

2nd Review of “FESOM2.1-REcoM3-MEDUSA2: an ocean-sea ice-biogeochemistry model coupled to a sediment model”

by

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Summary:

Ying Ye and colleagues resubmitted a revised version of their manuscript with significant changes to address the comments of both reviewers. Most notably, the authors changed the degradation rate constant for the low C:N OM class in MEDUSA2, they performed mass corrections to account for the asynchronous coupling of the models and the loss of nitrogen through denitrification in sediments, three new subsections were added to the Results (3.2.2 – 3.2.4, briefly describing MEDUSA2 results at the end of coupled simulation), and Figure 3 & Table 1 (summarizing their globally integrated results) has been revised and improved – even though problems still exist with the reported opal fluxes (see comment #1.3. I really appreciate the improved Fig. 5.

While the authors have made substantial improvements to the manuscript, there are still significant weaknesses that need to be addressed (some of which were raised in the previous reviews but have not been addressed in a satisfying manner). In my view, most importantly, it would be beneficial to include more and better “tuning” experiments (e.g., to enhance results for modern OM preservation + at least one idealized transient simulation). This will help to convince readers of the model framework's appropriateness to simulate not only appropriate steady-state modern and LGM conditions but also transient Earth system dynamics, as intended by the authors in future studies. Having a model configuration that simulates modern marine conditions well should be the minimum goal for such a *GMD* paper, especially considering that this configuration will be the reference for future studies!

Therefore, I still cannot support the publication of the manuscript in *Geoscientific Model Development*. I summarize my main concerns below and I hope that my general comments will help to improve the manuscript further.

General comments:

Comment #1.1: Improve the simulation patterns of preservation in the sediments:

The poor model-data fit in Fig. 6 can not only be explained by the lower resolution. As stated in the previous review: “Previous models with similar or even coarser resolution are able to simulate POC (and calcite) settling fluxes and preservation on the shelves much better (e.g., Palastanga et al., 2011; Hülse et al., 2018; Ridgwell & Hargreaves, 2007).”

Taking OM as an example, I think there are multiple reasons for why the wt% patterns are not very realistic (that could/should be addressed):

1. The settling fluxes at high latitudes are (potentially) too large (comparing your Fig. 4a with Fig. 5a of Dunne et al. – note, that their color scale is very different), whereas the global total seems to be way too small (see Tab. 1). So the pelagic POC degradation should be improved first as this will influence the OM available for benthic preservation.
2. OM is not further oxidized when nitrate is exhausted – this is not correct and will cause too much OM preservation in these grid-cells: Judging by Fig. 7 (right, or Fig. 4 in replies to reviewers), NO₃ is zero in a large fraction of higher latitude cells → in these grid-cells too much OM is preserved (see Fig. 6)
3. Better tuning of the OM fractions and degradation rate constants in MEDUSA2 will also help. (For the revision the authors simply increased one degradation rate constant by a factor of 5 and 10.)

Instead, rate constants could depend on seafloor depth, sedimentation rate or OM settling flux (see e.g., Boudreau, Springer Berlin, 1997). This would potentially not only improve the model-data fit but also responds to changing environmental conditions when simulating paleo-conditions.

Also, as the pelagic model only represents one OM fraction: How do you specify the two OM fractions in MEDUSA2 from this? I don't think this is discussed in the manuscript.

The results, of course, don't need to be perfect but should be better than presented in Fig. 6. And considering a run time of "2-3 weeks for 1000 model years" a few more experiments to improve the configuration are reasonable.

Comment #1.2:

The global burial fluxes of POC (110 PgC kyr⁻¹) and calcite (115 PgC kyr⁻¹) look good (see Table 1). Please check the highest POC data estimate in Tab. 1 (i.e., 2600 PgC kyr⁻¹) is this not a settling flux?

Comment #1.3: There seems to be something wrong with the opal fluxes:

How can the opal settling flux in areas >1km (79-84 Pmol Si kyr⁻¹) be larger than the global flux (22-40 Pmol Si kyr⁻¹) in Table 1? Also the units are different in the text (Tmol Si yr⁻¹) and the Tab. 1 (Pmol Si kyr⁻¹). And why is the global opal burial flux even larger (82 Pmol Si kyr⁻¹). And compared to this the observed global burial is tiny (7.1 Pmol Si kyr⁻¹).

Comment #2:

Figure 6: The new color-bars for the model results (right) are finer than the color-bar for the observations (left). Therefore, model and observations cannot properly be compared as more features appear in the model results than in the observations (this was introduced to address one of the comments of reviewer #1).

Comment #3:

A transient experiment or paleo-application. This would be highly informative and was suggested by both reviewers (e.g., 1st comment of Reviewer #1) but has not been addressed. At least the model could be used to simulate an idealised perturbation experiment to showcase that it can be used for transient applications and that the sediment properties respond. In particular, because this is mentioned as one of the main motivations for configuring this model.

Comment #4:

I was also not very convinced by the authors reasoning for why they did not include some output of C-isotopes. This was also suggested by both reviewers and is highly relevant for a model that will be applied to paleo-applications.

Comment #5:

Why a mass correction to account for the asynchronous coupling is necessary is unclear to me. Please explain and justify this better in the text. My understanding is, that while this might affect the inventories & fluxes during the spin-up phase the system should eventually come to a (new) steady-state.

Comment #6:

The higher spatial resolution result (R_{high}): I understand, that this was included to address parts of the 2nd comment of Reviewer #2. However, these results are rather out of place here. Also the authors argument that this experiment shows that the coarse model resolution of R_{coupled} is mainly responsible for the low settling fluxes (lines 316 – 320) is at least insufficient: It is unclear if model resolution is

the only boundary condition/parameter that changes between both configurations. Anyhow, there are multiple ways to increase the POC settling fluxes in the lower resolution R_{coupled} configuration (e.g., by increasing the low export production as acknowledged by the authors, or decreasing pelagic POC remineralization rates).

Comment #7:

Fig. 3: I suggest including the profiles at the end of R_{coupled} here as well because these are the new results presented and tested in the manuscript.

A few minor comments:

Fig. 6: What is the simulated wt% here? Is it mean over the bioturbated layer, at 10cm, or something else? And what are the corresponding values of Hayes et al. (2021) representing?

Fig. 7: I find it not very intuitive why you show R_{NO_3} here. Can you please motivate this. If sulfate reduction would be included in the model (more important than Mn, or Fe-reduction on a global scale) then one could nicely show the different fractions.

Fig. C1: Why is there such a large drop in the calcite burial flux even though the calcite settling flux increases over the experiment?