

Dear Dr Hemingway,

We thank you very much for your comments on our manuscript. Based on your very constructive comments, we have thoroughly revised the manuscript. Additional discussion and justifications have been added into the manuscript or into the Supplement. Please see below the detailed responses. Major changes have also been highlighted in the revised manuscript.

With best regards

Lishan Ran, on behalf of the coauthors

Synopsis

The central focus of this manuscript is to investigate carbon cycling in the arid-semiarid Wuding River catchment using both campaign-style and time-series sampling approaches. The authors quantify dissolved carbon concentrations, both organic (DOC) and inorganic (DIC), as well as particulate organic carbon (POC) concentrations and CO₂ outgassing fluxes throughout the catchment over multiple seasons. In particular, the authors compare and contrast signals across a range of Strahler stream orders (1 to 6) from subcatchments underlain by sand and by loess and quantify differences in their respective carbon budgets.

As the authors point out, arid-semiarid river catchments are severely underrepresented in global riverine carbon-cycle budgets. By presenting a large dataset for the Wuding River catchment, this study begins to ameliorate this issue. I therefore find the goals and targets of the present study to be impactful, as they attempt to advance our collective understanding riverine carbon cycling. However, I do have some issues with the interpretation of these data, particularly related to a number of claims that seem unsubstantiated or somewhat contradictory. Additionally, I feel that there are some areas that warrant further clarification and detail. Overall, I feel that the authors should remove some of the weaker and highly speculative text that attempts to prescribe carbon sources and should instead focus on the strengths of this dataset – namely, carbon fluxes and budgets. If the authors can address these issues, which I think they can, then I believe that this manuscript could provide a valuable contribution to Biogeosciences.

I outline my larger concerns in detail below, followed by a list of smaller concerns and questions. Please do not hesitate to contact me for further discussion regarding this review.

Sincerely,

Jordon Hemingway

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Reply: Many thanks for your very constructive comments. Please find below our responses to each of your comments.

Larger Comments

Methods details and measurement uncertainty

In general, I feel that more detail is required in describing the methodology and presenting data uncertainty. In particular, the paragraph beginning on L135 should be expanded considerably. For example, I would like to see more details related to:

Reply: Many thanks for your very constructive comments.

i) Field titration methods. How was this done? Were any standards measured? Field titrations generally have quite high uncertainty associated with them (~5 – 10%), yet there is no uncertainty assessment presented here. What is the resulting propagated uncertainty for calculated DIC concentration values?

Reply: Total alkalinity was determined by triplicate titrations in the field with 0.1 M HCl, and methyl orange was used as the indicator, following the standards as suggested by APHA (1999, *Standard Methods for the Examination of Water and Wastewater*). For the Wuding River with widespread presence of carbonates, its river water alkalinity is quite high (62.1–67.7 mg L⁻¹). Our field triplicate titration results are highly consistent with the difference between the three results generally less than 3%. Thus, we expected the obtained alkalinity results are reliable with high confidence. Finally, DIC was calculated from total alkalinity, pH, and temperature by using the program CO2calc. Because the measured pH varied from 7.68 to 9.29, the calculated DIC was approximately equal to alkalinity, with >96% of the alkalinity composed of HCO₃⁻, consistent with the relative speciation (%) of CO₂, HCO₃⁻, and CO₃⁼ in water as a function of pH (please refer to the figure below). The revised descriptions have been added into the manuscript. (lines 142-145)

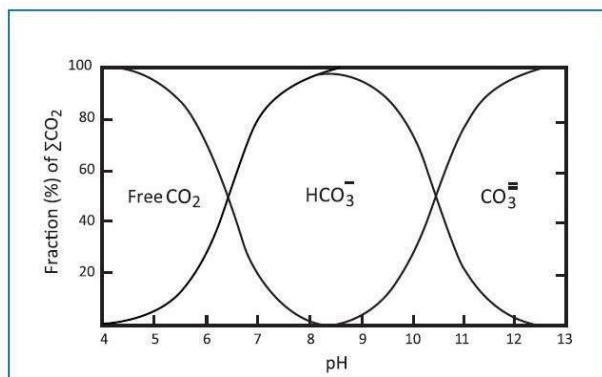


Figure: Relative concentrations of the different inorganic carbon compounds against pH.

ii) DOC uncertainty. How was DOC uncertainty estimated? Was a standard calibration curve used? If so, how often was the calibration curve analyzed? Was each sample injected in triplicate? Duplicate? Single injection?

Reply: DOC was determined by the high-temperature combustion method (850 °C) by using an Elementar Vario TOC Select Analyzer. A standard calibration curve was used for every round of field samples. Generally, the standard calibration curve was analyzed and re-determined for each 60-80 samples, depending on the variability of the DOC concentration. Triple injections indicated an analytical precision of <3%, and the average of the three injection results was calculated to represent the sample's DOC concentration. These descriptions have been added into the revised manuscript. (lines 140-142)

iii) Were solid samples fumigated with HCl at room temperature or at ≥60°C? I ask because dolomite will not be removed at temperatures below 60°C. If these samples are expected to contain dolomite, and if they were fumigated at room temperature, then I would expect resulting POC estimates to be biased upward.

Reply: To measure the POC concentration, the solid soil and sediment samples were fumigated at 65 °C for 24 h. For the Chinese Loess Plateau, carbonates in its loess-paleosols consist mostly

of calcite and dolomite, and the latter is the primary detrital material (please see Yang et al., 2000. *Palaeogeography, Palaeoclimatology, Palaeoecology*, 157, 151-159). Therefore, we carefully removed the dolomite with concentrated HCl at a higher temperature than the room temperature. We have added the description into the revised manuscript. (lines 149-151)

iv) CO₂ δ¹³C values. Were these analyzed by Beta Analytic using an IRMS on a separate gas split, or are these values generated by the AMS? I would expect these to be IRMS values, but this should be stated clearly.

Reply: The CO₂ δ¹³C results are generated by the AMS at the Beta Analytic Radiocarbon Dating Laboratory (Miami, USA). This has been clearly stated in the revised manuscript. (lines 156-158)

v) Radiocarbon notation. Throughout the manuscript, the authors conflate ¹⁴C age, Δ¹⁴C (which is always reported in units of per mille!) and percent modern, or pMC. I would strongly suggest that the authors choose one notation and stick with it (my personal choice would be to use pMC). Still, if the authors choose to use ¹⁴C age, this be reported in units of “¹⁴C yr BP” rather than simply “years”, as the latter is ambiguous and could refer to a calibrated age, which would not be appropriate here.

Reply: Thanks a lot for your suggestion on how to describe the ¹⁴C analysis results. We have chosen to use the percent modern (pMC) to describe the results throughout the manuscript, mainly in Sections 3.2 and 4.3. But, to compare our results with Wang et al (2012. *Global Biogeochemical Cycles*, 10, 26, GB2025, doi:10.1029/2011GB004130) that investigated the ¹⁴C age of DOC and POC in the Yellow River, we have kept ‘¹⁴C age’ results in Table 2 and Figure 5, and the simple notations ‘years’ have been replaced by ‘¹⁴C yr BP’. (lines 153-156; 533-540)

vi) Sediment accumulation rates. In Figure 10, a burial flux is presented in units of g C yr⁻¹, yet I find no reference to calculations for sediment accumulation rates (SAR). How was SAR calculated for each of these cores? This information is necessary in order to convert the measured %OC numbers into burial fluxes...

