

Response to Thomas Hoffmann - Reviewer 2

The manuscript presents a very valuable and interesting dataset of long-term carbon accumulation in the floodplain of the Lower Danube, which is connected to sea level changes of the Black Sea. The study is based on a solid sampling and analysis design and provides an interesting story of carbon sequestration. Yet, the implications derived from the results should be strengthened. For instance, the authors should make statements about the relevance of their study on the importance of floodplain carbon sequestration in context of river management in the light of the global climate crisis. How should floodplains be managed and can floodplain management be optimized (if yes, in which way) to increase carbon storage in river systems. In general, the paper is well structured and written and of high quality and is a valuable contribution to the journal Biogeosciences. Given the general comments above and the more detailed line-by-line comments below, I suggest publication after minor revision.

We thank the reviewer for this valuable suggestion. We will add a dedicated paragraph to the Conclusions (cross-referenced from Section 5.3.3) making the management relevance of our findings explicit. Specifically, we will argue that: (i) because carbon accumulation rate in the LDF is governed primarily by sediment delivery and accommodation space rather than by organic-matter concentration alone (Section 5.1), restoration measures aimed at increasing floodplain carbon storage should prioritize restoring sediment delivery and hydrological connectivity – e.g. through controlled levee breaches or managed reconnection of embanked sectors – rather than rewetting alone; (ii) the pre-disturbance CAR values documented here for stable, near-equilibrium floodplain conditions ($\sim 20 \text{ g C m}^{-2} \text{ yr}^{-1}$, Phase II) provide a quantitative benchmark against which restoration outcomes can be measured; and (iii) the magnitude and reversibility of the carbon-sink decline we document (Section 5.3.2) show that floodplain carbon function responds dynamically to both base-level and anthropogenic forcing, implying that it can also respond positively to targeted restoration within a human planning timeframe, as demonstrated by the Cosumnes and Camargue case studies already discussed in Section 5.3.3.

General comments:

The age models and derived sedimentation rates of the analysed cores are of fundamental importance for the calculation of the carbon accumulation rates (CAR). The used radiocarbon ages are presented in Tab. 1 of the manuscript. Given the importance of these data and the derived age models, the authors should give more information on this topic. As this is rather a fundamental basis, rather than the core of the topic, the details on the radiocarbon ages (outlier detection) should be documented in a supplementary file attached to the publication. I suggest that the authors provide plotted age models for each analysed core, including the outliers that were not used in this study and the derived sedimentation rates. This will be very valuable information to evaluate the robustness of the derived sedimentation rates. The authors should also give a table (within supplementary information) with the calculated sediment rates, TOC and CAR for each core.

Obviously, a strong focus is given on the stratigraphical analysis of the cores, however, I suggest to more strongly compare the upper and lower part of the study site, or longitudinal differences from up- to downstream. As the upper part of the study site is characterized by higher agricultural impacts and more pronounced anthropogenic stresses, compared to the

lower part of the study site, with more extensive active flooding areas. This is especially valuable for the discussion of potential restoration measures.

The authors relate the observed phases of carbon accumulation rates to the geomorphic setting, the long-term environmental conditions and the human impacts during the Holocene. I have the feeling that the discussion is partially very general and could be more strongly linked to the findings of the study. For instance, the authors state that during the past decades, after the implementation of major dams in the upstream catchment, this resulted in reduced CAR. However, this is not strongly presented in the results. The authors argue that in the upper 50 cm TOC is reduced 2.5-fold. This is in contrast to the general discussion that TOC is not controlled by depth. More in-depth presentation of this result is requested, e.g. by a more detailed comparison of the different cores in different environmental settings. As the potential for TOC storage is also linked to the areal changes of the floodplain, changes of the extent of these settings with time are relevant and could/should be discussed.

We sincerely appreciate the reviewer's positive comments regarding the relevance and importance of our research. We are also grateful for the constructive suggestions, which have helped us substantially improve the clarity and quality of the manuscript.

Detailed comments

Line 48: 0.5–1% to 0.5–8% is not really a convincing disproportionate contribution given the large variability.

We agree that given the wide range of the estimate, the comparison is not convincing. We will rewrite the opening of the Introduction to place floodplains within the broader class of wetlands, for which the contrast between areal extent and carbon storage is well constrained: "Although wetlands, of which river floodplains are an important part, cover only 4–6 % of the Earth's land surface, their soils contain 20–25 % of the world's organic soil carbon (Mitra et al., 2005)." The following sentence will be adjusted accordingly ("This contribution is driven by..."), and the reference added: Mitra, S., Wassmann, R., and Vlek, P. L. G.: An appraisal of global wetland area and its organic carbon stock, *Current Science*, 88, 25–35, 2005.

