General/more major changes:

**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:**

Agreed, zones overlap but how important is this on a global scale? The main applications of OMEN-SED are global studies when coupled to ESMs. However, OMEN-SED builds upon models developed for the coastal ocean (e.g. Vanderborght et al., 1977; Billen, Gypens et al., 2008) which have been shown to perform very well.

**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 produced in the denitrification zone is included at the transition to 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:**

**???**

**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:**

This 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).

*All methane is assumed to be oxidized anaerobically. Is this done for simplicity?*

@Sandra: Need to say anything about that?

**Comment:**

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

**Response:**

**???**

**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:**

**???**

OMEN-SED can potentially estimate pH from DIC and alkalinity (as stated in Section 5, page 54).

**Comment:**

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

**Response:**

**???**

**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:**

In the beginning OMEN-SED will be mainly applied on a 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). However, as stated in Section 5, “already planned future extensions of OMEN-SED include an explicit description of iron.” Especially, when the model is applied in specific coastal environments.

**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:**

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:

@Sandra: Any response to the first sentence?

**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, promising new approaches to constrain the degradation rate constant of discrete OM classes have been presented (Stolpovsky et al., 2015, 2017) that might help to parameterize OM mineralization kinetics on a global scale.**

**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 change as little as possible to site specific values in order to test how the default model performs for these sites.

**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:**

???

**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:**

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

**Comment:**

- link to cGENIE: - on page 43, it says that 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 OMEN-SED does not provide sensitive values. Therefore, its results are discarded and a reflective boundary is assumed. So far this has not occurred in our experiments.

**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 reviewer 1 and 3, the coupling of OMEN-SED to cGENIE has been removed from this paper and will be submitted to GMD in a separate manuscript. We will make sure, to address the steady-assumption earlier.

**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:**

TODO: Explain better how OMEN-SED could be used in specific settings!?

Regional: more site specific tuning

seasonal: as upper/lower boundary or discard this application completely???

**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:**

As suggested by reviewer 1 and 3, the coupling of OMEN-SED to cGENIE has been removed from this paper and will be submitted to GMD in a separate manuscript. We will make sure, to tone the statement down. For example:

“A comparison between simulated OM contents and observations indicates that depth dependent k-f relationships provide the best fit **in our model** (Section 4.2.2), confirming **the established age-reactivity relationship as presented in our** more theoretical considerations regarding the different time and reactivity scales that need to be considered (see 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 suggested by reviewer 1 and 3, the coupling of OMEN-SED to cGENIE has been removed from this paper and will be explored in more detail in another manuscript.

**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:**

This figure is not included in the revised version of the paper.

**Minor comments:**

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.

- 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)? 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.

Can lines 21-23 on page 21 be deleted?

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

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