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Geoscientific Model Development Discussions

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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In the following, reviewer comments are italicized.

1 Overall comments

Schourup-Kristensen et al present a skill assessment of the finite element global ocean model FESOM coupled with the biogeochemical model REcoM2. They focus in particular to the Southern Ocean. They compare results to a number of datasets but no formal comparisons to other model results.

Overall the paper contains some excellent results and the model shows reasonable skill at the global skill. The authors acknowledge the weaknesses in the model and which variables are not adequately represented. The paper is suitable for publication in GMD and to my knowledge is the first global FE-biogeochemical model to date. This represents a significant result.

I have a few suggestions to hopefully improve the manuscript.

We would like to thank referee #1 for taking the time to carefully read and comment on our manuscript. We have addressed the referee's suggestions for improvements as described below.

2 Suggestions

The authors claim to compare their results to other models. Whilst this is true in that they cite how their results compare to other models in the literature, there is no formal comparison to other models, not any graphical comparison within this paper (i.e. replotting results to the same scale as used in previous studies). I think they should strengthen the comparison to other models - it's vital to put their results in context.

We agree that the comparison of our results to other models could be strengthened. We had actually already drawn several of the key figures using identical scales as those used in preious studies, but without explicitly mentioning the comparison. We have therefore added a text describing what models we compare our results to in section "2.4 Data and skill metrics".

In addition, we have made the following changes to make the comparison clearer:

- Figure 9, "Plotted to the scale used by Schneider et al. (2008)." has been added

to caption.

- p. 4163, lines 20 - 23 have been replaced by: "The pattern of the surface iron concentration in the Atlantic Ocean (Fig. 9) fits well with the results from the MPI and NCAR models in Schneider et al. (2008), with relative high concentrations in the Equatorial region fed by the dust plume from the Sahara, and concentrations decreasing towards the poles. The iron concentrations in the IPSL model and Assmann et al. (2010) are somewhat lower in the equatorial and southern part of the Atlantic Ocean than our result. Due to the relatively high coastal input of iron from the sediment in the IPSL model, it has a relatively high coastal iron concentrations globally. In the Indian Ocean, our surface iron concentrations agree well with the IPSL and NCAR models as well as with the results from Yool et al. (2011), with values higher than 1 nM in the Arabian Sea and falling towards 0.3 nM in the main Indian Ocean. Our values also fit well with the observations in the North Indian Ocean (Table 2), and this, along with the agreement between the models using varying magnitudes of sedimentary iron input, indicates that the coastal upwelling in the Arabian Sea is well captured in these models, and that this upwelling is responsible for the high surface iron concentrations in the area. The lower surface iron concentration in South East Asia is on the other hand evident in all of these models with the exception of the IPSL model, indicating that the sediment source plays a larger role in this area. Here, unfortunately we do not have observations to validate the models. In the Pacific Ocean, our result is closest to the one from Assmann et al. (2010), though they have a higher iron concentration along the North and South American west coast, indicating a stronger coastal upwelling in their ocean model. We have a lower surface iron concentration than all models presented by Schneider et al. (2008), even though they all have low concentrations locally. The observations in Table 2 indicate that all models underestimate the surface iron concentrations in the Pacific, especially in the equatorial region where the upwelling plays the largest role. In the Southern

Ocean, the observations in Table 2 show that our surface iron concentration is too low. In this region, our result agrees the best with the IPSL model, with a relatively high coastal concentration, which then falls towards the north. Both the MPI and NCAR models have relatively constant higher values in the area south of 40^o S of around 0.5 and 0.3 nM respectively."

- p, 4164, lines 24 - 26 changed to: "Nanophytoplankton is mainly nitrogen limited in the Atlantic and Indian Ocean, concurring with the result by Assmann et al. (2010), Yool et al. (2011) and the IPSL model in Schneider et al. (2008). For diatoms, silicon is limiting in the Atlantic and Indian Oceans as well as the Arctic, a feature that we only share with Yool et al. (2011)".

- Figure 11, "Plotted to the scale used by Schneider et al. (2008)." has been added to caption.

