

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 #2

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Hagen Radtke and co-authors present a 1-D benthic-pelagic ecosystem/biogeochemical model for coastal systems which is created by coupling the marine ecosystem model ERGOM to an early diagenetic model (Reed et al., 2011). In addition, some further model developments have been made to the sediment model. Most biogeochemical models currently either focus on the water column or the sediments. Therefore, a numerical representation suitable to study coupled processes between bottom waters and the surface sediments is a very useful tool. However, there are a number of weaknesses/issues in the manuscript to be resolved before

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publication.

General comments:

1. Introduction and text structure: The introduction needs to be improved/restructured, better putting the model/work into context. Referring to another paper (i.e. Yakushev et al., 2017) for an overview of existing coupled models (pg. 2 In. 13-14) is not sufficient. This part of the manuscript is critical for putting your work into context and for the motivation of your work! Why did you decide to use a new model? How does your model differ from those? Why is it better suited to your study site? I think, especially the explicit fluff layer (which is also known as the bottom boundary layer, I suppose – or are they different things?) deserves some more detail. Also the introduction should be restructured, e.g. starting with a better introduction of the importance of coupled benthic-pelagic processes and linking this to your site of interest (i.e. the German part of the Baltic Sea). This should motivate why the modelling exercise is needed (especially why using a coupled model and not just running a stand-alone sediment model if you are mainly interested in “the type of ecosystem services that coastal sediments can perform”). How can your new model help to improve our understanding of this environment (e.g. what are the most important processes here)? Also the results/conclusion section does not address these kind of questions. In general, the results section is rather short and a “story” behind your experiments or what you learn from them is missing. As this model is mainly developed to investigate benthic-pelagic interactions a discussion of simulated sediment-water interface fluxes and its comparison with observations would improve the validation of the model. Especially, as the conclusion states “... where the efflux of nutrients from the sediment strongly influences water column biogeochemistry, like in our study area.”

It was not clear to me what questions you would like to answer with the model eventually? This could be included in a section on “Scope of applicability and model limitations” which is expected for a model development paper anyway and is currently missing.

2. The technical details of the implementation are incomplete: Just describing the processes/state variables qualitatively is not appropriate for a GMD paper which is supposed to be "detailed and complete". Include the most important equations and tables of parameters for e.g. (but not exclusively) the biogeochemical processes (2.4 + 2.5) in the main manuscript (e.g. compare ERSEM: Butenschön et al., 2016; PISCES: Aumont et al., 2015). Give values of parameters and references to justify your decision making. I know they are in the supplementary document but it is 189 pages long, therefore it is not easy to find what one is looking for and the very technical parameter names do not help either. I suggest not to use the code-names for the parameters in the text and equations. Give them more recognizable names and add Tables in the appendix which relates them to the code-names - if you wish (compare e.g. Aumont et al., 2015). For the diagenetic model: Add the 1-D mass conservation (general reaction-transport) equation and add table of reaction network for primary and secondary redox reactions and for precipitation/dissolution reactions (compare e.g. Reed et al., 2011; Jourabchi et al., 2005; Hülse et al., 2018).

The diffusivity (Section 2.3.1) and the initial and boundary conditions (Section 4.2) are from an unpublished MOM5 and ERGOM runs. More information on the model setups is needed as it is not possible to reproduce your results like this. This could go into the supplementary information together with the results needed to recreate your ERGOM SED results.

3. Model development: Make it more clear in the body of the manuscript what your model improvements are and describe them in more detail. Apart from abstract and conclusion this is not clearly mentioned and the explanation of the new developments are generally very short.

The coupling of the different modules (i.e. water column, fluff-layer, sediment) is obviously a new development as well but it is not clear to me how it is done. E.g. how do you calculate the bidirectional fluxes of dissolved species (give equations). Or the coupling of the fluff layer with sediment-column: You assume zero porosity? How are

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solute species transported from the fluff to the sediments?

