COMMENT AND RESPONSE FOR REFEREE 2

We thank the reviewer for carefully reading our work and taking time to comment on some of strengths and perceived weaknesses of the paper. Our responses are represented in the red text in reply to the reviewer comment in black.

OVERALL COMMENT

Concerning the overall evaluation of the paper and its novelty, the reviewer acknowledge that novelty is present in this model by the addition of new diagenetic pathways and by the addition of the potential to calculate the effect of the sudden deposition of a new and thick layer of sediment with a different concentration of organic carbon (OC). In the following ways, FESDIA differs therefore from the OMEXDIA model by implementing:

- An explicit description of the anoxic diagenesis beyond the nitrogen cycle including (i) Iron and Sulfur dynamics, (ii) methane production and consumption. In comparison OMEXDIA has a single state variable (ODU: oxygen demand unit) to describe reduced species.
- possibility to include sediment perturbation events such abrupt deposition of sediment. This is a crucial addition that makes the model suited for its intended application.

In this paper, we only discuss part of the FESDIA model concern with implementation of a perturbation event as it relates to some biogeochemical indicators. The model is implemented in Fortran (for speed) and linked to R (for flexibility).

In regard to the paper structure, efforts have been made to improve on its clarity, and flow as a result of the reviewer feedback.

GENERAL COMMENT

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?

Previous work on Rhône river delta sediments has revealed that the sediment contains calcium carbonates, which may have justified the inclusion of calcium carbonate in the model. However, Rassmann et al 2020 shown that the effect of calcite formation on the DIC concentrations is between can be at most below 15%. Carbonate system models have been discussed as computationally demanding (Boudreau, 1997; Hoffmann et al., 2008). In this paper and for this first version of the model, we intend to consider first order effects of flood deposition on the profiles, as well as to highlight the possibility of incorporating diagenesis changes in the sediment when including abrupt discontinuities. While other community tools, such as CANDI (Boudreau et al., 1996), MEDUSA (Munhoven, 2021) and the recently published RADI model (Sulpis et al., 2021), include the carbonate system, our aim here is to provide tools with the ability to study perturbational dynamics, such as sudden depositional events. Efforts will undoubtedly be made in the future to integrate the complexity of the carbonate system into flood deposition routine like FESDIA.

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.

We have modified this section and its organization as suggested. Without repeating the full text, the following snippet capture the changes we made with discussion of the deposition effect on the solids first and then the solutes:

"…The event calculation was carried dynamically within the same time run. For the solid species, following the flux of organic carbon via the boundary condition (see section 2.2.7), the portion of organic carbon is split between the fast and slow decaying component using a proportionality constant ($pfast$) as in Ait Ballagh et al. (2021). $pfast$ varies from 0 to 1 and it is express in percentage of carbon flux deposited associated to either fraction (fast and slow). However, at the time when the event is prescribed, the integrated profile of the solid species C_{org}^{fast} and C_{org}^{slow} from previous time step, defined as (t^{-}), was used to create a virtual composite of the deposited layer. This integral calculation was performed over a specified sediment thickness (Z_{pert}), which corresponded to the vertical extent of the depositional event. This average concentration for the solid, which we define exclusively for the time of deposition as (C_{org}^{flood}) is scaled with an enrichment factor (α) see below) and then nudged on top of the old layer which is supposed to be buried beneath after the event…

…The carbon enrichment factor (α) in the model (confac in the model code) is introduced here in order to scale the deposited OC with those observed from field data. This helps in calibrating the deposited organic matter concentration (C_{org}^{fast} and C_{org}^{slow}) in the new layer relative to the previous sediment fraction, simulating the wide range of TOC content observed in the field. For instance, when the newly deposited organic matter is similar to the former sediment topmost layer (average preflood layer concentration over an equivalent Z_{pert} depth), an (α) value of 1:1 is used. If the new material is lower in organic carbon content compared to what is near the sediment-water interface, then (α) < 1, while if the newly deposited material is higher in carbon content than the sediment surface, (α) > 1. This flexibility can be used to constrain the simulation to match the corresponding TOC profile from field observation. In modeling application, this parameter is generally specified by using different value for the magnitude of OC in each fraction depending on the empirical observation of the TOC data. This

quantity is therefore tunable and the upper bound of this parameter is dictated by the maximum TOC in the sediment sample…

Specifically, what is the carbon enrichment factor (confac) exactly, and how does it differ from the proportionality constant (pfast)?