Reply: The sediment accumulation rate behind check dams in the Wuding River catchment was based on our earlier estimate (i.e., Ran et al., 2013. *Global and Planetary Change*, 10, 308-319; please also refer to Section 3.3 of the manuscript). Our earlier work shows that the annual sediment accumulation rate in this study catchment is 3720×10¹⁰ g year⁻¹. In addition, based on the POC concentration (POC%) of the four sediment cores distributed in both the sandy and loess subcatchments (Figure 1), we calculated the arithmetic mean of the POC% (0.21±0.11%). With the sediment accumulation rate and the POC% in deposited sediment we estimated the total OC burial rate ((7.8±4.1)×10¹⁰ g C year⁻¹). Because the POC content in the top 0-60 cm soils is considerably higher than that in the deeper soil layers, our simple estimate is associated with great uncertainty. Future efforts are therefore needed for a more accurate assessment. In addition to Section 3.3, we have also added these justifications into the manuscript. (lines 469-477)

Additionally, all of the numbers reported in the “Results” section should include corresponding uncertainty, either analytical uncertainty (when reporting single values) or sample population uncertainty (when presenting averages). For averages, please be clear if reporting standard errors or standard deviations. Similarly, significant figures should be consistent throughout the manuscript!

Reply: Many thanks for your suggestion. We have provided the uncertainty, mainly standard deviation, for all of the numbers in the ‘Results’ section. In addition, we have also double checked the consistency of significant figures throughout the manuscript. Please refer to highlighted changes in the revised manuscript. (lines 230-245; 261-265; 289-298; 316-326)

Net Ecosystem Production

I am left somewhat confused by the assumptions and uncertainties related to NEP calculations. To convert SR to Rh, the authors apply a “forested” and “non-forested” fraction heterotrophic derived from Hanson et al. (2000). However, the “non-forested” estimates from this reference are for pasture and grassland, not barren landscapes such as those presented in the current study. Presumably nearly 100% of soil respiration on barren landscapes is heterotrophic, no? Additionally, while the “non-forested” fraction heterotrophic in Hanson et al. averages 40%, they observe values ranging from 10% to 90% -- nearly the entire possible range!

I wonder if the authors have any way to estimate the uncertainty on NEP estimates presented here – if so, these should be discussed in detail. I would expect these uncertainties to be quite large, yet this is not mentioned or discussed in the manuscript. For example, how do the values here compare to those calculated by subtracting SR from MODIS-derived GPP values? To me, this seems like a more straightforward method to estimate NEP that isn’t subject to the uncertainties associated with converting SR to Rh.

Reply: We divided the study catchment into two subcatchments, including the sandy subcatchment and the loess subcatchment. While forest cover in the Wuding River catchment is quite low (less than 5%) as a result of low precipitation, grassland is the major land cover in the sandy subcatchment and agriculture and grassland predominate the loess subcatchment (Wang et al., 2014. Spatial-temporal changes of land use in Wuding River Basin under ecological restoration, *Bulletin of Soil and Water Conservation*, 34, 237-243 (in Chinese with English abstract). This is largely the result of the implementation of the Grain-for-Green Project which was initiated by the Chinese government in 1999. After more than 10 years of implementation of this vegetation restoration program, the vegetation cover (forest and grassland) has greatly increased. Please also refer to two photos below showing the landscape of the sandy subcatchment (left) and of the loess subcatchment (right). Both photos were taken by me in 2015 when doing the fieldwork. To better describe the landscape of the catchment, we have revised the description in Section 2.1 ‘Study area’ (lines 88-90). Therefore, the landscape and land cover of the Wuding River catchment are generally consistent with the distinction of “forested” and “non-forested” by Hanson et al. (2000). With respect to the huge range of the “non-forested” fraction heterotrophic (i.e.,10-90% as you have noticed), we have discussed the potential uncertainty in the revised manuscript. (lines 560-563).



Figure: Landscape characteristics of the sandy (left) and loess (right) subcatchments.

Our rate is consistent with recent measurements under different vegetation types in this arid-semiarid region (e.g., Fu et al., 2013). Fu et al. (2013. Soil respiration as affected by vegetation types in a semiarid region of China. *Soil Science and Plant Nutrition*, 59, 715-726) measured total soil respiration in this arid-semiarid region. Their mean soil respiration rates under 4 different vegetation types are in the range of 1-1.4 $\mu\text{mol m}^{-2} \text{s}^{-1}$, which are equivalent to 380-530 $\text{g C m}^{-2} \text{year}^{-1}$. Thus, our estimate is reliable. We have carefully revised the manuscript with new references to justify our arguments (lines 316-326). Using the ratios derived from Hanson et al. (2000) has been widely used in the world to assess heterotrophic soil respiration in river catchments under different land cover types (e.g., Brunet et al., 2009. Terrestrial and fluvial carbon fluxes in a tropical watershed: Nyong basin, Cameroon. *Chemical Geology*, 3, 563-572; Lee et al., 2017. A high-resolution carbon balance in a small temperate catchment: Insights from the Schwabach River, Germany. *Applied Geochemistry*, 85, 86-96). Just as you have commented, this portioning is associated with potential uncertainty. Our ongoing research assessing NEP storage dynamics on the entire Loess Plateau is using the MODIS-derived GPP products. A preliminary estimate for the Wuding River catchment suggests that the results of the two methods are generally equal with a difference of $\sim 11\%$. We greatly appreciate your suggestion and we will adopt the more straightforward method. Many thanks.

Interpretation of DIC, CO₂ d¹³C, and $\Delta^{14}\text{C}$

In general, I am confused by the discussion on DIC sources, especially as they relate to measured CO₂ d¹³C and $\Delta^{14}\text{C}$ values – there seem to be a number claims that are either contradictory or are not explained in significant detail. Beginning in the abstract (L21) and repeated throughout the manuscript, the authors state that DIC is largely sourced from carbonate dissolution, especially in the loess subcatchment. Intuitively, this makes sense to me since loess contains a significant amount of carbonate, as the authors rightly state. However, this is incompatible with the d¹³C and $\Delta^{14}\text{C}$ values presented in this study, which suggest that remineralization of terrestrially derived OC is the main source of outgassed CO₂ behind check dams. What mechanisms could explain this discrepancy? I feel that there needs to be significantly more discussion and clarification here.

Furthermore, I find some of the claims related to CO₂ outgassing to be overstated. For example, the statement: “The evasion of old carbon [derived from pre-aged OC respiration as is seen here] is likely to be widespread in arid-semiarid catchments worldwide with similar hydrological regime and terrestrial ecosystems” (L477). This seems to be quite a stretch, especially given my confusion related to the lack of carbonate dissolution signature as stated above.

