**Specific comments for Andy**

**Andy, please change whatever you feel needs to be changed.**

**Anonymous Referee #1**

**8. Comment:**

A fraction of the sulfide produced is assumed to escape complete oxidation. Does this mimicking the effect of precipitation with iron, rather than escape from the sediment?

**Response:**

In the manuscript which got reviewed, this fraction mimicked the escape from the sediment. However, in response to the first critical comment of reviewer 3 (K. Wallmann) which also addresses this point, we made the following changes to OMEN-SED:

When coupled to an ESM we have made γH2S (fraction of H2S that is oxidised) dependent on the bottom water oxygenation state. That is, γH2S = 1.0 for oxic bottom waters and a user defined value γH2S <1.0 for anoxic bottom waters. For simplicity and as oxygen will eventually serve as the final TEA this is still implemented as aerobic sulfide oxidation.

In addition, we introduce another parameter (γPyr) representing the fraction of sulfide that is precipitated as pyrite (i.e. 0.0 <= γPyr < 1 - γH2S) under anoxic bottom waters. The text, tables and equations (for SO4, H2S and alkalinity) are changed accordingly. The presented results have not been changed and we note that γPyr = 0.0 for all simulations.

**DH: In the getaway you did not like the new parameter. Maybe we can just state that the model currently can't capture pyrite formation and let the non-oxidised H2S escape to the water-column!!??**

**DH:** SO we would never get an **H2S return flux???:**

OXIC: gamma=1.0 - everything is oxidized

ANOXIC: gamma < 1.0 - non-oxidized H2S is going to pyrite??

WHAT IF NOT ENOUGH Fe TO FORM ALL THE PYRITE?

**10. Comment:**

transport processes: A 1D diffusion/bioturbation model clearly faces major challenges in the coastal ocean, where sediments one predominantly are permeable. And setting fir=1, implying no bioirrigation, is also a very strong assumption.

**Response:**

Most sediments on the globe are non-sandy, therefore we had decided to neglect sandy sediments. However, the bioirrigation coefficient has been changed and is now represented by the empirical relationship with seafloor depth derived by Soetaert et al. (1996): fir = Min{1; 15.9 · z−0.43 }.

The text has been changed to (pg. 25 lines 16-18):

**TODO check pg + lines**

*“Soetaert et al. (1996) derived an empirical relationship between fir and seafloor depth (fir = Min{1; 15.9 · z −0.43 }) based on observations from Archer and Devol (1992) and Devol and Christensen (1993) which is used in OMEN-SED.“*

**DH: Shall we add something similar to Krumins e.g. in the limitations section or just in the response to the comment or in the paragraph on bioirrigation?**

**OMEN-SED – cGENIE coupling**

**20. Comment:**

The stated purpose of section 4 is …

… The validation of the coupled model requires more work, and I wonder whether this was not better done in a separate paper, in which the coupling to cGENIE and the parameterization of POC mineralization was explored in more detail.

**Response:**

As stated in the manuscript (page 45):

“*Our objective is not to perform and discuss a detailed calibration of the coupled models as this is beyond the scope of this sediment model development paper. Rather we want to showcase the feasibility of the model coupling, illustrate the range of results and thus information that can be generated with OMEN-SED and verify that model results capture the main observed global benthic biogeochemical features*.”

We think that demonstrating how OMEN-SED can be coupled to an ESM and illustrating the type of output/information generated by OMEN-SED within such a coupling is a central aspect of the model description paper.

However, we are fine with trimming down this section (as in the re-submitted version). We will discuss an improved model-data analysis (also using observations of SWI-fluxes) in a follow-up publication (as also suggested by reviewer #3 K. Wallmann).

Specifically, the sensitivity analysis for the spatially uniform degradation rate constants (Figure 12) and it's discussion has been removed (compare pages 47-50).

**DH: Should we remove more???**

**21. Comment:**

Figures 12 - 14: I gather the R2 values are for the bin averages. I don’t see much value

of that, as over- and underpredictions cancel each other out in the averaging. Why not

compute statistics for the actual model results with the Seiter data directly?

**Response:**

Figure 12 has been deleted from the manuscript. As the statistics for the actual data are not helpful/misleading (see also comment 4 of reviewer #2) we decided to remove the R2 values in Figures 13 and 14 (as well as their discussion in the text). Compare changes on pages 47-50).

**TODO check pgs!**

**Anonymous Referee #2**

**4. Comment:**

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/ ).

**Response:**

We thank the reviewer for the suggestion. We decided to follow suggestions from reviewer #1 and #3 (K. Wallmann) and shortened the cGENIE coupling section. Figure 12 (and its discussion) has been deleted from the manuscript. The R2 values in Figures 13 and 14 (as well as their discussion in the text) has been removed as well. Compare changes on pages 47-50.

**TODO check pgs!**

The ranges of simulated SWI-fluxes from the stand-alone OMEN-SED model are already compared to the Stolpovsky et al., (2015) database in Figure 6.

If binned by RRPOC for uniform k-values, all grid-cells with same RRPOC have the same preservation in OMEN-SED. Therefore, this would not be very useful.

**5. Comment:**

POC is not a very good constraint, since measured POC is in large part the less reac-

tive 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.

**Response:**

We shortened the coupling section of the manuscript and we will discuss an improved model-data analysis of the coupled model, using existing parameterizations and maps of SWI-fluxes, in a follow-up publication.

Also compare response to comment 20 of reviewer #1:

As stated in the manuscript (page 45):

“ *Our objective is not to perform and discuss a detailed calibration of the coupled models as this is beyond the scope of this sediment model development paper. Rather we want to showcase the feasibility of the model coupling, illustrate the range of results and thus information that can be generated with OMEN-SED and verify that model results capture the main observed global benthic biogeochemical features*.”