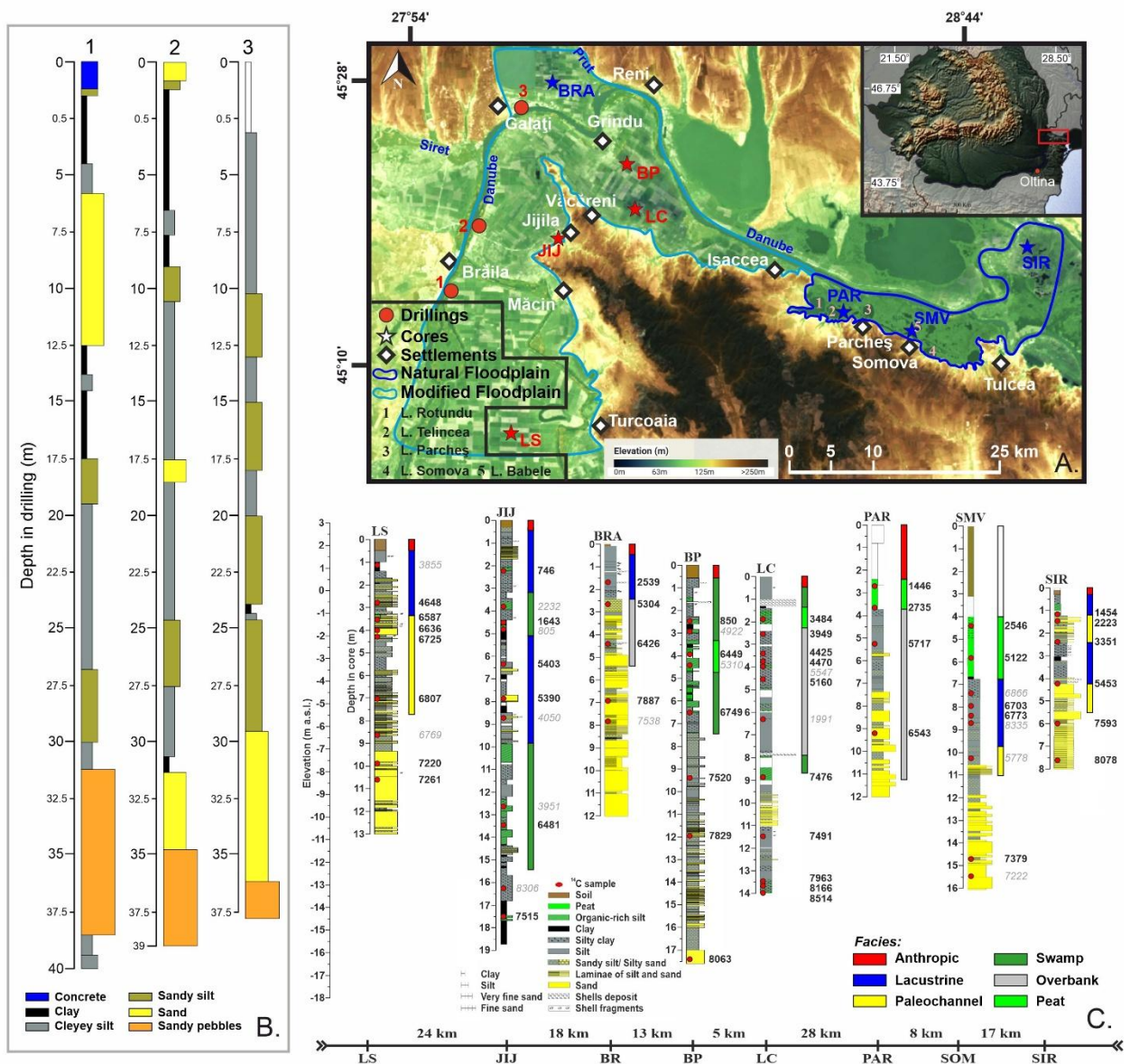
Line 65: Use Hoffmann (2022) (<https://linkinghub.elsevier.com/retrieve/pii/B9780128182345000699>) instead of Hoffmann et al (2013) here.

Thanks. We will update this citation to Hoffmann (2022) as you suggested. The line will become "...or permanently stored within floodplains and deltas (Hoffmann, 2022)." And at the bibliography we will add: Hoffmann, T., 2022. Carbon Sequestration on Floodplains, Second Edi. ed, *Treatise on Geomorphology*. Elsevier. doi:10.1016/B978-0-12-818234-5.00069-9

Figure 1: I am wondering why the cores on the right side are not vertically aligned to depth = 0 m. Does this reflect the topography of the floodplain? If yes, please use scale m a.s.l.; if not please align to depth = 0. Use the same z-scale for each core. Axis labels and legend are very hard to read (too small).

As also noted in our response to Reviewer 1 (comment 4), Fig. 1 has been restructured into three panels: (a) a geomorphological map of the study site on FABDEM Dataset (Hawker, L., Uhe, P., Paulo, L., Jehn, F., Kjeldsen, T. R., & Neal, J. (2022). A 30 m global map of elevation with forests and buildings removed. *Environmental Research Letters*, 17(2), 024016. <https://doi.org/10.1088/1748-9326/ac4d4f>) and Sentinel-2 Data (Satellite Overlay, European Space Agency (ESA). (2023). Copernicus Sentinel-2 MSI Level-2A Surface Reflectance.

Processed on Google Earth Engine), (b) the historical ISPIF boreholes, and (c) our own newly recovered cores. The cores were originally plotted relative to their ground-surface elevations, which vary across the floodplain. To make these elevation differences explicit, we have added a separate elevation scale (m a.s.l.) on the right side of the cores. For the ISPIF drillings, however, we do not have sufficiently precise information on their exact drilling locations and, consequently, the elevation was approximated in the original figure, so we cannot determine a precise ground-surface elevation at each drilling site. Although the potential uncertainty may be only a few tens of centimetres, now we preferred not to introduce potentially inaccurate elevation values in the new version of the figure. Therefore, the drillings have been aligned to a common depth. We have also substantially increased the font size of the axis labels and legend to improve readability, and introduced the calibrated ages from Fig. 2. This elevation treatment applies specifically to panel (c); ISPIF boreholes in panel (b) are aligned to a common reference depth, as explained above and in our response to Reviewer 1, comment 4.



Line 103: change to "... (2) an upper-stream, artificially modified section..."

Thank you. Accepted. The wording has been revised as suggested. The text now reads: “The area can be broadly divided into two sections: (1) a natural floodplain sector (~120 km²), located downstream, between the Danube Delta apex and the Isaccea promontory, characterized by numerous lakes (e.g., Rotundu, Telincea, Parcheș, Somova, and Babele), swamps, and fluvial channels; and (2) an artificially modified section, located upstream, between the Isaccea promontory and Turcoaia village, ...”

