**Complete responses**

**General/more major changes:**

1. Two out of three reviewers suggest to discuss the OMEN – cGENIE coupling in a separate paper.

Sandra:

I suggest a compromise: include example of coupling to illustrate ability and what can be done- i.e. slim down section. But take out the fitting OM section, which I never particularly liked

1. Abandon γH2S at least for oxic envirnments and introduce another parameter for the loss of H2S to pyrite.

Sandra:

No that is further development. I did try to push you to include such a simple parameter, but I don’t think we should do it now at such a late stage

**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:**

We agree with the reviewer that different biogeochemcical zones can overlap. However, as stated in the text, OMEN-SED is designed for the to ESMs and its formulation is thus first and foremost guided by achieving numerical efficiency while retaining biogeochemical reality. As summarized in the manuscript, there are essentially two approaches that can be used to describe biogeochemical processes in models. The first approach solves the general diagentic equation numerically on a regular or irregular grid and biogeochemical zonation emerges in response to inhibition terms allowing a certain degree of overlap between biogeochemical zones. This approach is highly flexible and thus preferable. Yet, its excessive computational demand unfortunately renders its application within a three-dimensional Earth System Model framework impossible. On the other hand, analytical models that subdivide the sediment into distinct biogeochemical model are computationally efficient and thus ideally suited to describe diagenetic dynamics in ESM.

By their very nature, analytical models do not allow for overlapping biogeochemical zones. As stated in the manuscript, this is a simplification. However, we disagree with the reviewer that this simplification would *per-se* prevent the application of such analytical approaches in shallower aquatic environments. In fact, OMEN-SED builds on a number of analytical models that were developed to investigate local, coupled nutrient and oxygen cycles in coastal sediments (e.g. [Billen, 1982; Goloway and Bender, 1982; Jahnke et al., 1982](https://www.sciencedirect.com/science/article/pii/S007966110700198X" \l "bib5), Slompo et al., 1996). Similar approaches were later successfully applied from oxic to anoxic sediments and at the regional coastal ocean scale (e.g. Ruardij and Van Raaphorst, 1995; Tromp et al., 1995; Gypens et al., 2008). In particular, Gypens et al., 2008 points out that accounting for secondary redox process in the boundary condition induces little error as “using a numerical model, [Soetaert et al. (1996a)](https://www.sciencedirect.com/science/article/pii/S007966110700198X" \l "bib38) showed that […] re-oxidation mainly occurs at the oxic-anoxic transition interface.”

Finally, the good agreement between OMEN-SED and the results obtained with a fully formulated numerical RTM (compare Section 3.3, allowing for overlapping TEA use) shows that this is not a critical limitation of OMEN-SED - even for shallow sediments.

We have clarified these points in the manuscript by….

However, we have included a sentence on this in the limitations section:

**TODO**: “Give sentence! E.g. like Schulz&Zabel book: This sequence is generally valid, even though numerous studies have identified an overlap of carbon oxidation pathways within the sediment resulting from competition between microbial populations (Canfield 1993) and the presence of microenvironments (e.g. Jørgensen 1977; cf. Chapters 7, 8, 12).”

**Comment:**

In the denitrification layer, all N goes to N2. However, in the suite of processes involved in the breakdown of organic matter, ammonium is produced as well, even if nitrate serves as TEA. Ammonium produced in the denitrification zone would need to be accounted for at the transition to the oxic zone as well, which may further reduced the O2 penetration depth.

**Response:**

This is a misunderstanding: Ammonium is produced in the denitrification zone and its reoxidation is accounted for at the lower boundary of the oxic zone (for NH4 see Table 4 FNH4(zox), for O2 see Table 3 Fred(zox)).

**Comment:**

Also, processes such as DNRA or anammox are not included, even though literature surveys (e.g. Dalsgaard et al. 2005) indicate that anammox is relevant at shallower water depth.

**Response:**

Anammox is implicitely included in the model. The organic nitrogen released during the denitrification process is assumed to be directly oxidized with nitrite to N2 through a coupling between denitrification and anaerobic ammonium oxidation.

However, we would like to stress again that OMEN-SED is a benthic model designed for the coupling to ESM. Most ESM do not even explicitly resolve N-dynamics. In addition, OMEN-SED is a system/global scale model that aims to resolve the most pertinent biogeochemical dynamics on a global scale (including a plaeoenvironmental context) and estimate the main SWI-fluxes and not a model that aims at resolving specific local scale dynamics. Even most local scale RTM applications doe not resolve DNRA and anammox explicitly. However, OMEN-SED could be easily adapted to explicitly resolve these processes if the specific application requires their representation (e.g. coastal ocean).