- p. 4165, line 20, text added: "Yool et al. (2011) similarly have a higher equatorial chlorophyll a concentration in the equatorial regions of the Atlantic and Pacific Oceans as compared to our model, but their concentration in the Southern Ocean is even higher than ours. And when we compare to the IPSL-model (Schneider et al, 2008), we again see that our model underestimates the equatorial chlorophyll a concentrations, whereas the concentrations in the North Atlantic and Southern Ocean is fairly similar to our result."

- Figure 12, "Plotted to the scale used by Schneider et al. (2008)." has been added to caption.

- p. 4166, line 23, text added: "When comparing the mean spatial distribution of NPP with other models, our result is the closest to the NCAR-model presented by Schneider et al. (2008), with a relative high production rate in the North and Equatorial Atlantic as well as the Indian Ocean. Moderate production in the area of the Polar Front in the Southern Ocean is a feature that our model shares with

the satellite-based estimate (Fig. 12) but also both the NCAR and IPSL models from Schneider et al. (2008). In the Equatorial Pacific and Northern Pacific, our results also fits well with the IPSL model, through we have a smaller production rate in the Southern Pacific."

- Figure 14, "plotted to the scale used by Moore et al. (2004)." has been added to the caption.
- p. 4168, line 17, sentence added: "Both the spatial distribution and magnitude of EP in our model is very similar to what was found by Moore et al. (2004)".
- Figure 18, "plotted to the scale used by Moore et al. (2004)." has been added to the caption.

The writing is disjointed, with many one sentence paragraphs throughout, particularly in the results and discussion sections: e.g. "In the Southern Ocean, the spatial distribution of iron in the model is reasonable, but it tends towards low values (Table 2)." This makes it difficult to read and place the results in context of the literature they cite.
We have read the paper again thoroughly and made many small changes to enhace the legibility and to make the larger context clear. Some examples are:

- p. 4162, lines 6-7, merged to "The seasonal variability of the MLD leads to entrainment of water with high nutrient concentrations to the surface water during winter, and the maximum depth of the mixed layer during the year (MLD_{max}) is therefore especially important from a biological point of view."
- p. 4165, lines 5-9 merged to: "...and also slightly below the estimate range of 35 to 70 Pg C yr^{-1} given by Carr et al. (2006), but higher than the modelled values ranging from 23.7 to 30.7 Pg C yr^{-1} reported by Schneider et al. (2008)."

- p. 4166, lines 3-6 merged to: "...which may not be sufficient for the upwelling zones to be resolved properly, additionally the low resolution further out in the subtropical gyres could play a role."

A more thorough description of the FESOM model is required in section 2. Which function space is used for these simulations? What timestepping method and what was dt? I don't think the current description, which does cite relevant literature, is not sufficient for a reader not familiar with FESOM to evaluate the output fully.
Section "2.1 Ocean model" has been expanded to describe the function space and the time-stepping method.
The size of the time step has been added in section "2.3 Model experiment", p. 4160, line 5-6.

Which bathymetry data were used in this study? Which coastline data was used?
This information has been added to section "2.1 Ocean model".

Although the authors correctly indicate the strength of unstructured mesh ocean models, they have not used the capabilities here. I think this needs to be explained. I assume this is the first step in a larger project where the unstructured mesh will play a more important part.
It is correct that we have not fully used FESOM's capabilities in terms of local high resolution in the current run, with gridpoint distances verying only between 15 and 180 km. We mentioned this in discussion (p. 4176, lines 12 - 17), but did not further elaborate on this before.
We have now changed the title of section "4 Discussion" to "4 Discussion and outlook" and have added a discussion of future plans and possibilities at the beginning of the section.

Minor corrections

Equation A1 - SMS(C) should that be C_{SMS}?
p. 4177, line 24: C_{SMS} has been changed to SMS(C).

Appendix A: "...tracers than in a model based in..." based on?.
Yes, "in" has been changed to "on".

Appendix A: I'm not sure why a quota approach means you need more tracers?
A quota approach means that nutrient or light limitation of phytoplankton growth depends on internal cellular nutrient/carbon or chlorophyll/carbon ratio; This requires that biomass is separated into carbon/nitrogen/chlorophyll ... pools (instead of suing constant ratios as is often done). The drawback is that more tracers are introduced. This is briefly described at the end of the first section in the Appendix.

Figure 4: Font is far too small.
Font has been enlarged.

Figure 6,13: Fonts for the axes and correlation coefficients are very small.
Fonts have been enlarged.

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