Line 107: suspended loads should preferentially be given in Mt/a.

Thank you. We will report the sediment loads in Mt/yr and change the text as follows: “...a solid sediment load of ~19 Mt/yr, markedly lower than the ~57 Mt/yr recorded under semi-natural conditions...”

Line 118: “previously” is misleading in this context, use “before anthropogenic transformation” instead.

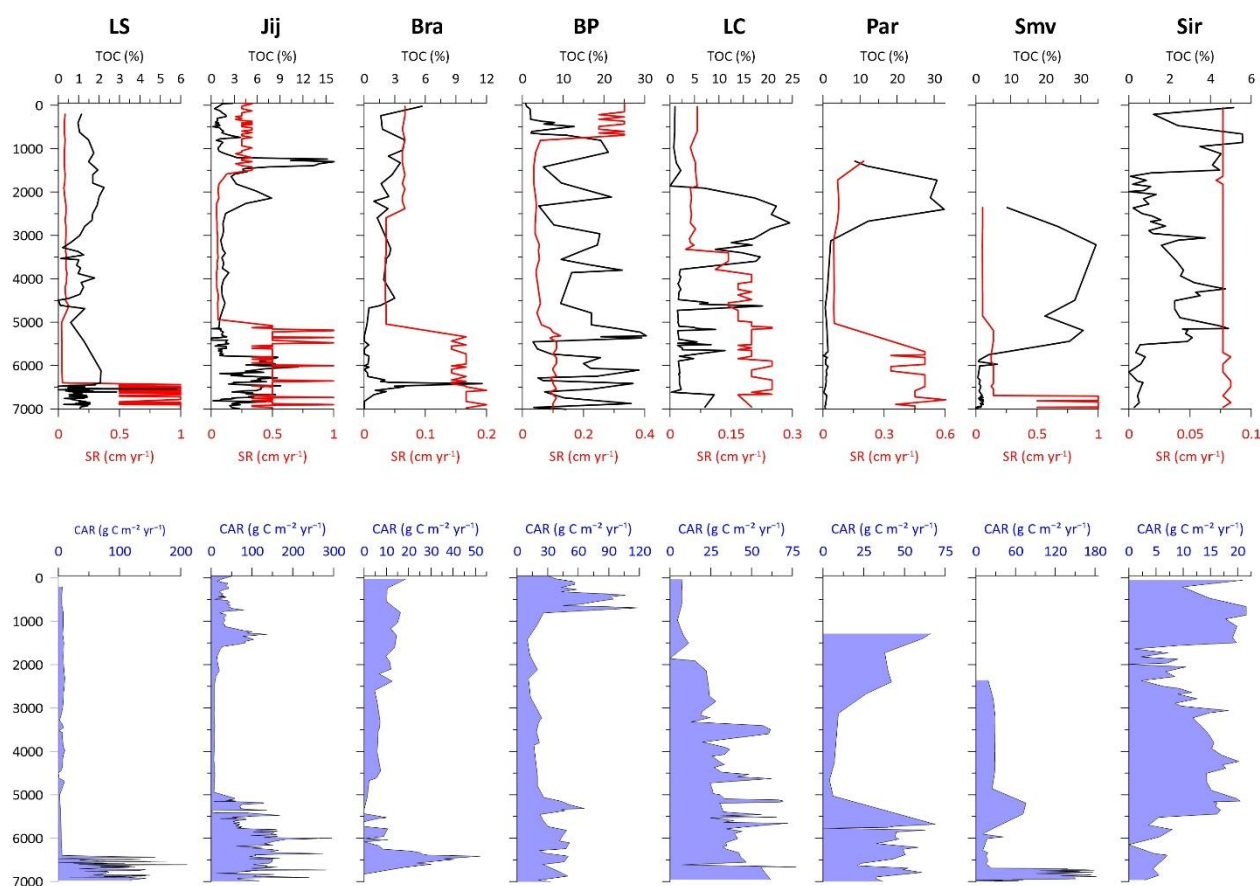
Thank you. We agree that “previously” is misleading in this context and have replaced it to clarify that the description refers to the pre-disturbance landscape. The text will read: “Before anthropogenic transformation, the area was characterized by a mosaic of wetlands, interconnected lakes, swamps, and seasonal channels; the landscape was fundamentally...”

Line 122: Is there any information available on groundwater changes, as this is a crucial factor for carbon conservation/decomposition?

Thank you for highlighting this factor. Unfortunately, no data are available on the historical changes in groundwater levels within the LDF. Therefore, we were unable to assess the role of groundwater dynamics in carbon preservation and decomposition.

Fig. 2: This Figure is very complex. However, not much detail can be derived due to the low quality/resolution of the graphic file. Furthermore, I wonder why the surface (y-axis = 0) of the cores are plotted at different heights? Does this reflect absolute topography? I suggest to plot sedimentation rate in combination with TOC in one graph and CAR for all cores in a comparable way (i.e. time as independent variable) in an extra graph. See also my general comment above.

Thank you. Radiocarbon ages have been relocated to Fig. 1, alongside the stratigraphic logs where they are more directly relevant. Fig. 2 has been restructured into two panels, following your suggestion, both referenced to calibrated age (cal BP) as the common vertical axis rather than physical depth: the top panel shows sedimentation rate (SR, cm yr⁻¹) as a red line profile and TOC (%) as black line profiles for all cores, while the bottom panel shows CAR for all cores as filled/shaded curves. The top-1-m TOC comparison between tillage-affected and underlying undisturbed sediment, which addresses the physical near-surface interval on a depth-based (cm) axis rather than a time-equivalent comparison, has been moved out of Fig. 2 and presented as a separate supplementary figure, to keep Fig. 2 consistent on a single, age-based axis. We will also substantially improve the graphical resolution and readability of the main figure as requested.