However, we included a sentence on this in the limitation section:

“**TODO**: add sentence here”

**Comment:**

Methane oxidation: All methane is assumed to be oxidized anaerobically. Is this done for simplicity? Is there no leakage term (gamma\_CH4; comparable to incomplete sulfide and ammonium oxidation) because methane escaping from the anoxic zone is assumed to be removed by aerobic methane oxidation?

**Response:**

There is a leakage term γCH4 comparable to incomplete sulfide and ammonium oxidation (see Table 10, SO4 boundary conditions 5 and 8.2; H2S boundary conditions 5 and 9).

However, it can be safely assume that almost all CH4 is oxidized anaerobically (e.g. Regnier et al., 20011; Reeburgh et al., (1991) estimated more than 80%)- except for active (very localized) sites and slope failure, which can, in theory, be accounted for through the gamma term.

The comment about the leakage term is a misunderstanding: There is a leakage term γCH4 comparable to incomplete sulfide and ammonium oxidation (see Table 10, SO4 boundary conditions 5 and 8.2; H2S boundary conditions 5 and 9).

**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:**

In OMEN-SED, pH can in theory be calculate from DIC and alkalinity (as stated in Section 5, page 54). However, simulating CaCO3 dissolution is complicated by its dependency on different chemical species.

In general, strongly coupled biogeochemical dynamics complicate the analytical solution of the reaction-transport equation. Carbonate dissolution is kinetically controlled by the amount of carbonate and thermodynamically controlled by the porewaters’saturation state with respect to carbonates. The reaction term thus depends on CaCO3, Ca2+ and CO32-. The carbonate ion concentration, in turn, depends on DIC and alkalinity (pH). In addition, carbonate dissolution exerts on important effect on alkalinity (pH). As a consequence, there is no analytical solution for the fully formulated reaction-transport equation.

**Just quickly summarize what kind of extreme assumptions Berner, guinasso and schink or was it Bernie? 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**

In addition, as stated in Section 5, “already planned future extensions of OMEN-SED include an explicit description of carbonate dissolution.”

**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:**

Why does some of the ammonium created below the oxic zone escape oxidation, but oxidation

of ammonium to nitrate is complete in the oxic zone?

**Response:**

This is a misunderstanding: There is also leakage term (γNH4) for ammonium to nitrate oxidation in the oxic zone (see Eqs. 12, 15 and 16).

**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.

**Comment:**

Iron cycling is not represented explicitly but some of the effect of iron cycling is parameterised. With its effect on sulphur cycling, P sorption and C mineralization (metal reduction can be the main mineralization pathway, see e.g. Canfield et al. 1993), I don’t fully understand the reason for doing so (apart from the added complexity when dealing with another solid phase).

**Response:**

OMEN-SED will be mainly applied on a system/global scale, coupled to an ESM, where iron reduction has been shown to play just a minor role (i.e. about 3% of the global carbon mineralization rate, Thullner et al. 2009). Also, Fe-dynamics are generally not explicitly resolved in ESMs.However, as stated in Section 5, “already planned future extensions of OMEN-SED include an explicit description of iron.”

**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 neglect them. 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 }.

**TODO:** Add changed sentence here:

**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:**

We added the suggested reference to the manuscript. However, the suggested approach is based on empirical relationships derived from modern ocean data, as well as strong assumptions. Its applicability to past and future oceans is thus questionable and the problem of parametrizing organic matter reactivity remains for these applications.

Added sentence, page 5 line 31:

“… parameters from one site to the global scale (Arndt et al., 2013). **Stolpovsky et al., 2017 suggested an empirically derived approach to constrain degradation rate constant in a 2G model on a global scale. These approaches are derived from present-day observations and might help constrain parameters for present-day applications. However, the problem of constraining 2G degradation model parameters remains for largely different environmental conditions encountered in the past and future.”**

**Comment:**

site comparison case studies: - Table 13: fix the depths for the SB and IM sites (SB is the 585m site ...).

**Response:**

Changed.

**Comment:**

site comparison case studies: - Why are the stoichiometric factors set to default values when

Epping et al. provide the C/N ratio of the surface sediment?

**Response:**

We intended to do as little site tuning as possible in order to test how the default model performs for these sites and to be able to evaluate the performance of the model in data poor areas.

