

Interactive comment on “Model predictions of long-lived storage of organic carbon in river deposits” by Mark A. Torres et al.

Anonymous Referee #1

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This manuscript by Torres et al. addresses the role of fluvial sediment transport on the storage and age of river particulate organic carbon (POC). To this aim, it combines an existing numerical model for the evolution of alluvial deposits associated to a meandering river, together with a simplified model for the dynamics of POC in a given sedimentary deposit. Based on these results from numerical simulations, along with considerations on key dimensionless parameters of the model and a compilation of literature data on “biospheric” POC ages, the authors show that river sediment transport dynamics is likely to play a significant role in setting river POC age, and make some predictions on how the interplay between sedimentary and POC dynamics might influence observed biospheric POC ages. This leads to a set of suggestions on how to interpret existing and future field data on river POC in terms of sediment / terrestrial

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POC dynamics.

I found this paper very interesting and highly relevant to a large, active research community working on riverine POC and more generally to those using organic sedimentary archives in detrital sediments. It also opens some new perspective for the dynamics of other non-conservative compounds transported by rivers, by setting up an interesting framework. In general I do not have any strong concern regarding the approach nor the conclusions reached by the authors. I have several comments (appended below) mostly regarding missing information or unclear statements, but altogether I recommend publication of this article in Earth Surface Dynamics.

I. 9-10: I find this statement (“sediment transport [...] terrestrial realm”) a bit weird. Storage in upland soils is clearly also a big player in setting the maximum time OC persists on land. Although I understand what the authors mean, I do not see why rivers “define” this maximum time, more than upland soils do, for example. I suggest rephrasing.

I. 16 and 77: Doesn’t “ameliorates” imply an improvement? If yes, and as I do not see why such “judgmental” word would be needed here, I would rather suggest “modulates”, or more simply “affects [too]”.

I. 113: Add (if correct) after “transit time distribution”: “of this collection of grains”.

I. 113-114: I think the physical reason why such transit time distribution is mathematically the result of “n” convolutions of the storage distribution deserves to be more explicitly stated, to keep most readers on board.

Equation 2: First, this equation does not make much sense mathematically speaking. If $\hat{14}C = f(t)$ (I. 116-117), in no way can $f^{-1}(t)$ be written (f^{-1} [provided that it can be defined, which requires the $f(t)$ to be monotonic, by the way, which is not necessarily the case for all $C = f(t)$ functions - depending on POC dynamics - although it is the case in the present paper] is a function of $\hat{14}C$ or of something that has the same dimension, at

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least). In addition, p_F and p_{Tr} , as probability distributions, are dimensionless, unlike $dt/d^{14}C$: eq. (2) therefore has a unit issue. Second, for the sake of clarity, I think the reason why the derivative term appears on the right-hand side should be better explicated, again to allow all the readers to understand what the authors are doing.

l. 147-149: The readers who are not familiar with this model will be interested in knowing how “local and upstream-weighted curvature” influence the local rates of relative channel migration. This is important because later in the paper, it was unclear to me which parameters were directly specified by the authors, and which ones were the result of the model.

l. 154-155: I think the sentence “In the model topology [...] bed elevation change” should be moved to l. 147 (after “discrete nodes”) as it refers to a very general feature of the model that should be given upfront.

l. 158-175: These explanations would benefit from an example of how a model result looks like, at a given time step or at the end of a simulation (this could be added to Fig. 1, the explanatory interest of which is limited). For example, I still wonder whether these are 2D (maps) structures of the river channel and alluvial deposits?

l. 158-188: Reading this, I also wondered how the initial conditions (river channel pathway, initial sediment deposit age distribution...) of the alluvial plain were defined in these simulations. This comment relates to some missing information l. 185-188 regarding “replicate simulations”: how did these replicate simulations differ exactly (e.g. were the initial conditions randomly set for each simulation, and if yes for which subset of parameters)? And why a set of replicate simulations for storage duration distribution and another set for deposit age distribution? Why 5 in the first set and 50 in the second? These replicate numbers come up again l. 425 and l. 428, but not anywhere else.

l. 196: The T_{cut} value of 350 years was obtained from the explicit tracking of meander cut-offs in model runs: is this number actually stable across different replicate

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simulations (whatever the difference between these replicate simulations, see comment above)? Exactly equal to 350 years? It seems that T_{cut} can vary depending on simulation parameters (l. 199-207); so I would guess that this number of 350 years pertains to particular simulation conditions, but this is unclear when reading the article.

l. 216: Add “of” before “two timescales”.

l. 217: “ E_L ” has not been defined at this stage (unlike “ $E_{L,max}$ ”) - it is defined only l. 238. In addition, the way it is retrieved is not clear. I sort of understand that $E_{L,max}$ is specified by the operator, but E_L is measured and is a mean of all values obtained from the model nodes?

