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Interactive comment on "A skill assessment of the biogeochemical model REcoM2 coupled to the finite element sea-ice ocean model (FESOM 1.3)" *by* V. Schourup-Kristensen et al.

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n the following, reviewer comments are italicized.

General comments

This is a very interesting paper addressing the skill assessment of the newly coupled REcoM2-FESOM1.3 model. The coupling of a global biogeochemical model to a finite element ocean model is to my knowledge a scientific novelty and therefore of high scientific significance. The methods utilized for the skill assessment are valid and adequate and the assessment is carefully conducted. Given the new usage of a finite *element ocean mode it would be beneficial to explain in more detail how the finite element ocean model improves the skills of the model. Otherwise the results are presented in a comprehensive and clear manner. I support publication of this paper in GMD, subject to minor revisions.*

We would like to thank referee #2 for the review of our manuscript. We are happy that our model study is found to be of high scientific significance, and that the methods are found to be adequate and presented well.

We have added some text in the beginning of section "4 Discussion" (now "4 Discussion and outlook") in which we discuss how FESOM improves the skill of the biogeochemical model.

Specific comments

The article places its focus on the Southern Ocean since the coupled model FESOM-REcoM2 is meant to simulate biogeochemical processes in the Southern Ocean (p. 4168, line 22-24). The authors should explain why a global model, especially this model, is appropriate for usage in the Southern Ocean. Is the model MITgcm-REcoM1/2 performing especially well in the Southern Ocean? Is FESOM expected to perform especially well in the Southern Ocean? It would be advantageous if the Authors would describe the reason for the model′s focus on the Southern Ocean in more detail.

p. 4168, line 22-24: Sentence has been changed to "The coupled model FESOM-REcoM2 is, as a first step, planned to be used for studies regarding biogeochemical processes in the Southern Ocean south of 50 $^\circ$ S, and we are therefore especially interested in its performance here. The reasons for this focus are further discussed in section 4.". Section "4 Outlook and discussion" has been expanded to entail the reasoning.

The oceanic regions utilized for the model-observation comparison (as defined in Figure 3) don′t consider any region north of 70N even though there is data available (see

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Figures 5, 7, 8, and 11). Is there any specific reason for leaving this area out? Please add an explanation (I guess the data coverage is too sparse for the evaluation of seasonal cycles. Does that mean the data north of 70N is also left out of the Taylor diagram for spatial-seasonal distributions (Figure 4b)?)

It is correct that the region north of 70° has been omitted from our comparison of modelled basin-averaged net primary production with satellite data. This choice is mainly due to the satellite-based data becoming too scarce here to evaluate seasonalities. We have explained this in the caption for Figure 3: "due to the scarcity of the satellite-based value of NPP, the area north of 70° has been omitted", and we have likewise now mentioned this in section "4 Data and skill metrics". In this section we now also explain how missing data in the observations are handled for the Taylor diagrams.

The authors compare the model output against data products for NPP, EP, chlorophyll, MLD, and nutrients (Fe, DIN, DSi). Still it looks like different comparison-methods where used for different variables and it is not obvious why that is the case. I think the authors should explain why Fe is the only variable missing in the Taylor-diagram (Figure 4) and why there are comparisons of spatial distributions of MLD (Figure 5), DIN (Figure 7), DSI (Figure 8), chlorophyll (Figure 11), NPP (Figure 12), and EP (Figure 14), but not for Fe. Also seasonal cycles are only presented for MLD (Figure 6), and NPP (Figure 13). Probably these choices are dependent on the scarcity of the data, but it would be beneficial to add explanations for these choices.

Indeed, data availability was one issue, especially for iron, where so far a global climatology is not available, but just scattered observations. Of course, it would have been possible to make a comparison based on the in-situ observations, but this is quite a different thing than a comparison with the validated and smoothed climatologies available for temperature, salinity and the major nutrients. We therefore preferred not to include iron into the Taylor diagram, but rather compare only basin-aggregated values, which are subject to less noise. Another consideration was the amount of new information the plots give the reader (as the tracers are connected they are fairly similar), versus

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the length of the manuscript. We feel that the NPP is the most important result, and the seasonal cycle of this tracer is therefore presented. Additionally, MLD is a factor with a large impact on NPP, and we therefore additionally plotted its seasonal cycle. We agree that an explanation would improve the understanding, and have added this in section "2.4 Data and skill metrics", p. 4161, line 12 onwards.

The text shows very clearly that the availability of Fe-data is very low (globally and specifically in the Southern Ocean). The authors put a lot of effort in comparing the available Fe-data with the model. Even though the model reproduces the spatial distribution of iron reasonably well, its iron concentrations are clearly too low. The latter is discussed in detail, comprising different explanations and possible solutions of the problem. In order to complete the discussion it would be beneficial to see how good the model performs in comparison with other models in terms of iron concentration.
Unlike for the major nutrients, different models still differ greatly in their assumptions on the dominant processes affecting the iron cycle, and in their modelled iron distributions. So far, therefore, there is no universally accepted standard model against which to compare. Indeed, at the moment there is an iron model intercomparison underway that aims first at documenting how big the intermodel differences are, as a starting point for model convergence. Concurring with the suggestion of referee #1 to strengthen the comparison to other models, we have replaced p. 4163, lines 20 - 23 with a further discussion.