**Comment:**

site comparison case studies: - If a non-local exchange mechanism resulting from bioirrigation is invoked for the Canyon site, what is the source of the high ammonium (and DIC) leading to the observed increase in concentration at depth?

**Response:**

We do not have detailed knowledge of the specific sites, however, we assume it is a result of degradation of organic matter which has been delivered from the shelf. As stated on page 34: “*For a more detailed description of the study areas and the experimental work, the interested reader is referred to the publications by Reimers et al. (1996) and Epping et al. (2002).*”

**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. It accounts for all the processes that may enhance escape but are not explicitely resolved. However, I would reject the comment that this is a problem with assuming strict zonation. I would argue that this reflects the more intense dynamics in shallow ocean regions. I would rather argue that the increased escape is due to enhanced macrofaunal activity.

Also stress that the analytical approaches (distinct redox zones) have given good results for coastal/estuarine sediments (see comment above)

DH:

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

No do not see the need for this

**Comment:**

- link to cGENIE: - on page 43, it says tha if fPOC is computed to be > 1 (more than 100% is preserved), then this is discarded and all POC is remineralized. Imposing constraints is fine, but what is the rationale for jumping from >100 to 0 % preservation?

**Response:**

The result fPOC > 1.0 does not imply that 100% preservation is a realistic result. It just means that the OMEN-SED solution does not provide sensitive values. Therefore, its results are discarded and a reflective boundary is assumed (a reflective boundary is a better choice than the conservative as in most cases the majority of OM is degraded during early diagenesis). However, this is just a safety measure and has not occurred in our experiments so far.

**Comment:**

- link to cGENIE. Page 44 discusses the challenges in applying the model in such a setting. In addition, deposition fluxes may change over time. At what point is the steady state assumption on the POC profile still valid under such settings? This is addressed summarily at of the bottom of page 54.

However, I think it is important to lead with this, before interpreting the data-model comparison.

**Response:**

As suggested by the reviewer the steady-assumption is addressed earlier in the text.

**TODO:** Add the sentence and where it is!

**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:**

As stated in the manuscript, OMEN-SED is first and foremost designed for the coupling to ESM. More specific tuning/adaptation is needed if OMEN-SED is used for specific, regional environments, e.g coastal environments.

However, we would like to re-emphasize that the current version of OMEN-SED performs well across different depositional environments ranging from the coastal to the deep ocean as evidenced by the model-data and model-model comparison. As outlined in the “Model Scope and Future development” section additional developments, such as adapting pseudo-transient dynamics will further facilitate the application of OMEN-SED to more dynamic environments. A number of benthic models specifically designed for coastal/estuarine environments (e.g. ERSEM Ruardij and Rapphorst et al., 1997; Arndt and Regnier, 2007) have successfully applied such an approach. We therefore maintain our point of view that, in theory, the scope of applicability of OMEN-SED also includes coupling to system-scale estuarine and/or coastal ocean models..

**Comment:**

I also suggest to tone down the finding that "A comparison between simulated OM contents and observations indicates that depth dependent k-f relationships provide the best fit (Section 4.2.2), confirming more theoretical considerations regarding the different time and reactivity scales that

need to be considered (see Section 4.2). " The age-reactivity relationship is pretty well established, without confirmation by this modeling effort.

**Response:**

**TODO:** Will be changed anyway as Section 4 will probably change quite a bit...

This is a misunderstanding. We do not argue that model results “confirm” the reactivity-age link. We wanted to emphasize these results confirm that reducing the continuous distribution of organic matter reactivities into two distinct reactivity classes (2G Model) requires different k-f values for shallow vs deep ocean sediments because of the largely different reaction timescales involved (also see Fig. XXX). To clarify, we rephrased the sentence:

“A comparison between simulated OM contents and observations indicates that a depth dependent k-f relationship provides the best fit (Section 4.2.2). These results confirm that reducing the continuous distribution of organic matter reactivities into two distinct reactivity classes (2G Model) requires different k-f values for shallow vs deep ocean sediments because of the largely different reaction timescales involved (also see Fig. XXX, Section 4.2). ”

**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:**

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*.”

“ 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).

**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:**

Most of the figures have been removed from the paper. In the remaining figures the statistics are calculated for the actual model/data points.

**Minor comments:**

**Comment:**

page 8/ line 1: It is said that all parameters in Eq. 1 may vary with depth, but above it is stated that porosity and burial rates are constant with depth.