"…It is important to note that this parameter differs from $pfast$. This OC flux partitioning by $pfast$ occurs regardless of the event and it is related to the carbon flux received at the boundary, but the carbon enrichment factor occurs only during the event. The Carbon enrichment factor (α) can be viewed as a method of imposing a new initial condition only at the time of the event by using the integral concentration from the previous time. However, using the approach described here, all calculations can be done dynamically without stopping the model.

For the solutes (O_2, NO_3^-, NH_4^+, DIC, SO_4^{2-}), the bottom water concentration is imposed through the perturbed layer at the time of event by assuming this new layer is homogenously mixed…"

What is C_{org}^{flood} 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?

C_{org}^{flood} is only a notational term defined for when the event occurs and helps to distinguish Corg (which we used consistently throughout the manuscript). It also differs from TOC. TOC, as defined in Eq 21, is not a modelled variable (not a state variable), rather, it is determined by the fast and slow degradable carbon. ($\mathrm{C}_{\mathrm{org}}^{\mathrm{fast}}$ and $\mathrm{C}_{\mathrm{org}}^{\mathrm{slow}}$) as well as considering for the refractory background carbon ($\mathrm{C}_{\mathrm{ref}}$).

Is confac tuned for each simulation or is it constant?

For any simulation, confac (α) is changed to account for targeted variations in deposited carbon concentration during the flood. In the revised paper, A statement have been added to clearly indicate that this parameter is application specific and can be tailored to the data at hand.

Is pfast tuned for each simulation or is it constant?

For the majority of the applications presented in the paper, the pfast is constant, drawing heavily on Pastor et al., 2011's and previous modelling optimization of this parameter to data in the study region.

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?

Given that the sedimentation rate in the Rhône prodelta can range from 10 cm to 41 cm/y and flood deposition can deliver about 30 cm in a few days (about 5 days), the burial rate in the non-flood period can be assumed to be constant (i.e., evenly distributed through the rest of the year). Because the deposition of the flood layer is treated separately by re-adjusting the depth profiles with the inclusion of the flood layer, the main variation of accumulation rate is already taken into account. Furthermore, because our model heavily borrows from OMEXDIA, where the flux of OC is decoupled from the advection rates (Soetaert et al., 1996a,b), we anticipate that the inclusion of time varying w will be marginal for the single flood application.

FESDIA model was designed in such a way that time-varying sedimentation is possible. The function FESDIAperturb() (discussed in section 2.2.9) has an argument (wForc) that can be given a functional time series or imposed as observational data for the sedimentation rates (w) if available. In the revised manuscript, we now highlight this possibility to potential readers/users.

w also changes with sediment depth, because of chemical reactions occurring within the sediment

Some modelling paper has alluded to this possibility (Munhoven 2021). However, the current version of FESDIA does not considered the vertical variation of w due to chemical reactions. Given that the flood input can be as high as 30 cm in some of this massive sediment depositional event, the change in advection rate due to chemical change is negligible in comparison. In addition, Munhoven 2021, noted that inclusion of this chemically induced change in advection rate required the formulation of a volumeless solid component which can results to “physically unrealistic transport” and so be “required only if necessary”.

Section 2.2.7: in most O2 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; Munhoven, 2021 https://doi.org/10.5194/gmd-14-3603-2021; Sulpis et al., 2022 https://doi.org/10.5194/gmd-15-2105-2022).

Including DBL in the model, particularly when it comes to O_2 and pH in the upper sediment can be necessary to calculate the benthic oxygen demand or to define accurately oxygen top boundary condition. However, a key focus of this paper was attempting to showcase approaches to establish an estimate for the relaxation timescale for dissolved chemical species whose zone of action occurs deeper in the sediment. Thus, the DBL zone was excluded from the model and its use-case scenarios as presented here. As evidenced by our findings, solutes such as SO_4^{2-} and DIC are prominent examples where this assumption can be made. This simplification is consistent with previous research on the role of DBL in sediment flux and reactions (Boudreau & Jorgensen, 2001, Chapter 9), which proposed that the importance of DBL in controlling diagenetic processes and fluxes is determined by the relative ratio of DBL thickness (δ_d) to the depth of solute change in the sediment (L) (Fig 9.9 in Boudreau & Jorgensen, 2001). For species such as SO_4^{2-} and DIC, where the depth of diagenesis change (L) is greater than the average thickness of DBL as found region close to the Rhône prodelta (0.12 cm, Sulpis, et al 2018), DBL will play only a minor role in controlling the flux and relaxation timescale.