Line 146: Clay content is a very good TOC proxy. This should be considered in the analysis.

Thank you. We agree that clay content is a strong and consistent TOC proxy in our dataset (Fig. 4A, $R^2 = 0.46$) and will expand the discussion accordingly. Clay-enriched, fine-grained sediment in the LDF is concentrated in specific facies and time intervals: predominantly in the lacustrine, organic-rich and peat facies (Fig. 5), and, at the millennial scale, in the intermediate unit (5000–2000 cal BP / Phase II, Table 2), when clay content increased to >10% and mean TOC nearly doubled to 8.5%, under low-energy. Coarser, paleochannel and overbank facies, by contrast, store lower clay and TOC.

Importantly, however, this clay–TOC relationship does not translate into higher carbon accumulation rates. The opposite pattern emerges in our data: the clay-/TOC-richest interval (Phase II) has the lowest CAR of the three Holocene phases ($16.1 \text{ g C m}^{-2} \text{ yr}^{-1}$), while Phase I, with markedly lower clay content (<5%) and TOC (4.2%), achieves the highest CAR ($45.6 \text{ g C m}^{-2} \text{ yr}^{-1}$) because of its much higher sedimentation rate (2.9 mm yr^{-1} vs. 0.6 mm yr^{-1}). The same pattern holds at the facies scale (Fig. 5): peat and organic-rich deposits combine the highest TOC with only moderate CAR, whereas paleochannel and overbank facies, despite lower TOC/clay, achieve the highest CAR through rapid mineral sediment burial. We interpret this as evidence that CAR in the LDF is fundamentally controlled by the mass of sediment (mineral + organic) delivered per unit time – i.e., by sedimentation rate and accommodation space – rather than by the concentration of organic carbon (or its clay-associated preservation) within that sediment. Clay content and TOC are therefore best understood as indicators of preservation efficiency and depositional energy, while sedimentation rate remains the primary control on the absolute volume of carbon buried.

To clarify this we will add a paragraph after line 335: “This clay-TOC relationship reflects the concentration of clay-enriched sediment in specific low-energy, standing-water depositional settings – lacustrine, organic-rich, and peat environments – whereas coarser paleochannel and overbank facies carry correspondingly less clay and TOC. At the millennial scale, in the intermediate unit (5000–2000 cal BP, Phase II), clay content rose above 10 % and mean TOC nearly doubled to 8.5 %, coinciding with base-level stabilization and reduced depositional energy (Section 5.2). This close TOC–clay coupling, however, does not translate into higher carbon accumulation: CAR during this phase remains the lowest of the three Holocene intervals ($16.1 \text{ g C m}^{-2} \text{ yr}^{-1}$), well below the $45.6 \text{ g C m}^{-2} \text{ yr}^{-1}$ recorded in Phase I, which, despite much lower clay (<5%) and TOC (4.2 %), benefits from a nearly five-fold higher sedimentation rate (2.9 vs. 0.6 mm yr⁻¹; Table 2). We therefore interpret clay content and TOC concentration as proxies for preservation efficiency rather than as direct predictors of carbon accumulation rate, with sedimentation rate – and, by extension, accommodation space – remaining the primary control on the absolute mass of carbon buried per unit time.”

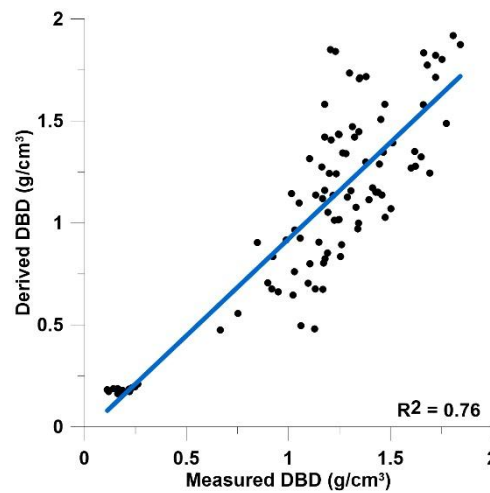
Line 156: bulk density from cored sediment is critical, as sediment in most cores is compacted during drilling, resulting in too-high BDs. Mostly empty space in the upper part of the core is indicative of compaction. Was compaction of the cores considered to correct calculated BDs? Given this issue, a quality assessment / error analysis of BD from cores should be undertaken.

Thank you for raising this important point. Because coring was carried out using opaque PVC liners, compaction or disturbance could not be observed in the field during drilling itself. Signs of compaction/disturbance (e.g., voids/empty space, or deformed contacts) were instead identified subsequently, during core splitting and description in the laboratory. Disturbance was identified in a limited proportion of the total cored length: approximately 20 m out of the 105 m of core recovered across all eight cores, of which only 8 m showed compaction/disturbance exceeding 10% of the metre. Disturbance was concentrated in sediments with higher organic content, with the Jijila (JIJ) and Crapina (LC) cores being the most affected. Critically, the 106 dry-bulk-density samples used to calibrate our regression (Line 161) were deliberately not collected from these visually disturbed metres, and are therefore drawn exclusively from sediment identified as undisturbed. For this reason, we did not apply a post-hoc compaction correction, since the potentially affected intervals were excluded at the sampling stage rather than corrected for after measurement; bulk density was then estimated for all samples, including those from the disturbed intervals, from LOI₅₅₀ with the regression calibrated on these undisturbed measurements, so that drilling compaction is not carried into the CAR values. We will state explicitly in the Methods that the regression was applied to all samples.