The discussion focuses mainly on the Fe-distribution as the main deficiency of the model. I think that should be clearly stated at the beginning of the discussion. Also the first part of the discussion doesn't follow the rest of the text and should be placed later, where the results of FESOM-Recom are discussed.
We have rewritten the discussion.

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Several passages claim that normalized standard deviations higher/lower than 1 (model output) indicate that the concentration gradients are too steep/low when compared to the observations. While this is in general true, it is not possible to confirm this finding with model-observation differences since the standard deviation is a measure with respect to the mean value. Therefore a confirmation with model-observation differences would only be possible when considering anomalies (with the mean value as reference value). Please correct these statements.

This is true, and something that we were not aware of. We have accordingly changed a number of sentences:

- p. 4162, line 26 now reads: "In the Southern Ocean, the surface DIN concentrations have a negative bias for DIN (Fig. 7) and a positive for DSi (Fig. 8) when the spatial distribution of modelled and observed values are compared."
- p. 4165, line 14-16 have been removed: "The normalized standard deviation, which is above 1 for the logarithm of both variables, indicates that especially the gradients with respect to chlorophyll a are too steep".
- p. 4168, lines 4-6 have been removed: "showing that the spatial gradients in the model are smaller than both those from Schlitzer et al. (2002) and Laws et al. (2000), but closer to the former."

When referring to the NPP data product, the authors refer to Behrenfeld and Falkowski, 1997. To my knowledge, Behrenfeld and Falkowski did not provide a NPP data product, but a model (vertically Generalized Production Model – VGPM) for the estimation of NPP. The latter relies on different variables which can be provided for example by a satellite-product such as SeaWiFs. In order to be accurate it would be good to refer to the data as "Net Primary Production estimated with SeaWIFS chlorophyll and the

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VGPM-model" or something similar. The authors should also clarify if they calculated the data-product themselves or retrieved it from a research group or a website. The same holds probably for EP.

p. 5160, line 24: The following sentence has been added to clarify which NPP product we used and how we acquired it:

"For the net primary production (NPP), we use the VGPM-product from the ocean productivity webpage (http://www.science.oregonstate.edu/ocean.productivity/index.php), which is based on the SeaWIFS chlorophyll measurements and the VGPM NPP-model (Behrenfeld and Falkowski, 1997). We have downloaded monthly values from the webpage, and from these calculated the spatial and seasonal means."

We have additionally added "SeaWIFS" as well as the address of the ocean productivity webpage to Table 1, and we have added "SeaWIFS" to all references to Behrenfeld and Falkowski (1997).

For export production we have likewise added where the data was retrieved.

p. 4162, line 7: the MLD-criterion of de Boyer Montegut is cited incorrectly. The mixed layer depth is defined as the ***first*** *depth at which the difference between the potential density at 10m depth and* ***the potential density at deeper lying reference levels is greater than*** *0.03 kg m-3.*

This is correct, both for the data and the model. We have changed the text accordingly.

p. 4165, line 9: please mention the values provided by Schneider et al.

Sentence has been changed to: "It is however higher than the modelled values ranging from 23.7 to 30.7 Pg C yr^{-1} reported by Schneider et al. (2008)."

p. 4166, line 46: the text claims that the seasonal cycle is closest to the satellite-based estimate between 10-45N and S. Figure 13 indicates that this statement is not correct for the North Indian region. Please correct the statement accordingly.

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Sentence added to p. 4166, line 28: "This is the case for all basins in the mentioned area, except the North Indian basin."

Table 3: The table should be expanded to be more consistent with the text. Please include the values of Behrenfeld and Falkowski 1997 as well as Schneider 2008 for global NPP, Siegel 2014 for global EP, and Carr 2006 for NPP in the Southern Ocean.
We have added the global NPP from SeaWIFS/Behrenfeld and Falkowski (1997) and the range in Schneider et al. (2008). For global EP we have added the value from Siegel et al. (2014). The Southern Ocean values from Carr et al. (2006) are already in the table.

Conclusions: When declaring that the modeled spatial fields are on average better in the Southern Ocean than on the global scale, the authors should state which variables are performing better (according to Figure 15 this seems to be the case for DIN and Si) and which variables are performing worse (according to Figure 15 this seems to be the case for MLD and Chl).
The conclusion has been rewritten.

Technical comments

Figures 4, 6 and 13: Please enlarge the labeling.
Has been done.

Figures 10, 17 and 18: please mention in the figure captions that the illustrated variables are modeled values.
Has been done.

-p. 4156, line 16: please add "for explicit scientific studies" or something similar.

"for scientific studies" has been added to the sentence.

- p. 4157, line 18: replace "grid points" with "levels" or "layers".
Sentence has been changed to: "In the vertical it has 32 layers, of which nine are located in the upper 100 m.

- p. 4157, lines 21-22: please add references to the terms "Redi diffusion" and "Gent and McWilliams parameterization".
References have been added.

-p. 4159, line 26: Please add "an" after "gives"
"an" has been added.

-p. 4167, line 14: please confine statement to "The EP of the model".
Sentence has been changed to "The modelled EP constitutes...".

-p. 4168, line 12: please consider replacing "the differences between the fields are especially clear" with "the differences between the fields are especially visible".
"clear has been replaced with "visible".

-p. 4171, line 24: please correct the sentence to "We will now examine the roles of MLD and iron concentration in explaining the seasonal variability of NPP".
Sentence has been changed.

Interactive comment on Geosci. Model Dev. Discuss., 7, 4153, 2014.