However, by omitting DBL for solutes such as oxygen where $\delta_d/L >> 1$, our model may overestimate the fluxes across the SWI. By varying the bottom water concentration accordingly to this DBL effect, calculated relaxation time for dissolved oxygen was longer of 1 day at most and do not really modify previous conclusion. Comments on the absence of DBL is now included in the discussion section where we now added a subsection on FESDIA limitations.

There are a lot of inconsistencies between number reported in the text and those in the tables (w, NC ratio, rslow, bottom boundary conditions). Please update and be consistent.

This is now corrected in the revised version.

SPECIFIC COMMENTS:

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

This is now corrected in the revised manuscript.

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

See previous comment above. The revised manuscript has been updated accordingly.

Introduction

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

This acronym was used because other authors and potential readers might be familiar with this naming international convention. Acronym has been removed but "reference to river dominated ocean margins kept".

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

POC has been removed.

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?

The sentence has been rephrased to: … *The fate of organic matter derived from riverine input to the sediment is of biogeochemical importance in coastal marine systems (Cai, 2011). This coastal environment serves as a sink for particulate organic carbon and nutrients, as well as an active site of carbon and nutrient remineralization (Burdige, 2005; McKee et al., 2004; Sundby, 2006)…*"

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.

We are aware of some new modelling framework and tools published especially in this journal and the list will be updated.

(Lasaga and Holland, 1976; Rabouille and Gaillard, 1991; Boudreau, 1996; Soetaert et al., 1996; Rabouille et al., 2001a; Archer et al., 2002; Couture et al., 2010; Yakushev et al., 2017, Munhoven, et al, 2021, Sulpis et al 2022)

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

The sentence has been replaced with:

"*However, because of the scarcity of observations and their unpredictability, the role of massive deposit of sediment in these early diagenesis models has frequently been overlooked*".

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?

The sentence has been rephrased to:

"*…Attempts to use mathematical models to understand perturbation-induced events such as sudden erosion/resuspension event, bottom trawling, and turbidity driven sediment deposition on early diagenetic processes have resulted in a variety of approaches that incorporate this type of phenomenon…*"

The word "local" has been removed from the sentence in the revised manuscript.

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?

This part has been removed.

L50: What is the "redox boundary"

According to Katsev et al. (2006), the redox boundary is defined as the depth zone beneath the sediment-water interface that separates the stability fields of the oxidized and reduced species of a given redox couple. In order to emphasize our use of the word in the text, we have provided a brief definition in the revised paper.

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

According to Katsev et al. (2006), the presence of temporal variation in the organic matter flux of Deep Artic sediment can result in a shift in the depth horizon of the so-called "redox boundary" (see definition above). This redox shift, along with the associated depth zone of oxidation and reduction, can influence where the deep manganese peak forms. That is what the paper (Katsev et al 2006) intends to convey, hence our use of the phrase "*redistribution of solid-phase manganese with multiple peaks*." To communicate this idea more clearly, the sentence has been restructured in the revised paper.

L62: "porewater species like oxygen (O2) can be restored after a few months": it is unclear. Do you mean that porewater concentrations can be restored to their pre-flood levels?

O_2 data indicate that their relaxation time is short (days to weeks at most), and that in the absence of another successive massive deposition over a short period of time, they can return to pre-flood levels. This is supported by data from multiple campaign observations (Rassmann et al., 2020).

The sentence has been rewritten "Vertical distribution of porewater species like oxygen (O_2), can be restored after a few days"

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

The short-lived species have been replaced with "species with short relaxation time"'.

L67: DIC is a component, not a species.

This has been corrected in revised manuscript.

