**Just specific questions**

**Anonymous Referee #1**

**Comment:**

Model formulation - The model assumes no overlap of mineralization reactions with

different terminal electron acceptors, and assumes that secondary redox reactions can

be collapsed onto the interfaces between different mineralization zones (p.9). This is

probably ok in environments typically encountered at greater water depth, but there is

ample evidence of ’overlapping’ mineralization pathways in surficial sediments, in par-

ticular in permeable or bioturbated settings.

**Response:**

**Sandra:** Soetaert et al., (1996) also emphasize that this is generally a good approximation based on obervations (look up the ref).

**DH:**

Do you mean the statement *“Nevertheless, there is general agreement between the models* (this one and the steady-state one used by Reimers et al. 1992)*, which indicates that nutrient profiles can be described confidently by steady-state models in order to determine properties of the prevailing mineralization process.”* in the paper “Dynamic response of deep‐sea sediments to seasonal variations: A model”?

**Comment:**

Methanogenesis: the 1/2 methane to DIC ratio seems to imply acetoclastic methanogenesis. What evidence is there to ignore hydrogenotrophic methanogenesis?

**Response:**

**??? What to say???**

**Comment:**

For a globally applicable model, the lack of CaCO3 dissolution is an obvious issue. Thus, can you expand on what problems the modeling of CaCO3 dissolution would cause (page 18)? Is this linked to the calculation of pH? Why can pH (and then carbonate) not be estimated from DIC and alkalinity?

**Response:**

**???**

Sandra:

Doooh, you cannot solve an equation that depends on Ca, CaCO3 AND CO3 (DIC and pH). So there is no easy analytic solution. Just quickly summarize what kind of extreme assumptions Berner etc took to solve this equation analytically. I do have an idea of how to circumvent this, but we do not need to give this away. We also already discussed to limitations at lengths

DH:

Which Berner reference? OR what are the limitations?

**Comment:**

Is there no P sorption on iron oxides below the oxic zone? If so, why?

**Response:**

**???**

**Sandra:**

Because there are no iron oxides.

DH:

I suppose I refers to the denitrification zone!!?? Shouldn't there actually be still P sorption!? But it is anyway a tiny layer....

**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 abandon the γH2S paramter (i.e. all H2S is oxidized) at least for oxic bottom waters. Instead, we introduce a new parameter to define the fraction of H2S that is precipitated as pyrite.

* **Sandra:** I need to think about this. I favored this option from the beginning. But do not want to change things too much.

**Comment:**

The 2G model is a sensible choice. Model parameterisation is a general concern (page 3, line 30), but I suggest to cite some promising new approaches to address this issues such as presented by <https://www.biogeosciences-discuss.net/bg-2017-397>

**Response:**

Added sentence, page 5 line 31:

… parameters from one site to the global scale (Arndt et al., 2013). **Recently, however, new approaches to constrain the degradation rate constant of discrete OM classes have been suggested (Stolpovsky et al., 2015, 2017) that might help to parameterize OM mineralization kinetics on a global scale.**

I would contest that these approaches are promising. I really dislike the 2017 paper due to the very very strong assumptions made. Leave “promising” and say have been suggested. I’d like to make reference to Felipe’s work.

DH: It's no Felipe’s Paleoceanography paper, is it?

**Comment:**

- global transect case study: I don’t see the value of the 5% reoxidation case. This is simply unrealistic for the conditions considered here. A possible explanation why lower gammas are giving better match to the data in the shallow sites is that under those conditions, the conceptual model of a vertical separation of reaction zones is more and more violated, so the gamma become a ’fudge-factor’ to account for this (see also above comments about the coastal ocean).

**Response:**

Sandra: Of course gamma is a fudge factor. Need to think about this one too

DH:

Shall I do γ = 0.95 and also 0.0 and 1.0 to show the whole range?

**Comment:**

On p.54 it says "In theory, its scope of applicability thus ranges from the regional to

the global and from the seasonal to the millennial time-scale. " - in the following para-

graph they recognize that "This steady-state assumption is only valid if the variability in

boundary conditions and fluxes is generally longer than the characteristic timescales

of the reaction-transport processes. "

I recommend to be a little more cautious in the application of the model, since I am not

convinced that violations of assumptions underlying the conceptual model and non-

steady state effects can be ignored. The model clearly requires substantial tuning. It is

clear that the authors are aware of the shortcomings, and they discuss that the model

may not be adequate to assess seasonal patterns (and one can think of additional set-

tings, where fluxes vary over timescales intrinsic to the POC profile in the top 50cm

of sediment modeled here). My concern is that they largely ignore them in their appli-

cation, before acknowledging them in the discussion.

**Response:**

The reviewer is right when recommending caution in the application of the model. Indeed, more specific tuning is needed if OMEN-SED is used for more specific, regional environments. However, we want to re-emphasize that certain model assumptions are required to solve the equation analytically and that we are aware that these assumptions limit the predictive abilities of OMEN-SED for sites where special features are observed. However, OMEN-SED is designed for system/global scale applications and we believe that it is a reasonable assumption to apply OMEN-SED for these purposes.

