**General/more major changes:**

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

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.

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:**

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. Soetaert et al., also emphasizes that this is generally a good approximation based on obervations (look up the ref). In addition, the good agreement between OMEN-SED and the results obtained with a fully formulated numerical RTM (allowing for overlapping TEA use) indicate that this is not a critical limitation of OMEN-SED- even for shallow sediments.

Also stress again the aims of this model. It is a global model for coupling to ESM (which is very clearly stated in the paper) and not a local RTM. So its main aim is to reproduce the main features (e.g dynamic redox zonation and estiamtes of fluxes) and not local scale dynamics (which again is stated in the main text)

**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:**

**???**

**Again, stress the aim of the model- it is a global scale model that aims at reproducing the main features and estimate fluxes and not a model that aims at resolving specific local scale dynamics**

* **However you might want to include a sentence on this in the limitation section…**

**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?

You can savely assume that almost all CH$ IS actually oxidized anaerobically- except for active sites (very localized) and slope failure. However this can in theory be accounted for through the gamma term

**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:**

**???**

**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**

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:**

**???**

**Because there are no iron oxides.**

**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.

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

**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?

Just make clear that we neglect sandy sediments. This is again a bullshit comment. Most sediments on the globe are non-sandy

**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.**

**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.**

**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.

Yes, as little site tuning as possible 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:**

??? How shall I know?

OM degradation

**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).

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

**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 the OMEN-SED solution does not provide sensitive values (explain how this can happen and specify how often this happens). Therefore, its results are discarded and a reflective boundary is assumed (explain why reflective is a better choice than conservative in this case). 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.

I really do not like a complete removal

**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???

This can all be contested. I don’t agree here. You need to emphasize that certain model assumptions are required to solve the eq analytically and that these assumptions obviously limit the predictive abilities of OMEN-SED for sites where you do observe special features. However OMEN-SED is designed for system-scale/global scale applications and it will be a reasonable assumption top apply OMEN-SED at these sites. Also stress that very successful coastal, system-scale models such as ERSEM, Gypens, Capet Arndt&Regnier make assumptions about the shape of the downcore solid phase profiles.

The ss assumption can be easily relieved with the relaxation term- as suggested in the limitatiuons section

**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.

That is not what is meant here!!! Probably need to reformulate this. We do not argue that k-f works, we say that this confirms that you need different k-f values for shallow vs deep because of the largely different timescales involved.

**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:

Don’t be such a wimp! He misunderstood this

“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.

We do not want to validate the model as mentioned in the text, but illustrate its use and I find thius very important for this paper. I am fine with slimming this down, but not with taking this out completly

**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?

**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.

You could define different porosity values with depth, but would not be able to resolve resulting advective velocity changes so what is the point. This is a mathematical limitation and this will induce a certain error (which is not terribly big). Stress again that OMEN\_SED was designed for global scale applications and is a big advance compared to what is used at the moment (see the wallmann comment!). Simple box models deal with far bigger concerns and limitations than constant porosity!!!

It does not make a big difference and we have al;ready shown this by comparing the performance of OMEN\_SED against observed data and against model results form fully formulated RTMs WITH depth-varying pporosity.

Jezz it is always the same. It is absolutely fine to do simple bullshit that is proven to give the wrong result, but if you make a big step towards reality, you have the “”but it does not resolve the gold0-shitting unicorn that has been observed in ….

**Response:**

**??? We cannot define different porosities for different depth zones, right? As that would complicate finding the analytical solution. Include porosity in sensitivity analysis? I'd rather avoid this, as the SA was not too easy... Can we just argue here somewhow? Or probably have to do the SA again anyway for new parameter for H2S loss to pyrite formation (see K. Wallmann).**

**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.

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

Validation of the model against measured benthic fluxes would probably demonstrate

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

**Response:**

**???**

The other is correct when stating that “Dividing the sediment column into functional zones in such a strict manner does not always represent reality well”.

Here we repeat the answer given to the first comment of Reviewer #1. 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.

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).

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

…told you so….

**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:**

**??? similar to reviewer 1.**

**Yes, add section to limitation section- however also stress that N-dynamics are often not explicitely resolved in ESM**

**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:**

If binned by RRPOC for uniform k all grid-cells with same RRPOC have the same preservation in

OMEN-SED. So this wouild not be very helpful.

The other suggestion (use existing parameterization and compare with O2, NO3 SWI-database) is more a stand-alone model exercise!? As database has not lat-lon coordinates, plus it would be unfair when in out is from GENIE.

[Also, to be honest I don't really understand/like the Stolpovsky parameterization... They relate it to kapp calculated with Martin-curve... and then, I believe, they relate it to RRPOC which is calculated from the depth-integrated rate of POC degradation in the bioturbated layer. But then they just get globally uniform k-values anyway...] **??? I find this very strange...**

Because it IS strange. I do find neither the 2015 nor the 2017 paper promising

**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:**

I do understand his/her point, also made by K. Wallmann. Split paper into 2 and compare in the coupling paper calculated SWI-fluxes with maps (even though the SWI-fluxe also depend heavily what we get from GENIE). Will also calculate global estimates of fluxes and compare these with other results from the literature.

**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

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 denitrifi-

cation 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 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”.

**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 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 ;) ).

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

**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:**

Added the Burwicz parameterisation as an option anyway. I will phrase it differently.

**3rd Comment:**

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.

I agree with the statement that TOC is not necessarily a good way to validate. But I disagree with the SWI degradation statement. That is a KIEL belive system.

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. 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).

Which agrees with published results! See Arndt et al., 2013 also see Pacific gyre data where O2 diffuses down to the basmnt du to low degradation rates, also see Middelburg compilation or burdige k-waterdepth compilation

DH: ALSO IN GOOD AGREEMENT WITH PALASTANGA rate constants!

This result is not consistent with the case study presented in

section 3.3 that yields much higher k values (Tab. 13).

Not really deep sea sites.

Moreover, we have shown pre-

viously 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).

Are these really deep ocean sites? Gyres etc??? Look at the D’Hondt and Danish papers about O2 fluxes in the gyre. Ask James Bradley for reference. He is working on the pacific gyre sitesd

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.

**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 should**

**aim to present other more useful applications of their highly innovative analytical model**

**in follow-up publications.**

**Response:**

The coupling with cGENIE will be deleted and discussed in more detail in a second paper.

I agree that we should put some effort into a better fit- maybe a joined effort with james Bradley and Philip? They could supply data (e.g. O2 fluxes etc) so that you only have to run the model?!