Materials and methods

L93: "the organic matter delivered reflects the Rhône 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

Organic matter delivered in this context is defined by its "composition." The Rhône prodelta, acting as a depocenter, can transport materials of various source compositions from the terrestrial domain (Pastor et al., 2018). However, the reactivity of the composite deposited sediment is not well defined in this area (we provided a comment on this in the discussion L599-L602). In the revised paper we will endeavour to emphasize the "compositional" distinction.

The caption/legend for the map will be included in the revised text.

L107: what does “mode of behavior” mean?

It has been deleted and replace with: "their evolution following the event…"

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

The proportion of reactive carbon to refractory carbon decreases with depth and reaction rate changes accordingly. This sentence has been rewritten in the next version.

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).

We have rephrased this sentence in the revised manuscript.

Eq. (7): What are FeSpro and H2Soxid? Are they different from the FeS and the H2S produced by the reactions shown in Eq. (5)?

We have corrected this inconsistency. The FeSprod and H2Soxid are names of the reaction terms as used in the code.

Why is one rate a capital R and the other a lower case r?

Text formatting issue with Microsoft when we translated our text from latex to Word. Corrected in the revised manuscript.

Eq. (9): What is the value of kads and can you give some information on this aspect of the model?

Kads is dimensionless quantity. In the revised we shall add:

“…*With special consideration of ammonium adsorption to sediment particles, the governing equation is given by:*

$$\frac{\partial \phi C}{\partial t} = -\frac{\partial}{\partial z}\left[-\frac{\phi \times D_{sed}}{(1+k_{ads})} \times \frac{\partial C}{\partial z} + w_{\infty} \times \phi_{\infty} \times C\right] + \sum \frac{\phi \times REAC}{(1+k_{ads})} \qquad (9)$$

where we assumed that the immobilization of NH_4^+ *is in instantaneous, local equilibrium (i.e. Any changes caused by the slow* NH_4^+ *removal process results in an immediate adjustment of the* NH_4^+ *equilibrium; so, can be modelled with a simple chemical species) and* k_{ads} *is the adsorption coefficient. The inclusion of this formulation for the diffusion and reaction term has the effect of slowing down ammonium migration in sediment. Derivation of this formulation is given in Berner, 1980; Soetaert & Herman, 2009…*“

Kads has a value of 1.3 (Soetaert et al., 1996a) and is now include in the table of the supplementary text.

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

In the revised text, more detailed have been added. "Bio"-irrigation is implemented as a non-local transport term and contained in the REAC term in Eqs. 8 & 9.

L236: What is a “time run”?

Time is the duration of the model simulation. In the revised manuscript, this “time run” phrase has been clarified.

Eq. (15) Please precise here that TOCold is the TOC concentration at the old sedimentwater interface

This clarification has been added in the revised manuscript.

L248: it would be good to have more information on confac (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?

See previous comment above. The concfac is tunable and we commented briefly on this parameter in section 4.2 L646 – “*This difference in characteristics was partially imposed in our study by assuming variations of* α *in the new deposit. The empirical observation of sediment characteristics associated with flood input dictates this parametric turning to match the TOC characteristics. …However, more data from the field and laboratory experiments that resolve the OM composition of flood deposits are required to constrain the choice of this numerical parameter*.”

The parameter has been created to simulate organic enrichment depending on the magnitude and type of flood. It can be optimized to fit the shape and distinct characteristics of the TOC profile.

Fig.2: change “reactive Corg” for the notation “C (superscript)fast (subscript)org” for consistency

Label was changed in the Figure accordingly.

Table 2: the value for rslow is 0.0 d-1, but in the text it is indicated as 0.0031 d-1. Please clarify that

It was truncated during the alignment of the table to fit the page width. This was fixed in the revised manuscript.

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.

We have removed the useless dw parameters from table since all the bottom boundary condition are same and given as zero flux boundary.

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

As sediment compaction is not included in the model, the advection rate for both solid and solutes are the same. In addition, for solutes, the effect of diffusion is order magnitude greater than transport due to porewater burial. This distinction has been made clear in the revised paper.

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

The value was automatically rounded when fitting the table to page. We have updated the value in the text and table.

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?

Table 2 have been corrected. The two NC ratio were derived from Ait-Ballagh et al., 2020 optimized fit to data in the Rhône prodelta.