Reply: We collected CO₂ emission samples in the Wuding River catchment for carbon isotope analysis by using the SrCl₂ solution. Thus, the measured $\delta^{13}\text{C}$ and $\Delta^{14}\text{C}$ results are for the emitted CO₂ from river water. Unfortunately, we did not collect water samples for $\delta^{13}\text{C}$ and $\Delta^{14}\text{C}$ analysis of the DIC. But prior studies indicate that the $\delta^{13}\text{C}$ of DIC generally ranges from -6.7‰ to -12.9‰ in Loess Plateau rivers (Liu and Xing, 2012. *Chemical Geology*, 296, 66-72). For the arid-semiarid Wuding River catchment and the whole Yellow River basin in which the Wuding River is located, carbonate dissolution has been found to be the primary source of DIC (mainly HCO₃⁻) due to its high carbonate content in loess soils (up to 20%; please see Chen et al., 1995. Major element chemistry of the Huanghe (Yellow River), China: Weathering processes and

chemical fluxes. *Journal of Hydrology*, 168, 173-203; Chen et al., 2005. Spatial and temporal analysis of water chemistry records (1958–2000) in the Huanghe (Yellow River) basin. *Global Biogeochemical Cycles*, GB3016, doi: 10.1029/2004gb002325). Therefore, we conclude that DIC is largely sourced from carbonate dissolution, especially in the loess subcatchment. With respect to CO₂ emissions, however, the emitted CO₂ is characterized by much depleted $\delta^{13}\text{C}$ values (-19.3‰ – -33.9‰), which is significantly different from the $\delta^{13}\text{C}$ signature of DIC carbonates (i.e., 0‰ for DIC derived from carbonates by proton attack and -8.5‰ for DIC derived from carbonate dissolution; Barth et al., 2003. *Chemical Geology*, 200, 203-216; Brunet et al., 2009. *Chemical Geology*, 265, 563-572). In comparison, the $\delta^{13}\text{C}$ values of the emitted CO₂ largely reflect the contribution of C3 and C4 plants which have a $\delta^{13}\text{C}$ values signature of -27‰ and -15‰, respectively. Mineralization of terrestrially derived OC has been widely found to be the primary source of river water CO₂ emissions. For example, Mayoga et al. (2005) found that respiration of contemporary organic matter (less than 5 years old) originating on land and near rivers is the dominant source of excess CO₂ that drives outgassing in the Amazon rivers (Mayorga et al., 2005. *Nature*, 436, 538-541). Similarly, Borges et al. (2015) discovered that lateral transport of soil or wetland DOC and POC that is mineralized to CO₂ within the rivers maintains CO₂ outgassing in African rivers (Borges et al., 2015. *Nature Geoscience*, 8, 637-642). Therefore, in combination with the measured $\delta^{13}\text{C}$ and $\Delta^{14}\text{C}$ results of the emitted CO₂, we conclude that decomposition of the terrestrially derived OC drives CO₂ outgassing in the Wuding River catchment although DIC is largely originated from carbonate dissolution. We have added more justifications with relevant references in the revised version of the manuscript to support our arguments. Based on your comments, we have removed the overstated comments, including the one you mentioned here, from the revised manuscript to make sure all the arguments are supported by our results and figures. Major changes have been highlighted in the manuscript. (lines 495-504; 525-528; 533-540)

DOC sources and trends

Beginning on L204 and continuing throughout the manuscript, the authors refer to a “significant downward trend along the river course from headwater downstream... in both subcatchments.” However, when I look at Figure 2, I am left puzzled and wondering if these trends are, in fact, significant. Given the large error bars for each stream order, my guess is that they are not. In my opinion, any subsequent discussion related to DOC sources and trends (e.g. L300-313; L306-309; L318-324) is highly speculative at best.

Additionally, I find some of these claims to be contradictory. For example, on L314, the authors state that “...there was no significant correlation between DOC and flow based on the spatial sampling results”. However, for the high-frequency sampling the authors observe a “positive correlation between DOC export and hydrography [that] demonstrates the enhanced leaching of organic matter from surface vegetation and organic-rich top soil layers”. Why would a positive correlation be expected during storm events yet not on a seasonal basis? What mechanism could explain this? This discrepancy is not addressed.

Reply: Many thanks for your comment on DOC sources and trends. To detect the DOC concentration changes along the river course from headwater downstream, we plotted the average DOC concentration with standard deviation (error bars) based on stream order (Figure 2). The ‘DOC first exhibited a downward trend along the river course from headwater downstream and then increased in the 6th order mainstem river in both the sandy and loess subcatchments (Figure 2)’. Because the downward trend does not pass the significance test at the

significance level of 0.05, we did not use the word ‘*significant*’ in the description. If we categorize the first 5 stream orders (1–5) into 2 groups (1st–2nd and 3rd–5th), we can easily detect that DOC in the headwater 1st–2nd order streams (4.7–5.4 mg L⁻¹) was on average 9–21% higher than in the 3th–5th order streams (4.2–4.9 mg L⁻¹), it increased to 5.2–6.1 mg L⁻¹ in the 6th order mainstem, representing an increase of 18–36% relative to the 3th–5th order streams. This is particularly true for the loess subcatchment (Figure 2a). When combining the two subcatchments together, the DOC in the 6th mainstem was 5.7 mg L⁻¹, which was 27% higher than the average of the 3rd–5th order streams (4.5 mg L⁻¹). To more accurately describe the DOC trend, we have revised the statement: ‘Although statistically insignificant, DOC first exhibited a downward trend along the river course...’, and also the description of the results (lines 230–237).

When plotting the DOC measured across the whole catchment over three seasons against the concomitant flow, there was no significant correlation between DOC and flow based on the spatial sampling results ($p > 0.05$; please refer to the graph below). In comparison, our high-frequency sampling at the catchment outlet Baijiachuan gauge indicates that DOC concentrations were 26% higher in the flooding periods than that in normal flow conditions. The positive correlation between DOC export and hydrography demonstrates the enhanced leaching of organic matter from surface vegetation and organic-rich top soil layers (Hernes et al., 2008. *Geochimica et Cosmochimica Acta*, 72, 5266–5277). Clearly, this positive response contradicts the indiscernible relationship between DOC and flow discharge within the catchment. This is probably because the three intensive seasonal samplings did not capture the carbon export in high-flow conditions. The flow discharge during the three sampling periods varied in the range of 0.002–105 m³ s⁻¹ (please see this range in the figure below), which largely reflects the carbon export processes during low flow to, at most, medium flow conditions. In comparison, the high-frequency sampling at Baijiachuan gauge captured the carbon export during extremely high flows (200–1760 m³ s⁻¹). We have also presented the raw data of monthly flow discharge and DOC concentrations in the Supplement (Table S2), so they are now available for free use. Please refer to the explanation of this discrepancy in the revised manuscript (lines 417–429).

