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Geoscientific Model Development Discussions

Interactive comment on "OMEN-SED 0.9: A novel, numerically efficient organic matter sediment diagenesis module for coupling to Earth system models" *by* Dominik Hülse et al.

Anonymous Referee #2

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This paper presents a one-dimensional analytical early diagenetic model resolving biogeochemical processes associated with organic matter degradation in near-seafloor bioturbated marine sediment over short timescale assuming steady state solution. The model parameters are constrained by a database of oxygen and nitrate fluxes trough the sediment-water interface, global distribution pattern of POC content in surface sediments, porewater profiles of solid and dissolved species and the model developed before. The goal of the work is to provide a general model of marine sediments which can be used in ESM due to its very low compared to traditional numerical diagenetic models execution time (less than 0.1 sec). This analytical diagenetic model as well as parameterization procedure and sensitivity analysis are clearly explained, the comparison

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of model predictions and measurements is discussed thoroughly and the manuscript presents a full procedure to model POC degradation in bioturbated marine sediments. I do find the work to be novel and important, however, I have some comments that the authors need to address.

Specific comments.

The model neglects the effect of sediment compaction “due to mathematical constraints”. I understand the rational for this and accept a consistency of this assumption to near-seafloor (bioturbated) sediments; however, this might be a problem for deeper sediments discussed in the paper (down to 50 or 100cm). The authors should either define different porosity values for different depth-zones or to demonstrate that the results are not particularly sensitive to the value of this parameter.

Dividing the sediment column into functional zones in such a strict manner does not always represent reality well. Thus, “nitrogenous” zone may overlap with “oxic” zone. This assumption, as far as I understand, made it impossible to simulate nitrate SWI flux directed into the sediments in oxygenated environment, which is definitely not true. Validation of the model against measured benthic fluxes would probably demonstrate to some extent accordance of suggested method with real benthic system.

Nitrogen dynamics include “the metabolic production of ammonium, nitrification, denitrification as well as ammonium adsorption”. Denitrification is considered as a single-step process ignoring NO2- production/consumption and anaerobic ammonium oxidation (Anammox) which is undoubtedly a significant component of the biogeochemical nitrogen cycle (Devol, 2015). In other words, nitrogen dynamics is somewhat simplified. This simplification should be quantified/discussed in more details.

The efficiency of binning procedure discussed in section 4.2.1 is doubtful. First of all, such binning assumes presence of STD bars on the plots. Also, I think that it would be more logical to group POC content into POC rain rate (RRPOC) classes rather than WD classes as RRPOC may significantly vary at different regions of the ocean of the same

WD. Finally this binning gives a false impression of a good POC content fit. I realize that parameterization of multi-G model is beyond the scope of this sediment model development paper, therefore I suggest to use existing way to parameterize multi-G models and validate your model against the databases suggested in those studies (for example Stolpovsky et al., (submitted) https://www.biogeosciences-discuss.net/bg-2017-397/).

POC is not a very good constraint, since measured POC is in large part the less reactive stuff that is left over after mineralization of the more reactive fractions. This was shown in Stolpovsky et al., 2015 paper (see the discussion in section 4.3). Fluxes at the SWI are believed to be a better constraint.

Minor comments.

Eq. 1: As a time and depth independent parameter, porosity should be moved out of differential in order to emphasize that it is constant: Porosity*dC/dt instead of d(Porosity*C)/dt.

P. 8, L. 1: It is not immediately clear that the authors are talking about water (not sediment) depth.

Eq. 5: This representation sounds a bit odd. I think z_∞ should be replaced with zmax, as POC content at infinite depth believed to be zero.

P. 9, L. 25: SWI is given without initial explanation.

P. 25, L. 6 – 13: I agree that bioirrigation may enhance SWI fluxes of dissolved species, therefore I do not understand why this way of transport is technically ignored for all water depths (fir=1)?

P. 27, L. 28: PAWN is given without explanation.

Fig. 7: Please add ticks and numbers to X-axis on H2S at 2213 and 4298m and NH4 at 108m. Some plates have very inconvenient ranges on horizontal axis, for example

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H2S at 4298m.

Sec. 3.3.2: I do not understand the rational for comparing OMEN-SED results with another model (Thullner et al. 2009). I would suggest comparing it to existing SWI flux database mentioned before (Stolpovsky et al., 2015). Also, reporting global denitrification rate modeled with OMEN-SED and its comparison with previous studies would support the model.

P. 55, L. 24 – 25: Bold assumption, I suggest to avoid such formulations. The major advantage of OMEN-SED is its tremendously low computation time which is so important for ESMs. As always, only two options of the following three can be true the same time: “quickly”, “cheaply (super-computer is not needed)” or “qualitatively”.

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