Line 161: Were the parameters tested here? What is the RMSE of the regression analysis? I suggest to show the scatter plot and the fitted line.

Thank you. We will report the RMSE and add in the supplementary a scatter plot with the fitted curve, comparing the directly measured bulk density with the bulk density derived from the

Keogh et al. equation, as a supplementary figure. The RMSE of the regression is 0.26 g cm^{-3} , with a coefficient of determination (R^2) of 0.76, as already reported in the text.



Line 169: if TOC as a central parameter is calculated from two estimated values (TC and TIC), which are associated with measurement uncertainties, TOC will have a larger uncertainty than the uncertainty estimates from TC and TIC. Please provide an uncertainty analysis of the estimated TOC values.

Thank you – we agree and have carried out this analysis using our duplicate measurements. For each sample, we calculated the standard deviation of the replicate pair; these individual standard deviations were then combined into a pooled standard deviation for TC and for TIC separately ($pooled\ SD = \sqrt{[\sum SD_i^2 / n]}$), yielding a pooled SD of 0.15 % for TC and 0.05 % for TIC. Since TOC is calculated as $TC - TIC$, the corresponding analytical uncertainty on TOC was propagated in quadrature from these two pooled standard deviations ($\sigma_{TOC} = \sqrt{[\sigma_{TC}^2 + \sigma_{TIC}^2]}$), yielding a propagated analytical uncertainty of $\pm 0.16\%$ for the dataset. We will report this uncertainty explicitly in Section 3.3 alongside the TOC determination method:

“...TOC was calculated by subtracting TIC from TC ($TOC = TC - TIC$) with an analytical uncertainty of $\pm 0.16\%$ for the derived TOC values.”

We note, however, that the TOC values used in the CAR calculation are derived from LOI_{550} through the regression in Section 3.4, whose residual uncertainty ($RMSE \approx 1\%$ TOC) is larger than this analytical uncertainty. The regression uncertainty is therefore the relevant one for CAR (see also our response to Reviewer 1, comment 8).

Line 172: SR needs to be defined.

Thank you. We will explicitly define SR as the sedimentation rate (cm yr^{-1}), derived from the age–depth models (Bacon for LS, JIJ, LC and BP; linear interpolation between dated levels for BRA, PAR, SMV and SIR), as now stated explicitly in the Methods.

Line 177ff: This is indeed a very good correlation. However, I am interested to know the RMSE of predicted TOC values.

Thank you. We calculated the RMSE of the TOC–LOI regression on the held-out validation subset, obtaining a value of 0.96 %. We will report this alongside the already-reported $R^2 = 0.90$ in Section 3.4: “...shows a strong linear correlation ($R^2 = 0.90$), supporting the use of this

equation to extrapolate TOC values from LOI data across the full dataset (Fig. 3). The regression yielded an RMSE of 0.96 percentage points TOC for the independent validation dataset.”

Line 207: Can you specify what “large analytical uncertainties” are? Please give a threshold. The sample LC8, with the 3rd largest analytical uncertainty (71), was not considered an outlier, while many other samples with much smaller uncertainty were. It would be good to plot the age models for each core in the supplementary material, for better documentation of the data. Tab. 1 could then also be moved to the supplement.

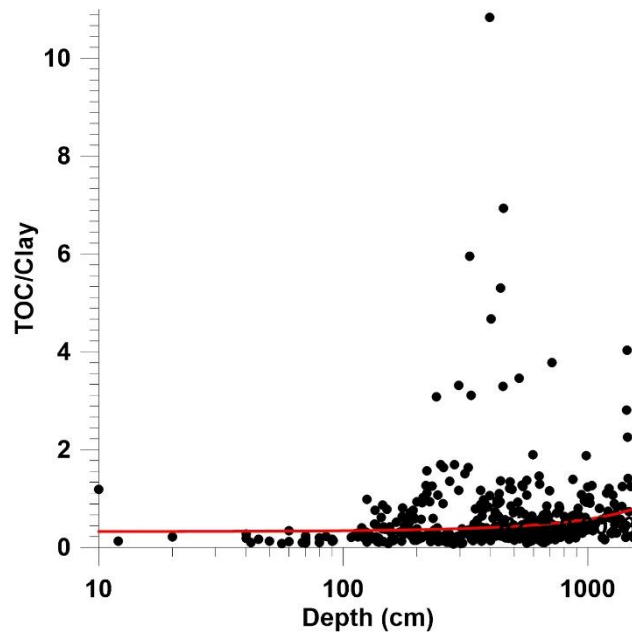
We will define the outlier criteria explicitly and quantitatively in the revised Methods: a date was flagged as an outlier if it produced a stratigraphic age reversal relative to adjacent, otherwise internally consistent dates; analytical uncertainty alone was not used as a criterion, and the misleading phrase “large analytical uncertainties” will be removed from the Methods. Where the dated material was a factor (bulk sediment, very small sample), this is stated in the individual justification. LC8 was retained despite its comparatively large nominal uncertainty because it was stratigraphically consistent with neighbouring dates and did not produce an age reversal, unlike the samples that were excluded. We will present these case-by-case justifications in a supplementary table, and, as suggested by both reviewers, move Table 1 together with the individual plotted age–depth models to the Supplement.

Line 271: rephrase: “Linear regression analysis of TOC vs clay content showed a positive correlation (R^2 ...”.

Thank you. We agree and will rephrase the sentence as follows: “Linear regression analysis of TOC vs. clay content showed a positive correlation ($R^2 = 0.46$; Fig. 4A).”

