

Interactive comment on “Ecological ReGional Ocean Model with vertically resolved sediments (ERGOM SED 1.0): Coupling benthic and pelagic biogeochemistry of the south-western Baltic Sea” by Hagen Radtke et al.

Anonymous Referee #3

Received and published: 19 June 2018

The authors proposed a coupled framework for vertically resolved pelagic and benthic models. This framework is built from two existing models: ERGOM (Neumann et al. 2000) for the pelagic part, and the diagenetic model of Reed et al. 2011 for the diagenetic part. Besides the coupling framework, the benthic model has undergone several developments. The model is applied on 7 stations of distinct environments in the south western Baltic Sea (mud,sands,silts) and shows the ability to reproduce most of benthic observations.

The manuscript is clear, well illustrated and well written in general but lacks the

C1

rigorous mathematical description one could expect from a model description manuscript. I understand the reasons for, and support, a large qualitative description that helps in getting a quick grab at which processes the model considers and which it discards, but this should be complemented by descriptive equations, eg. in appendixes. The reference to the user manual is not satisfactory, since it only contains automatically generated code, hard to read. In addition to this, I suggest major revision also to enhance the justification of the model developments presented here : enhancement of the benthic model, B-P coupling framework.

General Comments

* In the introduction and conclusion, the emphasis while presenting the ERGOMSED model is put on the online coupling between the Benthic and Pelagic part. However this online coupling is not valorised in the results and discussion section. This should be enhanced. Neither the pelagic part nor the solutes exchanges between the benthic and pelagic part are mentioned in the results, although the conclusion states that “In the long term, biogeochemical ocean models should aim at a process-resolving description of surface sediments. This is especially true for shallow ocean areas where the efflux of nutrients from the sediment strongly influences water column biogeochemistry, like in our study area.” In the case that benthic fluxes, for any reason, are not available within SECOS (which would be surprising), ranges from the literature could be used for comparison, and it would also be relevant to compare benthic fluxes to the lateral fluxes (inferred from the nudging procedure for the pelagic nutrients) to stress the relevance of such coupled framework.

* The fluff layer approach is an interesting feature of the coupling set-up, and a practical solution to handle solids lateral transport and exchanges between benthic and pelagic part. To my knowledge, the use of a fluff layer is not systematic in B-P coupled models and I would have found relevant to enforce introduction and discussion on this aspect.

* Appendix B supposedly justifies the inclusion of enhanced dynamics in the benthic

C2

model. This should be more developed. In particular: 1) Has the same calibration procedure been applied "from scratch" after having switched off those processes; 2) Some of the "reduced" experiments actually seems to behave better than the reference simulation. Can the authors justify their modification in this context ?

Specific Comments

* P3L13 : I suggest to add a references on ecosystem services (for instance : https://cices.eu/content/uploads/sites/8/2012/07/CICES-V43_Revised-Final_Report_29012013.pdf)

* From P9L17-19 and P10L10-15 I had understood that solid compounds were only transferred from the distinct fluff and upper sediment layers through bioturbation (which includes here also other mixing effects). However, at P11L29 we learn that accumulation (advection) also induces a transfer from the fluff to the sediments. This may be introduced earlier (eg. end of Sect. 2.3.3) and explained in more details.

* Tab 1. : Benthic tracers (both solids and dissolved) are defined in mol/m² which doesn't correspond with the definitions given in P10L6 and P10L25. Is there a general transformation applied to get those in units of mol/volume of liquids/solids ?

* P10L10-20 : Bioturbation rate are defined for the sediment compartment. Is the uppermost value used for diffusion between the fluff layer and the uppermost sediment cell ? please precise.

* Sect 2.4: Interactions between phosphate and iron aren't described for the water column. Are t_{ipw} , t_{ihw} and t_{mow} only included to enable a lateral transport of resuspended solids, or is there possible biogeochemical transformation in the water column ?

*P24L3 : In general, it might be relevant to comment which parameters were considered for the calibration and which were adapted, which were considered as equals for all stations and which were considered to differ between stations.

C3

* P24L15: It is not clear whether Δ_i is defined specific only to each variables, or specific also to each station, or also to each sampling depth. This is relevant as referred to when discussing model performances.

* P26L3 : Should the first "bioirrigation" be replaced by "bioturbation" ?

* Sect 5.12 : As is true for numerous model of this type, application in sandy sediments might be limited to the the lack of consideration of processes specific to permeable sediments. "Whashout" is mentioned in Sect 2.3.3, but this isn't the only aspect of it. This should be discussed. For instance in this section. You might refer to the review from Huettel et al, 2013.

* P30L2-3 : Switch "higher" and "lower".

* P30L19 : I don't see a TOC maximum at the top of sediments for station DS. Concerning this last paragraph, you could maybe consider the fact that the inability of the model to provide a TOC profile increasing with depth is related to the absence of dynamics specific to permeable sediments , washout in particular ? I insist on this point since it represents a major challenge for BP coupling intended to be implemented on shelves with mixed sand/mud conditions. I don't ask that this be solved in this study, but the issue should be commented.

* Table A1 : t_{h2s} has one H and 2 S ? Is that an error in table or in the model ?

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-109>, 2018.

C4