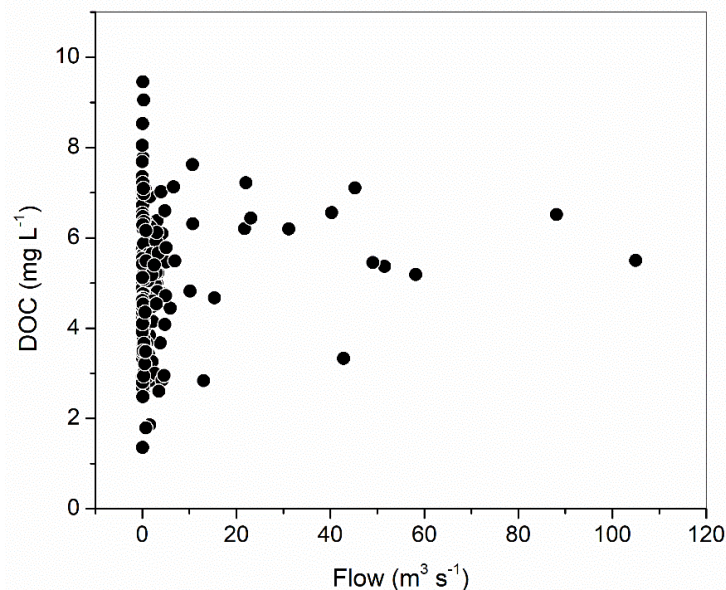


Figure: Relationship between flow discharge and DOC based on the three sampling results.

POC sources and trends

I find that a significant amount of discussion related to POC sources and sinks needs to be substantiated with more evidence or, at a minimum, alternate explanations need to be addressed. First, beginning on L326, the authors claim that low POC content (by which they mean % of suspended solids, a point that I address below) “reflects the ancient sedimentary OC origin of about 0.5% for fluvial sediments worldwide... [and is also] seen from the isotopic signature of the Yellow River sediment...” The authors go on to state that low %OC reflects “mobilization of subsurface soils that have a substantially lower OC content than surface soils” (L334). However, “ancient sedimentary OC” presumably refers to sedimentary rock derived material, which is certainly not the same as “subsurface soils”. I’m left confused as to what the authors expect to be the major source of POC – sedimentary rocks or subsurface soils? I think that, with concentration measurements alone, one cannot make strong claims either way.

The well-known relationship between grain size and %OC is also not addressed. The observed POC concentration trends could easily be explained by variable hydrologic sorting – i.e. coarser, OC-poor sediments that are transported during high discharge periods – which would mask any POC source signal. In the absence of isotopic ($\delta^{13}\text{C}$, $\Delta^{14}\text{C}$) or grain-size-dependent measurements (e.g. %OC as a function of Al/Si ratios), I find it hard to believe that POC sources can be prescribed as is done here (also repeated beginning on L385).

Reply: Thanks a lot for your comment. Just as you mentioned, in this manuscript we expressed the POC content in the total suspended solids (TSS). Therefore, it is a percentage of the TSS (dry weight). To make it more clear and consistent throughout the manuscript, we have replaced this term with ‘POC%’ throughout the manuscript, including all figures and tables. This has been explicitly introduced in manuscript (lines 161-163). As for the sources of POC, it is closely related to the soil erosion and sediment yielding characteristics of the Wuding River catchment, or generally, the whole Chinese Loess Plateau. The Chinese Loess Plateau (area: $\sim 440,000 \text{ km}^2$) is covered with 100-300 m thick highly weathered loess soils (Zhao et al., 2013. *Land Degradation & Development*, 24, 499–510; Nie et al., 2015. *Nature Communications*, 6:8511, doi: 10.1038/ncomms9511). As a result of the very fine soil particles, soils in the Loess Plateau are extremely susceptible to erosion. And gully erosion is the major erosion type and is responsible for >70% of the total erosion rate for most parts of the Loess Plateau (Xu, 1999. *Catena*, 36, 1-19; Li et al., 2015. *Geomorphology*, 248, 264-272). Actually, gully erosion of tens of meters is quite common (please also see the photo below for a visual experience).

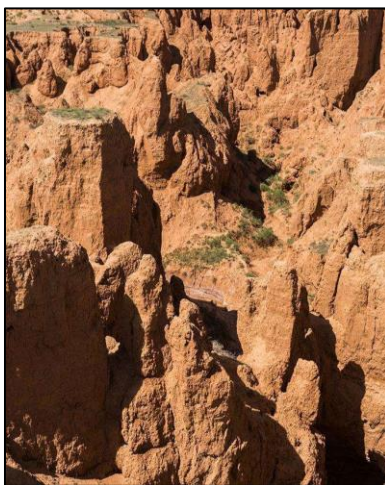


Figure: Gully erosion on the Loess Plateau.

The low POC% in the sampled sediments is quite close to the organic carbon content of sedimentary rocks (i.e., 0.5%; Ludwig et al., 1996. *Global Biogeochemical Cycles*, 10, 23-41). Recent studies investigating POC of the Yellow River sediment by means of $\delta^{13}\text{C}$ and $\Delta^{14}\text{C}$ analysis also suggest that its POC is not from the recently fixed terrestrial plant materials and freshwater plankton, but from the highly decomposed loess soils and weathering of sedimentary rocks and ancient kerogen (Wang et al., 2012. *Global Biogeochemical Cycles*, 10, 26, GB2025, doi:10.1029/2011GB004130). As the primary source of the Yellow River sediment, we can expect that the sediment in the Wuding River catchment carries similar carbon isotopic signatures as that in the Yellow River sediment. Therefore, in association with the high contribution of gully erosion to annual TSS transport and the low organic carbon content of the sampled TSS, we concluded that the lower POC% in suspended solids in summer likely reflects the origin of sedimentary rocks mobilized by gully erosion. Based on your comment, we have carefully revised the manuscript and corrected the misuse of ‘subsurface soils’ and ‘ancient sedimentary rocks’. In addition, necessary references have been added to justify the arguments. Please refer the highlighted changes in the manuscript (lines 369-378).

With respect to the relationship between grain size and POC%, prior studies have investigated the POC% changes in relation to the grain size of sediment in the Loess Plateau and the Yellow River. For example, Zhang et al. (2013. *Biogeosciences*, 10, 2513–2524) divided TSS into five categories (i.e., $<8\ \mu\text{m}$, $8\text{--}16\ \mu\text{m}$, $16\text{--}32\ \mu\text{m}$, $32\text{--}63\ \mu\text{m}$, and $>63\ \mu\text{m}$) and determined the POC% of each category. Their results show that more than 75 % of the POC was concentrated in sediment particles with grain size smaller than $16\ \mu\text{m}$, which suggests that the TSS grain size was the dominant factor controlling POC transport in the Loess Plateau and the Yellow River. Same results of a higher POC% in smaller particles are also discovered by Wang et al. (2012. *Global Biogeochemical Cycles*, 10, 26, GB2025, doi:10.1029/2011GB004130). We have added these justifications into the revised manuscript, and references have been used to justify our arguments (lines 434–446). In addition, our ongoing (biweekly) sampling at the catchment outlet Baijiachuan gauge is aimed to explore the relationship between grain size and POC%, and hopefully the sources of POC could be better prescribed. Many thanks for your comments and inspirational suggestions.

