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## Comment on gmd-2021-4

Anonymous Referee #1

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Referee comment on "OMEN-SED(-RCM) (v1.1): a pseudo-reactive continuum representation of organic matter degradation dynamics for OMEN-SED" by Philip Pika et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-4-RC1>, 2021

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The authors presented a multi-G approximation method, in which organic matter (OM) can be divided into a large number of classes with different degradation rate to represent a pseudo reactive continuum of OM, as an extension / further development of an existing analytical early diagenetic model (OMEN-SED) that is originally based on two reactive OM classes (2-G model). The approximation is based on two assumptions, namely (1) the distribution of reactivity/degradation rate of OM in marine sediments can be reasonably described by a gamma function, and (2) the vertical OM distribution in sediments is in an equilibrium status (i.e. the temporal gradient of OM is zero at any depth in sediments) so that an analytical solution of the transport-reaction equation of OM can be derived. Because the proposed multi-G approach is based on the analytical solution, which does not require solving the transport-reaction equation dynamically, therefore computational expense is not a hindering factor. This makes the proposed method appealing.

However, I have several major concerns: 1) although the method seems to produce reasonable results, the authors did not provide convincing arguments that the proposed method outperforms the original 2-G model; 2) the method is described in an unclear manner; 3) while the method tries to fit observational vertical profiles in sediments, the boundary conditions needed for the model-data fit at some places do not reflect reality and bio-physical laws; and 4) the precondition for the validity of the approach, namely a zero temporal gradient of OM at any depth in sediments, can hardly be met in a dynamic environment. This makes the approach of limited use for coupling to dynamic models in which sedimentation of OM is variable, which is especially true for continental margins.

Specific comments:

\* The method (section 2) is not described clearly.

(a) From eq.6, it is stated that  $om(k,t)$  represents the probability density function that determines the amount of bulk OM with a reactivity between  $k$  and  $k+dk$  at time  $t$ . As  $om(k,t)$  is a probability density function, the sum of  $om(k,t)$  across all  $k$  at any specific  $t$  should always be 1. However, this is not satisfied in eq.7, in which  $om(k,0)$  is dependent on  $OM(0)$ . Please clarify this.

(b) I am confused by the definition and use of  $k$ .  $k$  is supposed to indicate the reactivity of OM, which is a variable. So what is the justification of eq.8 that  $k$  is determined by  $a$ ,  $v$  and sediment age? I understand that the latter three parameters at any specific depth are either pre-described (e.g.  $v=0.15$  in case studies) or derived by model-data fitting. This means that  $k$  is also fixed by these values, which is not variable any more. Further, how is the age of sediment layer at depth  $z$  derived? It seems that this quantity is another variable which needs to be solved in the method, in addition to  $a$  and  $v$ . This contradicts the statement and conclusion that only  $a$  and  $v$  need to be solved. Please clarify.

(c) Another parameterization of  $k$  using eq. 16 clearly violates the original relationship as mentioned in (b). Please justify the validity of the method if a different parameterization of  $k$  is used.

\* In the case study 3.1, although the free variables  $a$ ,  $v$  are tuned that the model produces results close to observed sediment profiles, their setting has no mechanistic connection with other environmental variables, e.g. in Tabl 1, it is not clear why  $z_{bio}$  is set to 0.01 cm at depth 585m, which means that there is no bioturbation at all, but then why  $D_{bio}$  has a non-zero value and how these parameters are related to the setting of  $a$  and  $v$ ? Also it is not clear why  $a$  has a very small value (corresponding to very small lifetime of OM, therefore quite labile component) for depth 2213 m. Compared to a very confusing setting in this case study, the setting in 3.2 (Table 2) seems more reasonable and respects reality.

\* There is hardly justification for the validity of the approach in global application in section 3.3, as shown in Fig.6. In particular, the part that simulated OPD exceeds  $10^3$  mm is not supported by any observation.