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

A new section will be added to that effect.

A snippet in this subsection is as follow:

"Because the procedure is based on OMEXDIA, complete details of the derivation can be found in that paper and referenced therein (Soetaert et al., 1996). Here we recap the mathematical formulation of the method-of-lines (MOL) algorithm used by FESDIA. Direct differencing of (8) - (10) results to:

$$\begin{aligned}\frac{\partial C_i}{\partial t} &= \frac{\Phi_{i,i+1} D_{\Phi_{i,i+1}} (C_{i+1} - C_i)}{\Phi_i \Delta x_{i,i+1} \Delta x_i} - \\ &w_\infty \Phi_\infty \frac{\alpha_{i,i+1} C_i + \left(1 - \alpha_{i,i+1}\right) C_{i+1}}{\Phi_i \Delta x_i} - \\ &\frac{\Phi_{i-1,i} D_{\Phi_{i-1,i}} (C_i - C_{i-1})}{\Phi_i \Delta x_{i-1,i} \Delta x_i} + \\ &w_\infty \Phi_\infty \frac{\alpha_{i-1,i} C_{i-1} + \left(1 - \alpha_{i-1,i}\right) C_i}{\Phi_i \Delta x_i} \qquad (22)\end{aligned}$$

for a generic tracer C with a phase properties index Φ and D_Φ denoting porosity and dispersive mixing term respectively for solid or liquid. This equation is calculated such that the variables and parameters are defined both at the centre of each layer x_i and at the interface between layers ($x_{i,i+1}, x_{i,i+1}$). The position at the centre of the grid is then given as $x_i = \frac{x_{i-1,i} + x_{i+1,i}}{2}$. Δx_i represents the thickness of the i-layer and $\Delta x_{i,i+1}$ is the distance between two consecutive grid layers. A Fiadeiro scheme (Fiadeiro and Veronis, 1977) based on the model's Peclet number (a dimensionless ratio expressing the relative *importance of advective over dispersive processes) is used to provide a weighted difference of the transport terms in order to reduce numerical dispersion.*

Equations (8)-(10) implemented as Eq. (22) is integrated in time using an implicit solver, called lsodes, that is part of the ODEPACK solvers (Hindmarsh, 1983). This solver uses a backward differentiation method (BDF); it has an adaptive time step, and is designed for solving systems of ordinary differential equations where the Jacobian matrix has an arbitrary sparse structure."

L306: Please provide guidance on what dt values should users set depending on the simulation

The solver internal timestep dt value is adaptive as explained above to ensure stability, consistency of solution and overcome model stiffness (Soetaert et al., 2010; Petzold 1983). The user dt for model is set based on the time resolution for which the processes of interest are needed. We have updated the manuscript to reflect this suggestion for more guidance.

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

It has been removed.

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?

The reviewer made an excellent point about the summation over the grid layer, which corresponds to some of our internal discussions while developing this metric. However, decision was made based on the following factors.

The perturbation signal can indeed be strongest near the sediment-water interface. This is true, however, only if the perturbation is small in size (thickness). Given the high variability of flood deposition seen from: Deflandre et al., 2002, Tesi et al 2012, Pastor et al., 2018 - such a "close-to-surface" assumption is not rarely observed. Furthermore, the perturbation signature spreads to deeper layer as the solute reprofile begins to reorganize. This reorganization is frequently nonlinear, and integrating over a limited domain may result in an incorrect relaxation timescale estimate, particularly when large perturbations occur, as shown in Figure 8. We argue in section 4.3 that this large depth interval related to their timescale for some species. For all these reasons, the whole domain it included in the calculation.

Results

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

The α value chosen in this scenario was 10. This information has been included in the figure caption.

L409-412: Can the authors interpret the mismatch between modelled and observed SO4, DIC and NH4 values at depth? Wouldn't that argue for overestimated organic carbon reactivities at depth?

The figure in submitted was fitted using parameter from Pastor et al., 2011, Ait-Ballagh et al., 2021 where advection rate is w_∞= 0.03 cm/d. The figure represents an independent model validation without further calibration. With a slightly higher advection rate (w_∞ = 0.05 cm/d), a better fit to the data can be reproduced. However, the final relaxation time using this modified advection rate stays within the uncertainty already shown in the manuscript. The revised manuscript was updated accordingly.