4. Text structure and referencing: The manuscript could benefit from another round of editing, looking at the structure and referencing (technique and missing citations). Examples of wrong referencing can be found: pg. 1 ln.7 ; pg. 2 ln. 5 + 15; pg. 10 ln. 15; pg. 22 ln. 5; pg. 27 ln. 1-2; etc. The breakdown of the text into paragraphs and linebreaks should be revisited. Linebreaks after just 1 or 2 sentences are often used (e.g. 2.3.7, 2.3.8, 2.3.9, 2.4.5, 2.5.2, 2.5.5 etc.) which does not help with the flow of the paper. Also some of the crosslinks given to other sections of the text are not correct (see specific comments).

Specific/technical comments:

Abstract: Abstract should include some information about the main findings/how the model performs.

ln. 11: what does SECOS stand for?

pg. 2 ln.8-9: Why are there so few coupled benthic-pelagic model studies?

pg. 3 ln. 16 – 20. You introduce here the seven study sites you are modelling and categorize them by their granulometric properties. Some information on how these three categories differ and what these differences mean for modelling the sites would be good.

pg. 5 2.1 Ancestor models + 2.2 Model compartments and state variables: A better short summary of the main, specific features of the ancestor models is needed for the reader to understand the model setup. Both sections could be combined. Then make it more clear would be the improvements done here to the sediment model.

pg. 5 footnote: Mention that these input files can be found in the specific stations folder. It took me a while to find it.

pg. 6 ln. 1-3: Does that mean, porosity is always constant in the sediment column?

Does the model also work with varying porosity?

pg. 6 ln. 10 – 15: The alkalinity description is unmotivated and too technical. It would be more clear if you describe in a sentence or two how alkalinity is calculated and add the change in parameters in parenthesis. Also, I can't find a clear explanation for the reasoning of the approach in Section 2.6 as promised here (pg. 6, ln. 15).

pg. 6 Section 2.3 Transport processes: There are a lot of often very short subsections. I would propose to have just 2 subsections: 2.3.1 Ocean 2.3.2 Fluff layer/Sediment. Then using different paragraphs for different processes to increase readability.

pg. 6 ln. 25: "... lateral transport processes have a major impact." please give reference

pg. 6 ln. 29-30 relax wintertime nutrients in the surface layer: Is this approach adopted from somewhere (Ref)? Does this lead to realistic results?

pg. 7 ln. 2: The parameterisation of lateral transport is discussed in this section (see 2.3.9 Parameterisation of lateral transport)

pg. 7 ln. 6-9: more information needed for unpublished model run. KPP used without explanation

pg. 9 Particle sinking: Is there a reference for the different sinking speeds?

pg. 9 ln. 14: replace "from" with "as"

pg. 9 ln. 23: where does the rate of bioerosion come from? Ref?

pg. 10 ln. 10-15: coupling of fluff and sediments unclear Also why 3mm? What is a typical thickness, what influences it?

pg. 10 ln. 16-20: reference for the approach? Also, the oxygenation of the sediment column affects the depth/rate of bioturbation! Is this not represented in the model? I.e. is bioturbation possible even in the sulfidic zone? If you just fit your model to a specific site you probably change this manually but what are you doing when coupled to a 3D

ocean model?

pg. 11 ln. 6-9: Give equation for the exchange flux. Again the 3mm here...

pg. 11 ln. 22: replace “sediments” with settles or is deposited. I assume the sediment accumulation is taken as advective transport in the diagenetic model!? Clarify in the text.

pg.12 ln. 6-11: why transport away/towards different sites? explain/justify

pg. 12 Biogeochemical processes in the water column: add references for previously published ERGOM version you mean (ln. 16) and the equations for the processes described in the following subsections (2.4.1 – 2.4.5) with tables of parameters and their values as used in the model equations (see general comments).

pg. 13 ln. 18: rewrite to “from previous ERGOM versions”. I find the use of the term detritus here confusing as you use organic carbon/material in the rest of your manuscript - Particulate organic carbon (POC) might be a better choice

pg. 13 ln. 19-23 / 24-26: The decay rate constants and the partitioning into reactivity-classes are probably the most important steps for the model output (e.g. Arndt et al., 2013, Hülse et al., 2018). Therefore, this deserves some more words and justification.

pg. 13 decay rate constants: where do the 0-degree values come from? - are they representative for your study area? - how do you know? - and how is it temperature dependent? Give equation!