**Referee #3: K. Wallmann:**

**1. Comment:**

The model ignores sulfide precipitation and pyrite formation. Consequently, dis-

solved sulfide produced by sulfate reduction and AOM at depth diffuses upward to be

either oxidized by oxygen or released into ambient bottom waters. This is a very un-

realistic set-up. In most sediments dissolved sulfide is removed from the pore water

by pyrite precipitation while the remaining sulfide is oxidized with ferric iron, nitrate and

nitrite before it can reach the oxic surface layer or the ocean. Aerobic sulfide oxidation

is only important in highly reactive surface sediments where the diagenetic sequence

is not maintained but several electron acceptors are used simultaneously. The model

is based on the assumption that electron acceptors are used sequentially rather than

simultaneously. Hence, it cannot simulate situations where aerobic sulfide oxidation

is important but creates high rates of aerobic sulfide oxidation in geological settings

where this process does in fact not occur. The authors should try to fix this problem.

They could for example abandon the model parameter that defines the fraction of dis-

solved sulfide that escapes into bottom waters. In the modern ocean, sulfide leakage

from sediments occurs only in very rare situations and it does not make sense to sim-

ulate these anoxic sediments with a model that ignores iron cycling, pyrite formation

and sulfide precipitation. The authors could instead introduce a parameter that de-

fines the fraction of sulfide that is precipitated as pyrite and update the alkalinity model

accordingly.

**Response:**

We thank Prof. Wallmann for this very valid suggestion. At this rather advanced stage of the model development and evaluation processes we decided to implement the following changes to OMEN-SED: When coupled to an ESM we have made γH2S (fraction of H2S that is oxidised) dependent on the bottom water oxygenation state. That is, γH2S = 1.0 for oxic bottom waters and a user defined value γH2S <1.0 for anoxic bottom waters. For simplicity and as oxygen will eventually serve as the final TEA this is still implemented as aerobic sulfide oxidation.

In addition, we introduce another parameter (γPyr) representing the fraction of sulfide that is precipitated as pyrite (i.e. 0.0 <= γPyr < 1 - γH2S) when bottom waters are anoxic. The text, tables and equations (for SO4, H2S and alkalinity) are changed accordingly. The presented results have not been changed and we note that γPyr = 0.0 for all simulations.

**DH: In the getaway you did not like the new parameter. Maybe we can just state that the model currently can't capture pyrite formation and let the non-oxidised H2S escape to the water-column!!?**

**OLD:**

**???** So should I assume that under oxic conditions all H2S gets oxidized? But still with O2 and not with nitrate? But we could leave γH2S in for anoxic environments and introduce a new parameter that defines the fraction of sulfide that is precipitated as pyrite (you wanted that anyway ;) ).

**How to update the alkalinity model?**

**3. Comment:**

**Comment 3.1:** OMEN-SED is able to reproduce the strong down-core decrease in organic matter

reactivity observed in marine sediments by using two or more organic matter frac-

tions with widely different reactivity. This strength is nicely demonstrated in section 3.3

where the authors are able to show that typical pore water profiles are reproduced by

the model applying kinetic constants (k1, k2) that span several orders of magnitude

(Tab. 13). Subsequently, the authors try to reproduce the TOC distribution at the deep-

sea floor by coupling OMEN-SED to an earth system model. I think that TOC in surface

sediments is not a good parameter to validate the model because almost the entire or-

ganic matter raining to the deep-sea floor is degraded in the surface sediment rather

than preserved as sedimentary TOC. TOC concentrations in surface sediments at the

deep-sea floor are governed by TOC rain rates, mass accumulation rates (burial veloc-

ity), adsorption of organic matter on mineral surfaces, and the kinetic properties of the

very small refractory fraction that survives degradation (about 1 % of the total rain rate).

The strength of OMEN-SED to degrade the reactive fractions in a meaningful way does

not play out in this application.

**Response:**

We agree with the statement that TOC is not necessarily a good way to validate the coupled model and we would also favor fluxes or rates. However, we are not convinced that they give much better results if the database is limited. TOC in surface sediments was the data available on a global scale and also other ESM studies compare their results to it (e.g. HAMOCC, Palastanga et al. (2011)). As mentioned earlier, we will put in some more effort in a follow-up study where we compare calculated SWI-fluxes with observations.

In addition, as stated in the manuscript (page 45):

“ *Our objective is not to perform and discuss a detailed calibration of the coupled models as this is beyond the scope of this sediment model development paper. Rather we want to showcase the feasibility of the model coupling, illustrate the range of results and thus information that can be generated with OMEN-SED and verify that model results capture the main observed global benthic biogeochemical features*.”

Something about: This was the data available also other ESM studies compare their results to it (e.g. HAMOCC, Palastanga et al. (2011)).

**Comment 3.3:** I would encourage the authors to delete the entire section 4 of the paper because it

does not add useful information but presents rather misleading results. They shouldaim to present other more useful applications of their highly innovative analytical model in follow-up publications.

**Response:**

Here, we repeat parts of the response to comment 20 of reviewer #1:

We think that demonstrating how OMEN-SED can be coupled to an ESM and illustrating the type of output/information generated by OMEN-SED within such a coupling is a central aspect of the model description paper.

However, we are fine with trimming down this section (as in the re-submitted version). We will discuss an improved model-data analysis (also using observations of SWI-fluxes) in a follow-up publication.

Specifically, the sensitivity analysis for the spatially uniform degradation rate constants (Figure 12) and it's discussion has been removed (compare pages 47-50).

**DH: Should we remove more???**