**Response:**

We thank the reviewer for highlighting this. This has been changed in the revised manuscript to:

*“All parameters in Eq. (1)****, apart from porosity and burial rate,*** *may vary with* ***sediment*** *depth and many reaction rate expressions depend on the concentration of other species. ”*

**Comment:**

- the fraction of POC buried is defined as the POC at z=0 relative to the POC at depth.

Why is it not defined as the flux at z=0 vs. the flux at depth (it seems Eq. 5 ignores

the diffusion flux)?

**Response:**

We decided to calculate the fraction of POC preserved dependent on the concentrations of POC at z=0 and z=zinf mainly because this information is required by cGENIE.

**Comment:**

Related to that, page 10, line 18 refers to a concentration/flux

boundary conditions at the SWI. The following equations and Table 2 only show a

known concentration, not a flux condition. However, the latter would be useful when

connecting the sediment model to a model of the water column. On page 43, Eq. 51 this is addressed - make this clear earlier.

**Response:**

We thank the reviewer for highlighting this. The text on page 10, line 18 has been changed and a reference to Eq. 51 has been added:

*“For organic matter, OMEN-SED applies a known* ***concentration*** *at the sediment-water interface and assumes continuity across the bottom of the bioturbated zone, zbio. When OMEN-SED is coupled to an ESM****, the POC depositional flux from the coupled ocean model is converted to a concentration by solving the flux divergence equation (51).****”*

**Comment:**

Can lines 21-23 on page 21 be deleted?

**Response:**

We thank the reviewer for highlighting this. The lines have been deleted.

**Comment:**

page 31, line 2: specify NH4, SO4 and H2S FLUXES

**Response:**

Text has been corrected as suggested.

**Comment:**

Figure 3 did not help me much. Does the green dashed vertical arrow indicate possible locations of zbio?

**Response:**

Yes, it does indicate locations of zbio. We added this to the caption of Figure 3. However, we strongly believe that Figure 3 illustrates the bioturbation boundary problem in an efficient way and also highlights the occurrence of integration constants and ODE solutions for the different sediment layers.

New caption:

*“Schematic of the generic boundary condition matching (GBCM) problem. Showing the resulting integration constants (Ai , Bi ) and ODE solutions (Ei , Fi, Gi ) for the different sediment layers and the bioturbation boundary* ***(possible locations are indicated by the green vertical arrow)****.”*

**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 required 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). The novel model represents a big advance compared to the description of benthic-pelagic exchange processes currently incorporated into ESM (see comment by the K. Wallmann and Hulse et al., 2017). Conservative and reflective boundaries, as well as simple box models are characterized by stronger, simplifying assumptions and far bigger 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.

Validation of the model against measured benthic fluxes would probably demonstrate

to some extent accordance of suggested method with real benthic system.

**Response:**

See comment to Reviewer #1:

We agree with the reviewer that different mineralization zones can overlap, especially in shallower sediments. However, as stated in the text, OMEN-SED is a system scale or global model for coupling to ESMs and not a local reaction transport model. Thus, its main aim is to reproduce the main, global features (e.g. dynamic redox zonation and estimates of SWI-fluxes) and not local scale dynamics. Therefore, we argue that the assumption of no overlap is reasonable as it is of minor importance on a global scale.

In addition, as stated in the text (pg. 8), OMEN-SED builds upon models developed for the coastal ocean (e.g. Billen, 1982; Goloway and Bender, 1982; Ruardij and Van Raaphorst, 1995; Tromp

et al., 1995; Gypens et al., 2008) which make the same assumption and have been shown to perform very well. In addition, the good agreement between OMEN-SED and the results obtained with a fully formulated numerical RTM (compare Section 3.3, allowing for overlapping TEA use) indicate that this is not a critical limitation of OMEN-SED - even for shallow sediments.

However, we have included a sentence on this in the limitations section:

“**TODO**: Give sentence! E.g. like Schulz&Zabel book: This sequence is generally valid, even though numerous studies have identified an overlap of carbon oxidation pathways within the sediment resulting from competition between microbial populations (Canfield 1993) and the presence of microenvironments (e.g. Jørgensen 1977; cf. Chapters 7, 8, 12).”

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).