pg. 13 Partitioning: Is the Middelburg approach not for OM at the sediment-water interface? Could you please clarify and give the equation used to calculate the fractions. Also what is the fraction of the non-decaying detrital?

pg.15 ln. 25-26: Give equation for conversion from SO_4 to H_2S depending on the diffusive CH_4 flux from below the model domain

pg. 16 ln. 1: you DO favour the latter theory because your rate constants are indepen-

dent of the TEA. Or am I wrong?

pg. 16 ln. 1-2: You say: "...we chose to adopt the decay rates proposed by Middelburg (1989), which may implicitly take the effect of the oxidant into account." I do not understand this statement. If I remember it right, the Middelburg (1989) rate model is the same for oxic and anoxic conditions. Also what are the values of your degradation rate constants? They are the most important parameters in the diagenetic model. List them e.g. in a table, together with the rate constants for the water column and the fractions for the OM partitioning.

pg. 16 ln. 3: change "their study". It is just one author.

pg. 16 ln. 4-7 preferential release of P: under which conditions is P preferentially released? Is this important for your study area? As you say on page 13 ln. 16: "anoxic conditions which, however, do not occur in our study area."

pg. 16 t_detp_n :refer to table A1. How do you get the t_detp_n and t_det_n numbers for H? I understand that detritus is 50% enriched in C and P and how the values for C, N, O, P are calculated. But H does not add up. Should H for t_det_n & t_sed_n not be 22.875?; pref_remin_p: what's the value and where does it come from?

pg. 16 2.5.2 Specific mineralisation processes: Add table for reaction network as mentioned in general comments

pg. 16 ln. 17-20: The description is very vague - list the station specific content e.g. in table 2 and what is the "small amount of reducible iron"? Quantify!

pg. 16 ln. 24-25: reference for statement

pg. 17 2.5.4 Pyrite formation: you did some model development here: add equations to make it more clear

pg. 18 ln. 8-9: state the formula

pg. 18 2.5.6 Reoxidation of reduced substances: There is a lot of information about

the reaction network here. A summary of all that in a table would be very helpful! Also just use either iron-II/III or Fe-II/III, don't mix them up

pg. 19 2.6 Carbon cycle: this is also a new model development – at least include the equations you use to calculate pH and pCO₂.

pg. 20 In. 4 mode splitting method: give some quick background what this is and a reference

pg. 21 In. 7: style. Change to: ... in the Southern Baltic Sea (see Fig. 1, we always present the stations from west to east).

pg. 21 In. 9: units of salinity of 20 is missing pg. 21 In. 21: replace “interface” with interfaces

pg. 22 In. 14: replace “So” with thus

pg. 22 In. 22: replace “sampled” with samples

pg. 23 4.2 Initial and boundary conditions: ERGOM model: more information needed for the unpublished model run as stated in general comments Is the relaxation approach of DIN and DIP adopted from somewhere? Does this lead to realistic results?

pg. 24+25 4.3.2 Optimisation strategies: Why do you talk about the application of the AHR-ES algorithm which is in the end not used at all? Delete this part. pg. 25 In. 19 - end: What parameters are changed? What is the range they are varied in (add table to appendix)? Why don't you show any results of the optimisation? Are the final parameter values realistic, e.g. compared with other models or data? What are the most important parameters?

pg. 26 4.4 Manual correction of sand and silt station: How exactly did you modify the parameters? This is needed for reproducibility of your results!

pg. 26 In. 6: which detritus is meant here (just POC or POC with mineral particles)? - rephrase “out of the sediments”

pg. 26 - 5.1.1 Pore water profiles at mud stations: It should be easy to check with the data if the variability at site AB is because they are 23km apart.

pg. 30 ln. 1 phosphate is in the right panel

pg. 31. ln. 3-4: reference for this statement

pg. 31 ln. 6: rephrase

pg. 33 Conclusion: ignoring the N-cycle because it's not part of the SECOS project: This is a rather poor justification, especially as you say later: "... where the efflux of nutrients from the sediment strongly influences water column biogeochemistry, like in our study area." and "... denitrification [...] may strongly influence marine ecosystems".

pg. 37 Table A1: stoichiometry of t_{H_2S} is wrong: it should have 2^*H and 1^*S

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