Line 275: I am wondering how the scatter plot of TOC vs depth looks, if you use TOC normalized to clay content instead of overall TOC? I assume that this might change the story. I agree that the pattern is controlled by floodplain dynamics, but clay-normalized TOC might show some subtle differences between deep and shallow TOC.

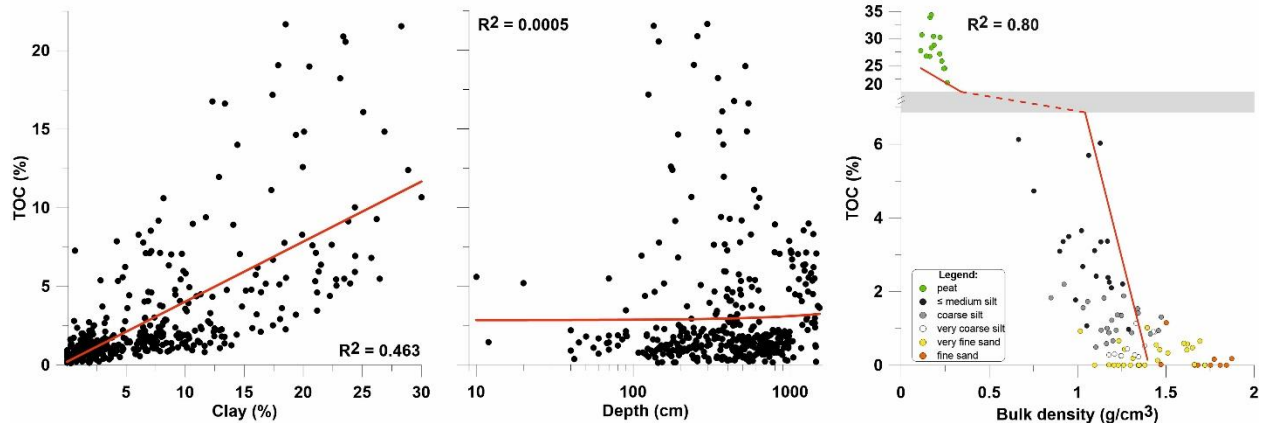
We thank the reviewer for this suggestion and have carried out the requested analysis. We calculated a clay-normalized TOC (TOC/clay ratio) for all samples with paired grain-size and TOC data. Twelve samples (~3% of the dataset) showed anomalously high ratios (>100); on inspection these all correspond to low-TOC (<1%) paleochannel and overbank (sand-dominated) samples in which clay content was below 0.01%, so the ratio becomes mathematically unstable due to a near-zero denominator rather than reflecting a genuine sedimentological signal. These twelve samples were therefore excluded as artefacts of the normalization itself. For the remaining samples (ratios ranging from 0.07 up to 10.83), regressing the clay-normalized TOC index against depth yielded an R^2 of 0.030, a value close to the $R^2 = 0.0005$ obtained for raw TOC vs. depth (Fig. 4B), indicating no meaningful depth-related trend even once grain-size variability is accounted for. Overall, this additional test does not reveal the subtle deep-vs-shallow difference the reviewer anticipated, but rather reinforces our original interpretation: organic carbon preservation in the LDF is governed primarily by depositional facies/setting at the time of accumulation, not by progressive diagenetic loss with burial depth. We will report this clay-normalized analysis (scatter plot and regression statistics, with the exclusion criterion for the twelve near-zero-clay samples stated explicitly) as a



supplementary figure, and add a sentence in Section 5.1 noting that this control test corroborates rather than modifies the original conclusion.

Fig. 4: I suggest setting TOC on the y-axis, as this is more likely the dependent variable, while clay content and depth are seen as the independent controls.

Thank you for the suggestion. Agreed. The axes will be rearranged accordingly in the revised figure, with TOC presented on the y-axis as the dependent variable.



Line 284 + Fig. 4c: Not sure how the red line was achieved in Fig. 4c. I have the feeling that all data points are located along one line and do not follow the broken lines.

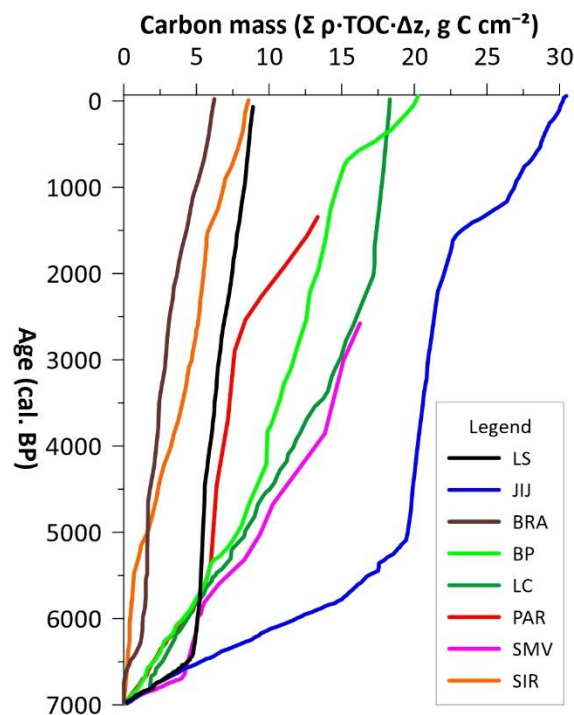
Thank you for pointing this out. The dotted red line in Fig. 4C does not represent an additional regression line or a separate trend. It indicates the break in the x-axis between 7 and 20% TOC, where no measurements are available. The x-axis was broken to avoid displaying this empty interval and to allow the measurements at higher TOC values to be presented more clearly. The red line represents the regression relationship across the full range of TOC values, with the dotted segment corresponding to the x-axis break. To avoid this potential confusion, we will explicitly indicate in the figure legend that the dotted red line represents the x-axis break. The figure caption will be revised to read: “...(c) TOC versus dry bulk density, showing a strong inverse relationship ($R^2 = 0.800$). The red line represents the linear regression, while the dotted

red segment indicates the break in the x-axis between 7 and 20% TOC, where no measurements are available. Point colours represent the mean grain-size class of each sample.”