**TODO??? Maybe add validation of the stand-alone model against the Stolpovsky database of O2 and NO3 SWI-fluxes??? Tune it to the different locations....**

**Comment:**

Nitrogen dynamics include “the metabolic production of ammonium, nitrification, deni-

trification as well as ammonium adsorption”. Denitrification is considered as a single-

step process ignoring NO2- production/consumption and anaerobic ammonium oxida-

tion (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.

**Response:**

See comment to Reviewer #1:

Here we want to stress again the aim of the model: OMEN-SED is a system/global scale model that aims to reproduce the main sediment features and estimate the main SWI-fluxes and not a model that aims at resolving specific local scale dynamics. Additionally, OMEN-SED will be mostly coupled to ESMs which very often do not explicitly resolve N-dynamics.

However, we included a sentence on this in the limitation section:

“**TODO**: add sentence here”

**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 (as in the re-submitted version). Therefore, most of the figures with bin-classes have been deleted from the paper. In the remaining figures the statistics are calculated for the actual model/data points (as suggested by reviewer #1). We will discuss an improved model-data analysis of the coupled model, using existing parameterizations, in a follow-up publication (as also suggested by reviewer #3 K. Wallmann).

However, 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.

**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 do agree with the statement that POC is not necessarily a good way to validate the model (also made by K. Wallmann). We reiterate here, that 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 the 1st OMEN-cGENIE coupling comment of reviewer #1). We will also calculate global estimates of fluxes and compare these with other results from the literature.

**Minor comments.**

**Comment:**

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.

**Response:**

This has been changed as indicated.

**Done**

**Comment:**

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

sediment) depth.

**Response:**

We agree that this is a bit misleading. We are actually talking about sediment depth. This has been changed in the revised manuscript to:

*“All parameters in Eq. (1)****, apart from porosity and burial rate,*** *may vary with* ***sediment*** *depth and many reaction rate expressions depend on the concentration of other species. ”*

**Done**

**Comment:**

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.

**Response:**

We disagree. The POC content of marine sediments does not tend to zero. A significant amount of POC is buried in marine sediments and enters the longterm C cycle (rock cycle). Without this imbalance between production and respiration, no O2 would have ever accumulated in the atmosphere.

We agree, z∞ has been replaced with zmax in the entire manuscript.

I don’t agree

**Done**

**Comment:**

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

**Response:**

The explanation has been added at this part in the manuscript: *“...* ***sediment-water interface*** *(SWI) …”*

**Done**

**Comment:**

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)?

**Response:**

**TODO!**

**Comment:**

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

**Response:**

As the name PAWN is derived from the authors names and not acronym we do not think this information is of any value here.

**“Done”**

**Comment:**

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

H2S at 4298m.

**Response:**

**TODO!**

**Comment:**

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 denitrifi-

cation rate modeled with OMEN-SED and its comparison with previous studies would

support the model.

**Response:**

**We evaluated the performance of OMEN-SED by comparing model results with data (section XXX), as well as the results of a fully-formulated, numerical RTM (section XXX). The comparison of OMEN-SED with the results of the numerically solved RTM allows evaluating to which extend simplifying assumptions (e.g. constant porosity, non-overlapping redox zones etc) affect simulation results and, thus, quantitatively test the performance of the computationally efficient OMEN-SED approach against the computationally expensive numerical approach.**

**Comment:**

P. 55, L. 24 – 25: Bold assumption, I suggest to avoid such formulations. The major

advantage of OMEN-SED is its treendously 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:**

**We disagree. This is not an assumption, but the conclusion from the model-data and model-model comparison. Results show that the performance of OMEN-SED at the system scale is similar to that of a numerical model.**

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???**

**2nd Comment:**

The authors use an empirical equation by Middleburg et al. (1997) to define burial

velocity (w) as function of water depth (Eq. 46). Unfortunately, w is seriously overes-

timates by this equation. As an example, w at 1000 m water depth results as 160 cm

kyr-1 applying Eq. 46 whereas the available data indicate global mean rates in the or-

der of 10 – 20 cm kyr-1 for this water depth (Burwicz et al., 2011). The extremely high

burial velocities derived from Eq. 46 compromise the TOC concentration and other

model results especially when the model is applied at global scale.

**Response:**

The Middelburg et al. (1997) equation is just used in the stand-alone OMEN-SED version. When coupled to cGENIE we use the burial velocity of the ESM. In addition, the Burwicz et al. (2011) parameterisation is already added as an option in OMEN-SED (see p. XX ??). We made it the default version for the stand-alone model:

**TODO:** Add the changed sentence:

**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 obtain).

**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.