Similarly, beginning on L408, %OC content behind check dams is compared to that on hillslopes and is used as evidence for burial efficiency. However, this “negligible OC loss after erosion” (L412) could be explained by alternative hypotheses. For example, deposited material could (likely does?) contain a different grain size distribution than that of hillslope soils, and thus a different %OC content. Also, any remineralization of terrestrially derived POC could be masked due to replacement by aquatic sources (as is discussed). Again, I find it hard to prescribe POC sources and burial efficiencies without additional measurements such as $\delta^{13}\text{C}$ and $\Delta^{14}\text{C}$. I also find the claim that this material “would have otherwise been mineralized to form CO_2 or CH_4 along fluvial delivery” (L418) to be somewhat speculative. Presumably some of this material would have been transported and buried in coastal marine sediments. Heuristically, it makes sense that burial efficiencies behind check dams are higher than for coastal marine sediments, as the authors imply, but I find a general lack of evidence supporting this claim. Reply: The hilly areas of the Loess Plateau is dominated by gully erosion which can mobilize both the surface soils and the deeper soils as shown in the figure above. And numerous studies on soil erosion in the Loess Plateau have also confirmed the dominant role of gully erosion in

annual total soil erosion rate as mentioned above. For example, in the Wuding River catchment, Zheng et al. (2008. *Geomorphology*, 93, 288-301) concluded that ‘when rainstorm intensity is sufficiently strong ($> 0.3 \text{ mm min}^{-1}$), all grain-size fractions of loess on a hillslope are eroded without sorting’. Because most of the check dams are constructed on gully channels and very close to the eroding sites, the eroded soils from hillslopes and gullies can be quickly trapped by check dams after a short delivery distance (usually less than 5 km based on our field surveys). The sediment trapping efficiency is surprisingly high (e.g., $>90\%$) as most dams only have a small intake for irrigation and don’t have spillway gates (please see a typical check dam shown below, taken by Lishan Ran during the fieldwork). Thus, the loss during fluvial transport is likely small as suggested by the comparison between POC% in sediments and soil OC in hillslopes. Moreover, just as you commented, any remineralization of terrestrially derived POC may have been masked due to replacement by aquatic sources. However, based on the combined use of ^{137}Cs and $\delta^{13}\text{C}$ techniques as well as C/N ratios, our earlier work (Wang et al., 2017. *Agriculture, Ecosystems and Environment*, 247, 290-297. Nufang Fang, a co-author of this manuscript, conceived the cited paper) discovered that most of the buried POC is derived from soil erosion from the catchment. In addition, approximately 70% of the eroded soil OC can be buried by check dams in the study catchment. Finally, eroded soil OC is subject to a number of biogeochemical processes, such as burial by impoundments, mineralization in the water column, outgassing, and export to the ocean depending on a suite of physicochemical conditions (e.g., Battin et al., 2009. *Nature Geoscience*, 2, 598-600; Drake et al., 2017. *Limnology and Oceanography Letters*, doi: 10.1002/lol2.10055). For the statement ‘would have otherwise been mineralized to form CO_2 or CH_4 along fluvial delivery’, we have reworded this sentence to make it more clear and accurate and added references to justify the claim. Please refer to the highlighted changes in the revised manuscript (lines 469-482).



Figure: A typical check dam in the Wuding River catchment.

Finally, I find that reporting “OC content” as %OC rather than a concentration (e.g. mg OC L⁻¹) or a flux (e.g. t OC km⁻² d⁻¹) is ineffective and is somewhat misleading. For example, the authors state that “the substantially lower POC content in the wet season largely reflects the impact of gully erosion” (L385). However, one would expect that POC concentration and flux are actually significantly higher during the wet season! As described above, changes in %OC could reflect hydrologic sorting and are not necessarily indicative of source. I would strongly

recommend discussing POC trends in the context of concentration and flux, rather than %OC. This would allow the authors to shift the focus away from attempting to prescribe POC sources (which I find to be a weakness overall) and toward OC flux and budget estimates, which I think is a strength of this manuscript.

Reply: First of all, many thanks for your comment and suggestion. Because the POC sampling is conducted at 74 nested sites across the whole catchment (Figure 1), we did not delineate the boundary of the sub-catchment that each sampling site controls and calculate the POC yield (in units of $t\ OC\ km^{-2}\ d^{-1}$) by normalizing to the size of each sub-catchment. Also, it is not feasible to calculate the annual POC flux at these sampling sites based only on the 3 sampling campaigns in spring, summer, and autumn. Instead, we only calculated the annual flux of C ($g\ C\ yr^{-1}$), including DOC, DIC, and POC, at the catchment outlet Baijiachuan gauge for which we have monthly C results and daily flow and sediment export data. And this gauge-based fluxes were used in the C budget to evaluate riverine carbon export in relation to NEP. To better present the POC results, we have now used POC% (i.e., the percentage of POC in total suspended solids (dry weight)) to express the POC content in suspended solids. The term ‘POC%’ has now been used throughout the manuscript to avoid unnecessary misunderstandings. Just as you have expected, although the POC% in the wet season is lower than that in the dry season, the POC flux in the wet season is considerable on an annual basis because of the high sediment loading, accounting for 65% of the annual total POC flux. Also, with respect to the potential sources of POC, we have thoroughly revised the manuscript based your earlier comments. Please refer to the highlighted changes in the revised manuscript (lines 434-447; 469-477).

Data availability

In my opinion, a major strength of data-rich manuscripts such as this is the ability for readers to incorporate these data into future studies – whether those be review articles or comparisons to other, similar catchments. Along those lines, I am left wondering why the authors do not make all of their raw data available as supplemental tables? I would strongly suggest do so or, at a minimum, including a “Data Availability” statement pointing the reader to a repository that includes these data.

Reply: Many thanks for your comment. We strongly agree with your suggestion. This study is an extension of our earlier work (Ran et al., 2017. *JGR-Biogeosciences*, 122, 1439-1455). In the Supplementary Information of the Ran et al. (2017) paper, we have already made most of our raw data used in this study available. These data include the physiochemical parameters (e.g., location, elevation, channel slope, flow velocity, wind speed, pH, water temperature, dissolved oxygen, Chl *a*, etc.), CO₂ emissions (*p*CO₂ and areal flux), and dissolved carbon concentration (DOC and DIC) in both river and reservoir waters. To facilitate future review studies and/or comparison analyses, we have made the leftover data available by presenting them in the Supplement of this study. Specifically, these data include POC of sediment samples (2015 and 2017) and of drilled sediment from check dams, monthly DOC and DIC concentrations at the catchment outlet (Baijiachuan gauge) as well as the concomitant flow information. Please refer to the Supplement for these data (Tables S1-S3).

Smaller Comments

L14: Remove dash between “terrestrially derived”, change “represent” to “represents”.

Reply: Changed.

L15 (also L68): What is meant by “redistribution”? Do the authors mean “partitioning between DIC, DOC, and POC”? I would change this wording for clarity.

Reply: Here we meant the fate of riverine carbon during its transport from headwater streams to the catchment outlet, including downstream export to catchment outlet, CO₂ evasion from water surface, and organic carbon (OC) burial through sediment storage. We have replaced the word ‘redistribution’ with ‘fate’ for clarity in both sentences. (Lines 15 and 69)

LL17: Change to “While DOC...”

Reply: Changed.

L18: What is meant by “DOC concentration is spatially comparable within the catchment”? I’m confused by this statement. Don’t you argue that DOC concentrations decrease with increasing stream order? (although I question this trend, as stated above).