Line 295ff: I suggest to plot cumulative carbon accumulation from bottom to top for all profiles. This could be done either using TOC_cum ~ depth or, better, TOC_cum ~ age. This should be done in a way that cores can be compared between upstream and downstream.

Thank you for this suggestion. We will add a supplementary figure presenting cumulative organic-carbon storage (g C cm^{-2} , the running sum of dry bulk density $\times$ TOC $\times$ layer thickness from the base of the record upwards) against calibrated age, with all cores on common axes and the cores of the embanked upstream sector and of the natural downstream sector in contrasting colours, so that they can be compared directly, consistent with the age-referenced CAR panel introduced in the revised Fig. 2 (see our response to the Fig. 2 comment above).

Line 348 & 360: I wonder if “decoupling” is the correct wording here. Depth plays a crucial role in preserving the TOC (it’s not decoupled from depth), as the pattern of TOC-deposition with depth is preserved. The causes for the differences to other studies should be discussed here in detail. As groundwater plays a crucial role, this should be discussed. Are groundwater



conditions in the upper and lower parts of the study site different?

Thank you. We agree with the reviewer that the term “decoupling” was not used correctly in this context. When drafting this passage, we had in mind an implicit comparison with upland soil profiles, where TOC systematically declines with depth due to progressive decomposition – hence the “decoupled from depth” phrasing. In floodplain settings, however, organic carbon storage is not, in fact, decoupled from depth or from the geomorphic evolution of the floodplain through the Holocene; rather, it reflects that evolution directly. We will remove the closing sentence of this passage (“These transitions between fluvial and wetland settings produce complex vertical carbon distributions that are decoupled from depth alone.”) and revise the wording accordingly.

We would also like to highlight an additional consideration that strengthens this point: because sedimentation rate varies substantially between cores, the uppermost 1 m of sediment does not represent a comparable timeframe across the dataset – it corresponds to only the last few centuries in the Jijila (JIJ) and Popina Swamp (BP) cores, but to roughly the last one and a half millennia in the Brateş (BRA) and Crapina (LC) cores. Restricting any depth-based comparison to this uppermost interval would therefore be temporally inconsistent across cores and not representative of the full sediment package. We will add a sentence making this explicit in the revised paragraph.

Regarding groundwater conditions, we unfortunately do not have monitoring data or a comparative dataset on water-table depth/evolution between the upper and lower parts of the study area, and we will state this explicitly as a limitation rather than speculate.

We will remove this sentence from lines 347-349: “This finding is consistent with growing evidence that, in alluvial and wetland environments, carbon storage is decoupled from burial depth.” And we will revise the next paragraph (lines 354-359) as follows: “... Cosumnes River floodplain (California) when sampling depth is extended from 1 to 3 m. These findings are particularly relevant to the interpretation of our dataset, where restricting comparisons to the uppermost 1 m would not only exclude a substantial part of the sedimentary carbon pool but would also introduce a temporal inconsistency among cores. Because sedimentation rate varies substantially between cores, the uppermost 1 m of sediment does not represent a comparable timeframe across the dataset – it corresponds to only the last few centuries in the Jijila (JIJ) and Popina Swamp (BP) cores, but to roughly the last one and a half millennia in the Brateş (BRA) and Crapina (LC) cores. Restricting any depth-based comparison to this uppermost interval would therefore be temporally inconsistent across cores and not representative of the full sediment package.

Line 364ff: Please link this to your findings. How is this shown in your results, i.e. what are the spatial differences expressed by the differences in the cores?

Thank you for this comment – we agree the statement needs to be explicitly grounded in our own results, and will revise this paragraph on two levels.

First, this observation connects directly to the sedimentation-rate argument developed in the paragraph before line 336 on clay/TOC/CAR: in better-connected settings, higher sedimentation rates promote faster burial of freshly deposited organic matter, more rapidly isolating it from atmospheric oxygen and establishing anoxic/sub-oxic conditions within the sediment column – limiting microbial oxidation and enhancing TOC preservation, the opposite of what occurs under prolonged surface exposure in poorly connected settings.

Second, we can support the statement with our core-based dataset: using historical topographic maps from the first half of the XX century to independently characterize the hydrological connectivity of each site, compared against measured TOC in the uppermost facies. Among lacustrine-facies cores, Brateş (BRA) and Sireasa (SIR) – both well connected to the Danube (Brateş flooded during high-water stages; Sireasa linked via a network of small distributary channels) – show higher TOC than Şerban (LS) and Jijila (JIJ), both comparatively isolated (LS on the interior of Insula Mare a Brăilei; JIJ in the distal floodplain, >5 km from the main Danube channel). A parallel contrast holds among organic-rich facies: Popina Swamp (BP), connected via several small channels and being closer to the main Danube channel, shows higher TOC than the more distal Crapina Lake (LC). This comparison is deliberately restricted to the uppermost facies of each core, i.e. the deposits formed under the connectivity conditions shown on the early-20th-century maps, and not to the whole stratigraphy: the connectivity of

each site has changed through the Holocene, so whole-core means would mix deposits formed under different conditions.