DH:

Not sure – is the below what you meant:

We also want to stress that very successful coastal, system-scale models such as ERSEM, Gypens et al. (2008), Capet et al. (2016) and Arndt and Regnier (2007) make assumptions about the shape of the down-core solid phase profiles. This assumption can be easily relieved with the relaxation term - as suggested in the limitations section.

**OMEN-SED – cGENIE coupling**

**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: Okay like this?**

The reviewer is correct when saying that the validation of the coupled model requires more work. However, our objective was not to **validate** the coupled model as mentioned in the comment. 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, demonstrating the successful coupling of OMEN-SED to an ESM is very important for the paper. However, we are fine with trimming down this section (as in the re-submitted version) and 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).

**Anonymous Referee #2**

**Comment:**

The model neglects the effect of sediment compaction “due to mathematical con-

straints”. 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.

**Response:**

Assuming constant porosity is a requirement in order to solve the diagenetic equation analytically. It is a mathematical limitation and it will induce a certain error. However, the error is not very large and we have already shown this by comparing the performance of OMEN-SED against observed data (Section 3.2) and against model results from a fully formulated RTM with depth-varying porosity (Section 3.3).

We want to reiterate that OMEN-SED is designed for the coupling to ESMs and thus for global scale applications (compare responses to reviewer #1) and is a big advance compared to what is used at the moment (see comment by the K. Wallmann). Simple box models deal with far bigger concerns and limitations than constant porosity.

DH: Shall we add sentence to limitations?

E.g. The depth invariant porosity introduces a certain error as in reality porosity decreases with sediment depth. However, the error is not very large as has been shown by comparing the performance of OMEN-SED against observed data (Section 3.2) and against model results from a fully formulated RTM with depth-varying porosity (Section 3.3).

**Comment:**

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.

Sandra: No, what was the issue of that again???

DH:

Not sure – what you mean by this?

E.g. I say: Also. it is possible to simulate nitrate influx into the sediments in oxygenated environments with OMEN (see e.g. Fig. 6C, green line in Fig. 8).

**Comment:**

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 impor-

tant 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”.

**Response:**

**???**

He is talking about this statement:

*“We have shown that the performance of OMEN-SED is similar to that of a fully formulated, multi-component numerical model. “*

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

**1st 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: See also comment of reviewer 1**

**???** 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?

Sandra: Yes, I told you so. This will also be a bit of an issue for the OAE simulations!

What to do???

**3rd 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. As mentioned earlier, we will put in some more effort in a follow-up study where we compare calculated SWI-fluxes with observations.

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

**Comment 3.2:** Moreover, the model results are unrealistic. The best fit to the TOC data is apparently obtained assuming that the organic matter flux to the seabed is composed of two TOC fractions with very low reactivity in the order of 0.001 – 0.01 yr-1 (Fig. 12). This result is not consistent with the case study presented in section 3.3 that yields much higher k values (Tab. 13).

**Response: ???**

The low reactivities obtained for the global application (e.g. Fig. 12) agree with published results (see Arndt et al., (2013)), as well as with the results obtained with HAMOCC using a 1G-model (they found kox=0.005 yr-1 & kanox=0.002 yr-1 for deep sea sediments, Palastanga et al. (2011)). In addition, our simulated oxygen penetration depths compare well with observations (see Fig. 16). Especially deep sea sites in the gyres are characterised by very low POC input and degradation rates which causes O2 to diffuse down to the basement of the sediments (Fischer et al., 2009; D'Hondt et al., 2015).

The sites used for the stand-alone case study in section 3.3 where not really deep sea sites (complete data sets from deep sea sites within gyres are difficult to get). Therefore, degradation rate constants here are higher.

**Comment 3.2:** Moreover, we have shown previously that this very low reactivity is not consistent with the benthic fluxes of oxygen and nitrate that have been measured at the seabed (Stolpovsky et al., 2015). The error may be caused by the too high burial velocities applied in OMEN-SED (Eq. 46) and/or may be related to the rain rate and reactivity of organic matter calculated in GENIE.

**Response: ???**

The Stolpovsky et al. (2015) database is a very valuable source of information and we will compare our calculated fluxes using the coupled model with it in the follow-up study. The ranges of simulated SWI-fluxes from the stand-alone OMEN-SED model are already compared to the database in Figure 6. However, we would also argue that the Stolpovsky et al. (2015) database does not contain a representative amount of very deep ocean sites (e.g. within ocean gyres) characterised by very low SWI-fluxes (see e.g. Fischer et al., 2009; D'Hondt et al., 2015). D'Hondt el al. (2009) for instance found that the net rate of diagenetic degradation in the South Pacific Gyre is 1 to 3 orders of magnitude lower than at previously explored sites and they suggest that almost 50% of the worlds ocean may be characterised by these rates.

**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:**

We repeat here parts of a response to a comment of reviewer #1:

We think, demonstrating the successful coupling of OMEN-SED to an ESM is very important for the paper. However, we are fine with trimming down this section (as in the re-submitted version) and we will discuss an improved model-data analysis (also using observations of SWI-fluxes) in a follow-up publication.