Reply: Based on your comment, we have removed this ambiguous claim and rephrased the abstract. We have also discussed the spatial variation of DOC concentration from the headwater streams to the mainstem channel. (lines 17-19; 231-236). Many thanks.

L19: “This reflects the enhanced...” seems overly confident. I would say “This likely reflects...”

Reply: Changed.

L21-22: I’m still confused by the DIC sources – carbonate dissolution seems incompatible with the measured CO₂ δ¹³C and Δ¹⁴C values.

Reply: The measured δ¹³C and Δ¹⁴C values of the emitted CO₂ are different from that in the DIC of the Loess Plateau rivers (i.e., -6.7 to -12.9‰; Liu and Xing, 2012. Isotopic indicators of carbon and nitrogen cycles in river catchments during soil erosion in the arid Loess Plateau of China, *Chemical Geology*, 296, 66-72). Also, based on the δ¹³C values of the DIC, Liu and Xing, (2012) discovered that it is largely derived from carbonate dissolution (48.1-94.6%). The observed differences in this study reveal that the emitted CO₂ is not likely from carbonate dissolution-derived DIC. We have also revised the claim in the manuscript. (lines 495-504)

L23: Please be clear that you mean %OC in sediments when stating that “[POC content] shows low values in the wet season.” As stated, this implies that POC concentration or flux are lower in the wet season, which I presume is not true.

Reply: Thanks a lot for your comment. We have clearly stated the ‘POC%’ in the abstract and throughout the manuscript.

L27 (and throughout): Please update the ¹⁴C notation, as described above. “Indicating the release of old carbon previously stored in soil horizons.” Couldn’t this also be described as a mixture of ¹⁴C-free carbonate dissolution and respired young OC? I’m not sure that this claim is supported.

Reply: Based on your major comment above, we have updated the ¹⁴C notation throughout the manuscript. If looking at the ¹⁴C results only, it could also be a mixture of ¹⁴C-free carbonate dissolution and respired young OC as you suggested. But if we take the δ¹³C results into account, it seems the contribution of carbonate dissolution is quite small, because the δ¹³C signature of carbonate-derived DIC is 0‰ for DIC derived from carbonates by proton attack and -8.5‰ for DIC derived from carbonate dissolution (Barth et al., 2003. *Chemical Geology*, 200, 203-216;

Brunet et al., 2009. *Chemical Geology*, 265, 563-572). This $\delta^{13}\text{C}$ signature is significantly different from the observed $\delta^{13}\text{C}$ values of the emitted CO_2 . Please also refer to our detailed responses to your major comment above. (lines 495-504; 525-528; 533-540)

L32: Define “NEP”. I don’t follow the last sentence of the abstract. What is meant by “...has been significantly offset by riverine carbon export”?

Reply: The definition of NEP ‘net ecosystem production’ has been inserted into the abstract. Because the lateral C export into the Wuding river network can be considered a loss of carbon from its terrestrial ecosystems, whether it is related to lateral transport of soil CO_2 (i.e., respiration taking place in soils) or lateral transport of SOC that is processed within the aquatic column. Therefore, the lateral transport of C from the upland terrestrial biosphere to the Wuding river network and its subsequent outgassing to the atmosphere offsets the estimates of terrestrial NEP. Similarly, Borges et al. (2015) discovered that riverine CO_2 evasion in African rivers offsets the terrestrial NEP of the Arica (Borges et al., 2015. *Nature Geoscience*, 8, 637-642). We have rephrased this claim in the revised text. (lines 32-34)

L38: “Rivers play an exceptionally significant role by directly linking...” Role in what? The global carbon cycle?

Reply: ‘The global carbon cycle’ has been added into the text.

L39: Remove dash between “terrestrially” and “derived”.

Reply: Removed

L43: add “the” between “along” and “river”.

Reply: Added.

L44: Remove comma after “processes”.

Reply: Removed.

L45: Change “in-situ” to “in situ” for consistency.

Reply: Changed throughout the manuscript.

L46: How up-to-date is this 1.8 Pg C yr⁻¹ number? See Drake et al. (2017) L&O Letters for an updated number.

Reply: Many thanks for your information. We have checked the updated C outgassing from global rivers and streams in Drake et al. (2017), which is now 3.2 Pg C year⁻¹, excluding the outgassing from non-running inland waters. This has been inserted into the revised manuscript. (lines 45-47)

L51: Has the number of studies on riverine carbon really been increasing exponentially? Change “recent” to “last”.

Reply: To better describe the increasing studies on riverine carbon, we have reworded the statement: ‘Although studies on riverine fluxes of carbon have been considerably increasing over the recent last 20 years...’. (lines 53-54)

L66: this should read “...through the drainage network to the catchment outlet...”

Reply: Revised. Many thanks.

L67: Remove “in” before “northern”.

Reply: Removed.

L83: This should read “...and is located”

Reply: Added.

L85: Consider defining “loess” here.

Reply: Because we divided the river catchment into two subcatchments based on the geomorphological landscape. For the loess subcatchment (Figure 1), it is generally covered with 50–100 m deep loess soils. (lines 88-89)

L92: Citation for hydrologic regime description?

Reply: A reference (Li et al., 2007. Assessing the impact of climate variability and human activities on streamflow from the Wuding River basin in China. *Hydrological Processes*, 21, 3485-3491) has been inserted to support the statement. (lines 94-95)

L94: I’m confused – is this sentence saying that one particular extreme event led to an erosion rate of 7000 t km⁻² yr⁻¹ for a particular year? If so, what is the average erosion rate? I feel like this would be more informative.

Reply: The Wuding River catchment suffered severe soil erosion during the period 1956-1969, prior to the implementation of large-scale soil conservation programmes which were initiated from the early 1970s. The average soil erosion rate is about 7000 t km⁻² yr⁻¹ during this period. Since then, the soil erosion rate has been significantly reduced due to soil conservation, and current soil erosion is only 1500 t km⁻² yr⁻¹. We have reworded the description in the revised manuscript. (lines 95-97)

L102-103: “[The altered CO₂ exchange] remains to be quantified”. Didn’t you quantify this in Ran et al. (2017)? If so, how does this “remain to be quantified”?

Reply: We have quantified the CO₂ exchange in our earlier work (i.e., Ran et al., 2017. *Journal of Geophysical Research: Biogeosciences*, 122, 1439-1455) and have removed this statement from the revised manuscript.

L110: Change “was” to “is”.

Reply: Changed.

L124: Change “triple” to “triplicate”.

Reply: Changed.

L127: “radiocarbon Δ¹⁴C samples” is somewhat redundant. Change to “collected samples for ¹⁴C analysis” or similar.

Reply: Changed. Many thanks.

L147 (and throughout): “The Δ¹⁴C values were reported as percent modern carbon (pMC)”. These are two separate units! (see above discussion).

Reply: We have clarified the description in the revised manuscript ‘The ^{14}C results were reported as percent modern carbon (pMC)’. (lines 153-156)

L156: How reliable is this method for calculating carbon loads? This seems too simple. Why was something like LoadEst not used?

