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Geoscientific Model Development Discussions

Interactive comment on "Model of Early Diagenesis in the Upper Sediment with Adaptable complexity – MEDUSA (v. 2): a time-dependent biogeochemical sediment module for Earth System Models, process analysis and teaching" *by* Guy Munhoven

Anonymous Referee #1

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Guy Munhoven presents a revised version of his time-dependent vertically resolved biogeochemical model of early diagenesis (MEDUSA) described in an excellent model development paper. In particular the model code has been thoroughly revised to create a more flexible simulation environment for reaction transport calculations that can be specified by the user and thus easily tailored for specific applications. A code generator has been developed that produces the respective parts of the source code and a complete reference guide is provided as a supplement to the manuscript. The new model

code facilitates the coupling to common grid layouts of ocean biogeochemical models and the coupling procedure is explained and exemplified in additional supplementary documents. The numerical efficiency of MEDUSA-v2 allows for its coupling to Earth system models and the execution time can be further reduced via parallel processing of multiple sediment-columns.

Overall, the revised model represents a very useful and important tool for a multitude of applications. The model description is very thorough and precise, including many excellent documents as a supplement. I only have a few minor comments about the description of the solution strategy and a few technical comments the author should address before publication.

2.2.2 Solution strategy

The different initial conditions for the iteration scheme are excellent and clear. However, I think it would be good if the convergence criteria is made more clear in the main manuscript. From your "Technical Reference" I understand that you test for a convergence of the (solid) concentration profiles. The second criterion, however, is difficult to comprehend. I think it would be good to include a clear description of both criteria in the main manuscript. Also, is the overall solution divided into different steps? E.g. is OM degradation and the resulting profile calculated first and from it MEDUSA deduces the O2 profile? Because the zonation of oxic and anoxic OM degradation has implications for the production of alkalinity and thus carbonate dissolution. Finally, does MEDUSA check for the convergence of burial velocity at the very end if the solid components are not declared as volumeless?

Specific/technical comments:

page 1: ln. 21: I think it should read "the latter" here

Page 2: ln. 25/26: Maybe also add: . . .or organic matter completely oxidized even if oxygen levels are low and/or organic matter input is very high.

ln. 46: Please give example reference for the meta-model approach.

ln. 49 - 50: Please add references for examples for the " expert-chosen empirical parametric functions" and for the " system identification theory" approach

Page 3: Table 1+2 and in the corresponding text (e.g. ln 70 and first paragraph on pg. 5): For completeness please include OMEN-SED (Hülse et al., (2018) which is an analytical early diagenetic steady-state model for OM degradation dynamics with flexible resolution. OMEN-SED is available as stand-alone model and coupled to cGENIE and presents a noval approach to simulate benthic-pelagic coupling (i.e. different to the approaches presented in Table 1 and 2).

Page 5: Ln 127: "... during which some parts gets remineralised (i.e., oxidised or dissolved), and some parts gets buried." Please check the spelling here.

Ln 130-133: Please add reference.

ln 134: " Previously buried carbonates will then return to the sedimentary mixed as a result of the bioturbation activity..." I guess the "sedimentary mixing" is a typo, please rephrase.

Page 7: ln 142: I think the "instead of" can be deleted

Page 9:

ln 197: So for solutes DBL is the same as REACLAY only that porosity equals 1? Could you please include a brief explanation why/when a DBL is important and when it should be used?

Page 10: ln 215: Pointing the reader here to you supplementary document "Early Diagenesis in Sediments – A one-dimensional model formulation" would be good as it provides more and very useful information for instance about the parameterisations for tortuosity and how the diffusion coefficients are calculated. Maybe also cite Ullman and Aller (1982) who did a lot of early work on diffusion coefficients and tortuosity

parameterization.

ln. 221: bioirrigation "constant". What are you using for alpha in the set-up used in your simulations for this manuscript? Is it a depth dependent approach (e.g. Soetaert et al. (1996))?

Page 11: Additional constraints: Maybe it makes sense to include the constraint that the porosity profile is prescribed (i.e. time independent, $d\varphi/dt = 0$) at this point?

Page 12: ln. 292: "... and the topmost interior node of REACLAY" Is this part of the sentence not redundant as this note is always below the top of REACLAY? The same applies for the bottom of DBL.

Ln 297: " but the spacing and extent of each of these may be different." Depending on what? E.g. if the user wants to have a higher resolution for shelf-sediments compared to the deep ocean?

Page 16: ln 405: I think this should read: dissolved organic matter instead of "dissolved inorganic matter"

Page 17: ln 438: I think it should read: simulation experiments from Munhoven (2007)

ln 450-451: I guess the sentence could be changed to: "MBM is an eleven-box carbon cycle model of the carbon cycle in the ocean and the atmosphere."

ln 453: close the parenthesis after Pacific

Section 3.2 COUPSIM – Coupling simulator page 21 ln. 519: why did you just simulate depths greater than 1000m below sea-level? And are the shallower observations in Seiter (where generally the highest TOC concentrations are observed) excluded from the data-set (or is there hardly any data for shallower settings because of the resolution of the observations)?

Fig. 3 : I think opal are the red triangles. In that case the caption needs to be corrected.

Ln 551: Your rate constants for OM degradation could be for instance compared to Palastanga et al. (2011) who used the HAMOCC model coupled to a diagenetic model using a 1G-approach for OM. They also compare their results to the Seiter et al. (2005) data and find the best fit using k_ox=0.005 yr-1 and k_anox=0.002 yr-1 for depths > 2000m (k_ox=0.01 yr-1 and k_anox=0.008 yr-1 for depth < 2000m).

page 27 ln 652: spelling: I think it should read: "... in order to allow..."

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