thoughts on this. The importance of physics to the biology are f.ex. discussed by Skogen & Moll Journal of Marine Systems (2005).

Secondly, when doing a validation a two year hindcast is not enough. The present ms. only show that the new model is able to reproduce the seasonal signal. I would like to see that the model is able to reproduce the inter annual variability. To do this at least 20 years are needed. To conclude, before publication the study should be extended to more years, and there should be more focus on why POLCOMS-ERSEM are so bad (is it a bug?). I have no doubt that the NEMO-ERSEM system is of high quality, but

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the present work is not enough to state that.

I have also some other points that should be taken into concern in a possible resubmission:

- Page 748. The authors mention some other systems overseas. Please also include European systems.
- Section 2.1.2: I guess the biological set-up are the same in both models. Please state that.
- Section 3.1: see commenst above. What is really happening in the winter nutrients in POLCOMS-ERSEM?
- page 757, line 5. This is of course correct, but since the level of silica is much higher than for phosphorous, it is not very interesting to compare the absolute bias.
- page 757, line 3, 18 and further on in the document. It is not very interesting to discuss correlations since all variables have a very strong seasonal cycle. As long as the model show any kind of a similar seasonal cycle, the correlation will be high. For this to make any sense the authors have to focus on anomalies (see second general comment above), and to do this two years are not sufficient.
- Section 3.2 and further on: Please stop using log<sub>10</sub> for the results. Yes, chlorophyll varies with several orders of magnitude, but log<sub>10</sub> is hiding all information on the level for the reader and should not be used.
- Page 760: The comparison including both buoys, satellite and model are very interesting. The authors are really pointing to some of the frustration that modelers meet: Which data can be used for model validation. I will encourage the authors to bring this work further, possibly in a seperate paper where they discuss in situ versus satellite data and limitations in their use.

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- Page 762, line 1: I think the main problem is within the physics. Please include results from O’Dea (2012)
- Page 763, line 27. Why use climatological river data. For the hindcast real data are available

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