

Geosci. Model Dev. Discuss., referee comment RC3  
<https://doi.org/10.5194/gmd-2022-84-RC3>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## **Comment on gmd-2022-84**

Anonymous Referee #2

---

Referee comment on "FESDIA (v1.0): exploring temporal variations of sediment biogeochemistry under the influence of flood events using numerical modelling" by Stanley I. Nmor et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-84-RC3>, 2022

---

### **Review of FESDIA (v1.0): Exploring temporal variations of sediment biogeochemistry under the influence of flood events using numerical modelling by Stanley I. Nmor et al.**

The consequences of floods on the chemistry of solids and porewater in sediments of the Rhône River delta have been reported by previous investigators. Here, the authors build up on previous developments of the OMEXDIA model, updated recently by De Borger et al. (2021, <https://doi.org/10.5194/gmd-15-2105-2022>) to represent the effects of bottom trawling as a process that removes an upper layer of sediment. The present study aims at simulating a conceptually opposite process: (1) a flood that adds an upper layer of sediment. Another new addition is (2) the presence of new metabolic pathways involving S and Fe, and (3) the introduction of an enrichment factor, denoted alpha, that aims to account for the fact that deposited sediments can be either depleted or enriched in reactive carbon depending on the flood type and on the nature of sediments transported. Besides these three aspects, the model does not seem to introduce any update compared to earlier versions. The text also suffers from a lack of clarity and structure, inconsistency in the names of variables, and between parameter values reported in the text and in tables. Even though the parts about relaxation time and carbon enrichment were novel and interesting, particularly in the context of the Rhône delta flood case study, the manuscript is overall hard to follow. I have a few general comments and a list of specific comments for improvement.

#### **General comments:**

- Rassmann et al. (2016 <https://doi.org/10.5194/bg-13-5379-2016>, 2020 <https://doi.org/10.5194/bg-17-13-2020>) described Rhône river delta sediments rich in calcium carbonates, and reported signs of the various reactions associated with calcium carbonates occurring in those sediments. If the focus here is Rhône river delta sediments, why not including any calcium carbonate species in the current model?
- Section 2.2.6: this seems to be the most important section in terms of model development, but it is also the part I had the hardest time to follow. There are a lot of new terms introduced here and they are not well defined. The second paragraph starts to explain how post-flood organic carbon contents are derived, then mentions the solutes, then goes back to post-flood organic carbon content with Eq.(15)... I suggest putting more effort clarifying this section, defining terms with precise and consistent words, in order not to confuse the reader. Specifically, what is the carbon enrichment factor (confac) exactly, and how does it differ from the proportionality constant (pfast)? What is Cflood/org and how does it differ from TOC (both are present in Eq. (15)? Is confac tuned for each simulation or is it constant? Is pfast tuned for each simulation or is it constant?
- It is stated L187-188 that "For dynamic simulation,  $w$  can change as a function of time but in most cases we assumed a constant value." In which cases exactly was  $w$  changing? Changing  $w$  in all cases seem like a necessity given that the novelty of the model is to simulate events in which the flux of deposited material (thus  $w$ ) is strongly changing with time. How can a constant  $w$  be appropriate to simulate a flood?  $w$  also changes with sediment depth, because of chemical reactions occurring within the sediment (see Munhoven, 2021 <https://doi.org/10.5194/gmd-14-3603-2021>). Can the authors either better justify their choice of a non-changing  $w$  or update that in the model simulations?
- Section 2.2.7: in most O<sub>2</sub> and pH microprofiles from the Rhône delta presented in Rassmann et al. (2016) we can see the influence of a diffusive boundary layer. Please discuss and justify the absence of diffusive boundary layer control on solutes as an upper boundary condition, or update the upper boundary condition accordingly to include this, as other models do in a simple manner (Boudreau et al., 1996 [https://doi.org/10.1016/0098-3004\(95\)00115-8](https://doi.org/10.1016/0098-3004(95)00115-8); Munhoven, 2021 <https://doi.org/10.5194/gmd-14-3603-2021>; Sulpis et al., 2022 <https://doi.org/10.5194/gmd-15-2105-2022>).
- There are a lot of inconsistencies between number reported in the text and those in the tables ( $w$ , NC ratio,  $r_{slow}$ , bottom boundary conditions). Please update and be consistent.

### Specific comments:

Shouldn't "Rhône" be spelled "Rhône", even in English language?

### Abstract

L21-24: Here the enrichment factor alpha is mentioned but not clearly defined. This is confusing. Please update.

## **Introduction**

L35: The use of the acronym RiOmar is not really needed, since only used once after. In general, avoid unnecessary acronyms.

L36: Although more commonly used, POC is also an unnecessary acronym here, since only used once after.

L36-39: The sentence is unclear. "because it serves as a sink for particulate organic carbon and nutrients as well as an intense site of carbon and nutrient": what is the "it" referring to?

L40: I am not convinced that all the cited models have time-dependent capabilities, unlike several other, more recent models, published in this journal that explicitly do. Please update the list.