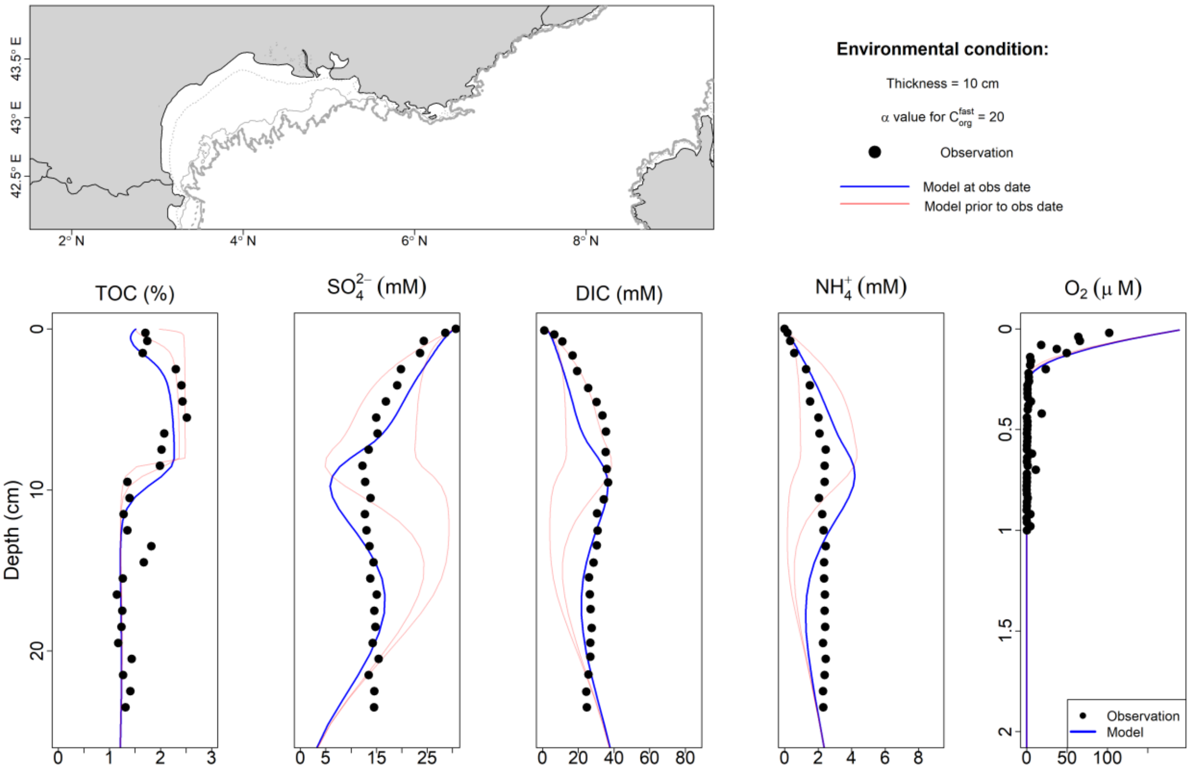

Table 3: How can there be an oxygen flux to the sediment that is ten times smaller than a DIC flux from the sediment? Wouldn't a value closer to one be expected?

The O_2 and DIC fluxes are different as supported by measurement from Rassman et al 2020, where DIC fluxes are 8 times higher than dissolved oxygen uptake (DOU). In the proximal station considered here, anaerobic pathways are dominant in comparison to the aerobic one (Pastor et al 2011). Hence, the DIC flux is expected to be significantly higher than the O_2 flux and not expected to be follow a 1:1 relationship.

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.

It should be notable that both fraction of OM is degradable here. Only their timescale of degradation change. The α value was chosen to span the spectrum of what might be consider rich OC deposit and poor OC deposit.

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.

The “mixing event” has been removed. This has been corrected to “deposition events”

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?

That is another perspective we can consider as related to the relaxation timescale. However, it should be noted that this is predicated on the assumption that the curve is exactly exponential, which is not always the case in the simulated data we computed. With an analytical exponential fit, we can capture the main trend of the curve but it is difficult to estimate the time point when the event disappears.

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

This has been updated in the revised manuscript.

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