Reply: Estimating riverine carbon flux is a very important part of this study in which we attempt to investigate the fate of carbon after entering the drainage network from terrestrial ecosystems. Just as you have pointed out, there are a number of methods to estimate the annual fluxes of dissolved and particulate matter transported by rivers. Major methods currently used include linear interpolation and ratio estimators, regression-based methods historically employed by the USGS, and recent flexible techniques such as Weighted Regressions on Time, Discharge, and Season (WRTDS), etc. As you have also suggested, the most commonly used USGS software package for estimating constituent load using regression is known as LOADEST (Runkel et al., 2004. Load Estimator (LOADEST): A FORTRAN Program for Estimating Constituent Loads in Streams and Rivers. U. S. Geological Survey Techniques and Methods Book 4, Chapter A5). Lee et al. (2016) recently reviewed the potential for flux estimation bias across a broader range of estimation methods and concluded that the Beale’s ratio estimator and WRTDS generally exhibit greater estimation accuracy and lower bias (Lee et al., 2016. An evaluation of methods for estimating decadal stream loads. *Journal of Hydrology*, 542, 185-203). Our annual carbon flux estimation in this study was based on the Beale’s stratified ratio estimator. Since the riverine carbon concentrations were measured with “sparse” sampling frequency while flow and suspended sediment had a continuous daily measurement, this method could greatly reduce the bias introduced by relatively low sampling frequency, in particular the high flow events that are often under-sampled (Parks and Baker. 1997. Sources and transport of organic carbon in an Arizona river-reservoir system. *Water Research*, 31, 1751-1759). Indeed, we have already used the Beale’s ratio estimator in our earlier estimation of carbon flux in the Yellow River with success (i.e., Ran et al., 2013. Spatial and seasonal variability of organic carbon transport in the Yellow River, China. *Journal of Hydrology*, 498, 76-88). And the Beale’s ratio estimator has proven to be highly reliable and is recommended if the relationship between discharge and concentration is weak (e.g., Fulweiler and Nixon, 2005. *Biogeochemistry*, 74, 115-130; Awad et al., 2017. *Environmental Pollution*, 220, 788–796; Chen et al., 2014. *Journal of Geophysical Research: Biogeosciences*, 119, 95-109; Sun et al., 2017. *Hydrological Processes*, 31, 2062-2075). In comparison, we have also estimated the carbon flux by using the suggested LOADEST software package. The flux results show high consistency with each other, with a difference of less than 4.5%. We have added a detailed description of the estimate method (i.e., the Beale’s ratio estimator) in the revised manuscript. Please refer to the highlighted changes in the text. (lines 161-180)

L160: “by multiplying annual sediment deposition rate...” How was deposition rate calculated? This is not described at all in the text.

Reply: Our earlier work (Ran et al., 2013. *Global and Planetary Change*, 100, 308-319) has estimated the average annual sediment deposition rate behind all check dams in the study catchment by considering sediment input into each check dam and its sediment trapping efficiency. This has been added into the revised manuscript. (lines 191-193)

L162: Change “was” to “were”. Were these CO₂ flux data taken directly from Ran et al. (2017),

or are these new data originally presented in this study? Overall, I would clearly state which data are new and which data are taken from previous studies (as these authors appear to have published multiple papers on this dataset...)

Reply: Changed. The CO₂ efflux data are taken from our earlier work (Ran et al., 2017). This study is built upon our earlier work (i.e., Ran et al., 2017). But in Ran et al. (2017), we only explored the environmental controls and dam impoundment impact on areal CO₂ emissions (mmol m⁻² d⁻¹). In this study, we aim to evaluate the riverine C budget by considering lateral C export, OC burial, and CO₂ emissions from the whole drainage network. We have clearly stated which data are new (thus presented in the manuscript or in Supplement) and which data have been presented in our earlier work (i.e., Ran et al., 2017) in the revised manuscript. (lines 146; 187-188; 23-238; 271-272; 309-310)

L172: Please provide the minimum catchment threshold area, as this will affect calculated Strahler stream order.

Reply: To delineate the stream network, a threshold value of 100 cells (90-m resolution) was set on the assumption that a stream initiates within the cells. The delineated stream network was then classified using the Strahler ordering system. We have applied this minimum catchment threshold to delineate the whole Yellow River catchment and the result was validated with ground-truthing (Ran et al., 2015. *JGR-Biogeosciences*, 120, 1334-1347). This description has been added into the manuscript. (lines 195-200)

L177: How valid is the assumption that “each round of field sampling [is] representative of CO₂ emissions” for these four-month periods? What about for DIC, DOC, and POC concentrations – presumably you assume these are representative too?

Reply: Located in the arid-semiarid climate zone, the surface water pCO₂ in the Wuding River catchment shows temporal variations between the dry and wet seasons (please see Ran et al., 2017. *JGR-Biogeosciences*, 122, 1439-1455). However, the pCO₂ is generally consistent within the dry (or wet) season, which is probably because of the dominance of groundwater inflow. Our fieldwork in each campaign lasted ~25 days, and repeated pCO₂ measurements at some sites over 20-day intervals show high consistency (e.g., <6% difference). Thus, in view of the hydrologic regime (mainly groundwater input), we assumed that the three sampling campaign results in different seasons are representative of CO₂ emissions in the three four-month periods. To make it clearer, we have added ‘a first-order estimate’ into the statement. As for the DIC, DOC, and POC concentrations, we instead used the monthly sampling results at the catchment outlet (i.e., Baijiachuan gauge in Fig. 1) for the yearly flux calculation. (lines 205-206)

L189: Heterotrophic soil respiration need not be due to bacteria – this could also be fungal or archaeal respiration. I would simply stick with “heterotrophs”.

Reply: Many thanks. We have revised the statement and ‘heterotrophs’ was used instead.

L216: Is this decline (insofar as it is statistically significant) really “remarkable”?

Reply: We performed a one-way ANOVA test for the DIC in the loess subcatchment. The p value in spring, summer, and autumn is 0.02, 0.05, and 0.01. Thus, we concluded that this decline is remarkable (statistically significant). We have added the ‘one-way ANOVA test, p≤0.05’ into the manuscript to justify the claim. (lines 245-247)

L225: I'm confused by the sentence beginning with "Because the flow regime in 2017 was significantly biased..." What is this saying? You applied the 2015 hydrological regime to the 2017 data?

Reply: The flow regime in 2017 was significantly biased due to an extreme flood on 25-26 July caused by heavy rainstorms (maximum daily rainfall: 203 mm; spontaneous discharge: 4490 m³/s with a return period of 200 years. Figure S1 in Supplement). In comparison, the multi-annual mean water discharge is 35 m³/s. As a result, the annual water flux in 2017 is 1.5-fold the recent mean annual water flux (2000-2015). Because both CO₂ emissions and NPP were measured in 2015, we used the hydrological data for 2015 to calculate downstream carbon export by assuming that carbon concentration was comparable in 2015 and 2017 and evaluated the carbon budget. We realized that this may have caused errors to the flux estimation. We also calculated the carbon flux based on the 2017 flow data. The results show that, if the extreme flood on 25-26 July was excluded, the carbon flux in 2017 is close to that in 2015 (7.3×10^{10} vs. $(7 \pm 1.9) \times 10^{10}$ g). We have revised the statement in the manuscript for clarity and a new reference has been added to justify the impact of this extreme flood (He et al., 2018. *Geomatics, Natural Hazards and Risk*, 9, 70-18) (lines 256-261). In addition, we have also added a detailed description of the extreme flood event on 25-26 July in the Supplement (Figure S1).