We will revise the paragraph (Lines 361–368) to include this core-based dataset comparison:

“Another critical control is local hydrological connectivity, which strongly shapes spatial TOC distribution across the LDF by modulating oxygen exposure and redox conditions, sedimentation rates, and organic matter inputs. Well-connected settings typically sustain higher sedimentation rates, promoting more rapid burial of organic matter and earlier onset of anoxic/sub-oxic conditions within the sediment column, thereby limiting microbial oxidation and enhancing long-term carbon preservation. This pattern is supported by our core-based dataset: using historical topographic maps from the first half of the XX century to characterize the connectivity of each site, and comparing the mean TOC of the uppermost facies of each core (the deposits formed under these connectivity conditions), cores from well-connected lacustrine settings – Brateş (BRA, mean TOC 2.5%), periodically inundated during high-water stages, and Sireasa (SIR, 4.1%), linked to the Danube via a network of small distributary channels – show consistently higher TOC than cores from more isolated lacustrine settings – Şerban (LS, 1.3%), situated within the interior of Insula Mare a Brăilei, and Jijila (JIJ, 1.3%), located in the distal floodplain more than 5 km from the main Danube channel. A comparable contrast is observed among organic-rich facies: Popina Swamp (BP, 7.7%), well connected via several small channels to the main Danube channel, exhibits higher TOC than the more distal Crapina Lake (LC, 2.9%). These site-specific differences are consistent with observations from the Elbe floodplain (Heger et al., 2021) and the Austrian Danube floodplain (Tockner et al., 1999), where connectivity gradients exert a primary control on SOC stocks and the efficiency of organic matter retention.”

Line 439: Please describe what you mean by hydrological connectivity.

Thank you for this comment. By “hydrological connectivity” we refer to the degree of hydrological exchange between the Danube channel and adjacent floodplain environments. This exchange occurs through surface-water connections, particularly during flood pulses, and it controls the lateral transfer of water, suspended sediment, nutrients, and organic matter between the river and the floodplain.

In the Lower Danube Floodplain, hydrological connectivity varies with river stage. At low water levels, floodplain water bodies may remain largely disconnected from the main channel. At higher stages, surface-water connections become established, allowing direct exchange of water and sediment between the Danube and floodplain channels, wetlands and lakes. Thus, in our study, hydrological connectivity describes the extent to which floodplain environments are hydraulically linked to the Danube and able to exchange water and associated materials with the river.

We will add the following paragraph in the text: “Following Tockner et al. (1999), we define hydrological connectivity as the degree of hydrological exchange between the Danube channel and adjacent floodplain environments, occurring primarily through surface-water connections during flood pulses and controlling the lateral transfer of water, suspended sediment, nutrients and organic matter between the river and the floodplain.” – inside lines 361–368, where the term appears for the first time.

Line 468: Limit to base-level impacted floodplains. This is not the case for upstream floodplains.

Thank you for pointing this out. We agree that the effect of rising base levels on carbon sequestration is not applicable to all floodplain environments, particularly those located upstream and not directly influenced by base-level changes. We have therefore revised the sentence to explicitly limit this interpretation to lower floodplain environments under direct base-level control: “...rising base levels can enhance carbon sequestration in lower floodplain environments that are directly influenced by base-level changes.”

Line 498–506: How is this decline expressed in your data?

Thank you for this question. We will rewrite this passage to make the causal chain between dam construction, embankment, agricultural conversion and the observed TOC decline more explicit, and to directly answer how this decline is expressed in our own data.

This process is driven by reduced sediment supply from the Iron Gates I/II and tributary dams, which lowers the floodplain sedimentation rate; with less sediment delivered, previously deposited organic matter is covered more slowly by a thinner sediment layer, delaying the onset of the sub-oxic/anoxic conditions required to protect it from oxidation. Embankment then interrupts floodplain connectivity almost entirely, and the resulting conversion to agricultural land exposes organic matter – deposited, according to our age–depth models, over roughly the past 800 to 100 years – to oxidation through drainage and mechanical disturbance.

Empirically, this endpoint of the chain is what our own dataset directly documents: the up-to-2.5-fold TOC reduction visible in the top-1-m inset (now presented as a supplementary figure; see our response to the Fig. 2 comment), where TOC in the upper 50 cm (most depleted at 20–30 cm) is markedly lower than in the immediately underlying, undisturbed 50–100 cm interval. We do not, however, have the temporal resolution to independently isolate the relative contribution of each step in this chain (dam-driven sediment reduction vs. embankment vs. tillage) within our age model, and we will state this explicitly.

Revised passage (merging and replacing Lines 498–511):

“The second, more recent, stage of anthropogenic transformation has had the opposite effect. Since the mid-20th century, large-scale hydraulic engineering – particularly the construction of dams such as Iron Gates I and II, together with dams on major tributaries – has dramatically reduced the sediment load delivered to the lower Danube. With less sediment reaching the floodplain, sedimentation rates declined accordingly, so that organic matter deposited at any given time was covered more slowly by an increasingly thin sediment layer, delaying the onset of the sub-oxic/anoxic conditions required to protect it from oxidation. At the same time, in the study area, extensive embankments and flood-control infrastructure have disconnected more than 85 % of the historical floodplain from overbank flow, almost entirely interrupting this hydrological exchange and further curtailing renewed sediment cover (Constantinescu et al., 2015). This ‘great disconnection’ allowed large parts of the former floodplain to be converted into agricultural land, where drainage and mechanical tillage subsequently exposed organic-rich horizons – deposited, according to our age–depth models, over roughly the past 800 to 100 years – to oxygen, accelerating microbial decomposition and transforming formerly stable carbon sinks into carbon sources. This shift is directly reflected in our dataset by TOC reductions of up to 2.5-fold within the upper 50 cm of agriculturally used floodplain sediments (see Supplementary Figure), where previously waterlogged layers are now regularly aerated and mechanically disturbed.”