l. 218: To me, the fact that “relative T_{cut} values can be determined [...] through comparison of w^3/Q_s ” is implied by the correlation of Fig. 2b, not by the two correlations of Figs. 2a-b.

l. 242: “ n_x ” is not defined anywhere. I guess it is equal to L / x_{tran} ? If yes, such equation should be added (which requires defining L , defined for now not before l. 256).

l. 287: Avoid using “ λ ” (rate constant for radioactive decay of ^{14}C) here to avoid confusion with “ λ ” (wavelength of meander bends) l. 213.

l. 309-310: The concentration of POC does not necessarily have to “increase” with time in a sediment deposit (it will decrease if the initial POC concentration is higher than the steady-state concentration).

l. 316: I think the steady-state value of “ F_m ” is actually $(k^*R)/(\lambda+k)/(^{14}C/^{12}C)_{modern}$ (note the division by “ $(^{14}C/^{12}C)_{modern}$ ” compared to what is stated l. 316). $(k^*R)/(\lambda+k)$ is rather the steady-state value of “ $(^{14}C/^{12}C)_{sample^*}$ ” (following eq. 6).

l. 317: Wouldn’t “a given” be more appropriate than “constant” (as “steady state” implies “constant” by definition)?

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l. 325: Note that eq. 2 refers only to 14C dynamics - POC and Fm also need an equation for 12C. For this sentence to be correct, eq. 2 would need to be generalized (using e.g. $\hat{n}C = f_n(t)$).

l. 345-347: The relative steady-state values of the fast and slow-cycling POC pools also depend on the production rate P (l. 309 and 316).

l. 351: Add a dot after "0.01 yr⁻¹".

l. 352: "affect" -> "effect".

l. 397: I'm not arguing against the use of a distribution law with finite moments, but in a way many river systems will not recycle all the sediments they store (e.g. in case of floodplain subsidence, as acknowledged by the authors l. 603-616), or at least only over time scales which make the present model pretty irrelevant. Maybe this statement should be altered to reflect this fact.

l. 421-422: Isn't this ratio of input to output fluxes equal to 1 at "steady-state" (l. 415)? Or does eq. 15 outside of steady-state (unlike what is suggested by the way this whole paragraph is written)? And what is this "reservoir" in the "total reservoir size"?

l. 519-522: I understand that this should be possible "in principle", but in practice this requires that the parameters relevant to the POC dynamics (k_S , %S. . .) remain the same across the river course. This is an important requirement, which might not be fulfilled in many, relatively large river systems.

l. 603-616: This paragraph points out several limitations of the model, which has to be credited to the authors. However, reading this, I thought that it could be slightly extended to reflect the fact that the model used focuses on only one type of river morphology, namely meandering rivers. Although I agree that this river type is widespread, other types of river morphology exist (and are especially represented in the dataset shown in Table 2: braided rivers in the Ganges-Brahmaputra, straight or only slightly bended rivers with stabilized banks by persistent vegetation in the Amazon. . .). While

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the authors acknowledge the limitations of their model in terms of processes that might take place even in meandering contexts (such as overbank deposition), this reference to other river morphologies (in which these other processes might be even more important) is lacking. Additionally, one could also emphasize that the sediment grain dynamics addressed by their model referring to banks subjected to erosion / deposition along the channel, the corresponding results are most likely most relevant for coarse grain sizes. However, the "bulk" POC characteristics measured in a river sediment - especially if this sediment is transported as suspension - might be more reflective of fine grains (often OC-richer) that are affected by other processes such as overbank deposition. This could result in patterns for POC characteristics partly decoupled from meandering dynamics.

l. 627-669: Compared to the rest of the article, I find this section a bit weak. First, these "findings" are not related to POC dynamics (especially to processes affecting POC along the lowland river course) at all but rather to grain dynamics, right? And as such they also apply to any tracer deemed as conservative, the signal of which is set in upland rivers and not modified in alluvial plains, right? So why focus the message on biomarkers (which is organic carbon, making the whole point a bit misleading. . .)? And anyway, aren't these "findings" (Fig. 6) a bit textbook? I mean that most readers will probably know that convoluting a periodic signal with some filter that has a reasonable frequency distribution, the original signal will be dampened and offset in phase, with strongest modulations obtained for high frequency (compared to some representative metric of the frequency of the filter)? Therefore, I suggest removing this section and Fig. 6.

Fig. 3: In panel a, why are the numerical survivor function data (and the corresponding fit) in red on the graph and in black in the legend? Also, It would be informative to represent other functional forms for the possible fit to the numerical data (e.g. exponential. . .). Finally, The red curve should continue as a flat line below $t/T_{cut} \sim 1.2$, at a value of 1. This is not visible on this figure.

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Fig. 4 caption: I think %S is defined for steady state. Maybe this should be written explicitly.

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