L43: "massive episodic events" could refer to lots of processes, please be more specific.

L47-50: Sentence unclear. "Attempts to use mathematical models to understand perturbation-induced events on early diagenetic processes have resulted in a variety of approaches that incorporate this type of local phenomenon.": what is "this type of local phenomenon" referring to?

L48-50: "As an example, previous research in deep-sea systems suggests that megafaunal perturbation can cause a 35% increase in silicic flux when compared to steady-state estimates (Rabouille and Gaillard, 1990)" this is interesting but this level of precision seems unnecessary, what is the relevance for this study? Besides, what is a "silicic flux"? In which direction is the mentioned flux going?

L50: What is the "redox boundary"?

L52-53: What does the "redistribution of solid-phase manganese with multiple peaks" mean?

L62: "porewater species like oxygen (O<sub>2</sub>) can be restored after a few months": it is unclear. Do you mean that porewater concentrations can be restored to their pre-flood levels?

L66: what does "short-lived species" mean?

L67: DIC is a component, not a species.

## **Materials and methods**

L93: "the organic matter delivered reflects the Rhone River inputs (Lansard et al., 2008; Cathalot et al., 2013)", in terms of what? Composition? Reactivity?

Fig.1: I assume that the dashed and solid gray linings shown on the map depict bathymetry; it would be useful to precise it in a caption/legend

L107: what does "mode of behavior" mean?

L140: how exactly do "the reactivities decrease with depth" in the present model? From Table S1, it seems that the reactivities are constant.

L140: The sentence formulation is awkward: it is the degradation that would "cease", not its rate. Saying this also slightly exaggerated, degradation rates become indeed very small deep below the sediment-water interface but they are never really equal to zero (e.g. Bradley et al., 2020 <https://doi.org/10.1126/sciadv.aba0697>).

Eq. (7): What are FeS<sub>pro</sub> and H<sub>2</sub>S<sub>oxid</sub>? Are they different from the FeS and the H<sub>2</sub>S produced by the reactions shown in Eq. (5)? Why is one rate a capital R and the other a lower case r?

Eq. (9): What is the value of k<sub>ads</sub> and can you give some information on this aspect of the model?

L216-220: How is irrigation implemented into the model, i.e., where does it appear in Eqs. (8 & 9)?

L236: What is a "time run"?

Eq. (15) Please precise here that  $\text{TOC}_{\text{old}}$  is the TOC concentration at the old sediment-water interface

L248: it would be good to have more information on  $\alpha$  (alpha): here it is tuned. How should it be used in future applications? Always to the same value? Does its value depends on type and magnitude of flood?

Fig.2: change "reactive Corg" for the notation " $C^{\text{fast}}_{\text{org}}$ " for consistency

Table 2: the value for  $r_{\text{slow}}$  is  $0.0 \text{ d}^{-1}$ , but in the text it is indicated as  $0.0031 \text{ d}^{-1}$ . Please clarify that

Section 2.2.7: what about bottom boundary conditions? Is the concentration really set to 0 for all species, as indicated in Table S1? That would seem unjustified.

L289: By sedimentation rate do you mean solid burial velocity? Porewater burial velocity? Both should be different because porosity is not constant with depth.

L289: Is  $w$  0.027 or 0.03 cm per day? Be consistent between the text and tables.

L283: Why a different NC ratio for both organic matter fractions? How were the values of 0.14 and 0.1 obtained? Why are these values different from those shown in Table 2?

L306: Can you provide details (i.e., show the formula) on how are equations 8-10 integrated?

L306: Please provide guidance on what  $\Delta t$  values should users set depending on the simulation

L313: First time the “mix” perturbation is mentioned. What is that?

Eq.(22) Why is it summed over the total number of grid points? Any perturbation following a flood should be the highest near the sediment-water interface, so wouldn't using data coming from deeper in the sediment to compute the relaxation time dilute the true signal and induce additional uncertainties?

## Results

Fig4: what is the alpha value for the slow organic carbon fraction?

L409-412: Can the authors interpret the mismatch between modelled and observed SO<sub>4</sub>, DIC and NH<sub>4</sub> values at depth? Wouldn't that argue for overestimated organic carbon reactivities at depth?

Table 3: How can there be an oxygen flux to the sediment that is ten times smaller than a DIC flux from the sediment? Wouldn't a value closer to one be expected?

Fig.9: what is “degradable OM”? does that mean that the alpha value is the same for both fast- and slow-decay organic carbon? If so, precise it.

L524: “mixing events” are again mentioned as something the model is able to simulate, but they are not described earlier, so it is unclear what they are.

Section 4.4: to add to this discussion, and in reference to the mention earlier in the manuscript of a “perturbed trajectory frequently arbitrarily divided into a fast, transient phase and a slow, asymptotic stage”: should we instead think about relaxation time as the time necessary for most of, rather than all, changes to occur, similar to the concept of half-life in radioactivity?

## Reference

[Ait Ballagh et al., 2021] is missing from the list