L230: Fluxes should be in units of "g C yr⁻¹" (I'm assuming the "yr⁻¹" got dropped by accident).

Reply: Because we have already mentioned the 'annual' in the statement (The *annual* downstream carbon export...), adding 'yr⁻¹' is redundant.

L257: This should read "Assuming the water surface area remained constant..."

Reply: Changed.

L265 (and throughout) Please add "VPDB" after "%" when reporting δ¹³C values.

Reply: Added throughout the manuscript. Thanks a lot.

L277: Should "soils" instead read "sediments"? How can sediment cores contain "soils"?

Reply: Revised.

L300: How turbid are these rivers? If they are quite turbid, then I would expect that photochemical degradation is probably insignificant.

Reply: The Wuding River catchment is one of the major sediment sources of the Yellow River as a result of severe soil erosion. The average suspended sediment concentration in recent years is in the range of 7900-11,000 mg/L, and it can reach 120,000 mg/L during floods. The extreme of in 2017 is recorded on 26 July at 488,000 mg/L. Thus, we have removed the claim of photochemical degradation from the text. (lines 340-342)

L301 (and throughout): Please change "labile" to "bioavailable" as this language is more consistent with our current understanding of OC decay dynamics.

Reply: Changed throughout the manuscript.

L312: I'm confused by the statement "...and the mixture of carbon export from the two subcatchments." I thought the "6th mainstem channels" are the two subcatchments, which combine to form the 7th order Wuding River? Or have I misinterpreted this? (It is hard to see on

Figure 1).

Reply: We have double checked the drainage network in Figure 1. The mainstem channel of the northwestern sandy subcatchment is in the 5th order and the mainstem channel of the southwestern loess subcatchment is in the 6th order. Thus, when the two subcatchment mainstem channels confluence, it is still a 6th order river. We have carefully adjusted this figure in terms of color scheme, marker size, label size, etc., and have added the subcatchment boundaries to make the figure much easier to read. Please refer to the revised Figure 1 for the changes.

L330: Similarly, please change “biogeochemically refractory” to “persistent” in order to be consistent with our current understanding of OC decay dynamics.

Reply: Many thanks for your suggestion. We have replaced this statement in the text.

L358 & 362: Phytoplankton are not aquatic plants. Please clarify this language.

Reply: Thanks a lot for your comment. While phytoplankton are plant-like in their ability to use sunlight to convert CO₂ and water into energy, they are not plants. We have reworded the claim in the revised manuscript ‘...intensive nutrient loading from agricultural fields may have facilitated the growth of phytoplankton like algae, ...’. (lines 402-405)

L426: I’m confused by the inclusion of this sentence – what does it add to the discussion?

Reply: It seems this sentence is irrelevant to the discussion as you commented. It has thus been removed.

L464: Is the correlation between d13C and Δ14C statistically significant? Figure 9 does not report the regression slope equation nor any statistics, so I have no way of gauging the strength of this relationship (I’m not even sure if the line drawn in Figure 9 is a regression line...) Please clarify.

Reply: Based on your comment, we have performed the regression analysis by using the results of all the three sampling campaigns and have added the regression equation (slope and r²) into the revised figure. Accordingly, we have clarified the argument in the revised text. Thanks a lot for your comment. (lines 525-528)

L470: “...which suggests the outgassing of ancient terrestrial OC after entering aquatic systems”. I’m confused here – OC itself cannot be outgassed. Does this refer to CO₂ generated from remineralization of old OC? If so, how do the authors know that this was remineralized after entering the aquatic system and not simply remineralized in soils and transported with soil pore waters?

Reply: As you have commented, the OC itself cannot be directly outgassed from the water-air interface. Here it refers to the CO₂ generated from remineralization of old OC, which is mineralized either in soils and then transported into rivers or in aquatic systems during transit. We have reworded the claim in the revised manuscript ‘This suggests that the emitted CO₂ is derived from ancient terrestrial OC which is mineralized either in soils and then transported into rivers or in aquatic systems during transit...’. (lines 533-536)

L472: This claim (and others throughout the manuscript) isn’t necessarily supported – I would urge caution when making concrete statements such as this. Rather, I would phrase this along the lines of “These results are consistent with...”

Reply: Thanks a lot for your comment. We have rephrased the wording of these claims, including this one and also others throughout the manuscript, to make them more accurate and appropriate. Thanks again. (lines 469-482; 495-504; 525-528; 533-540, etc.)

L510: I'm confused by the statement "...this percentage (16%) falls into the range of global-scale estimates of 50-70%..." 16% is not in the range of 50-70%... am I missing something?

Reply: Many thanks for pointing out this misinterpretation. The percentage of 16% in this study is lower than the global-scale estimate of 50-70% by Cole et al. (2007). We have revised this in the manuscript '... this percentage (i.e., 16%) is much lower than the global-scale estimate of 50-70% by Cole et al. (2007)'. (lines 575-577)

L514: In what way is the estimate of Cole et al. (2007) "conservative"?

Reply: Based on published estimates of gas exchange, sediment accumulation, and carbon transport, Cole et al. (2007) constructed a carbon budget for the role of inland waters (particularly lakes, rivers, and reservoirs) in the global carbon cycle. However, constrained by data availability, they were not able to characterize carbon transport in each inland water body and in most cases, they used mid-range values for the estimation. This can be seen from their paper, they repeatedly mentioned that their carbon flux estimates are conservative and associated with considerable uncertainties. The full citation is: Cole et al., 2007. Plumbing the global carbon cycle: Integrating inland waters into the terrestrial carbon budget. *Ecosystems*, 10, 171-184.

L516-517 (and 531): Please remove "... it is worth noting that".

Reply: Both have been removed based on your suggestions.

L537: Remove the dash between "terrestrially" and "derived".

Reply: Changed.

L548: "CO2 emissions represented an important pathway..." An important pathway for what? Carbon loss from the landscape? I would change this to "CO2 emissions are quantitatively important..." or similar.

Reply: We have rephrased the claim based on your suggestion. Thanks a lot.

Figure 1: This figure is hard to read given the current color scheme and marker sizes. I would consider changing the color scheme for clarity and making the markers significantly larger. Also, please provide a catchment outline for the "sandy" and "loess" subcatchments, as this delineation is currently unclear.

Reply: Thanks a lot for your suggestion. We have carefully adjusted this figure in terms of color scheme, marker size, label size, etc., and have added the subcatchment boundaries to make the figure much easier to read.

Figures 2-3: I would consider writing "Loess Subcatchment" and "Sandy Subcatchment" above panels (a) and (b) so that the reader does not have to dig through the caption to understand what is presented.

Reply: The names of the two subcatchments have been added into the two panels in both figures. Many thanks for your suggestion.

Figure 4: I'm confused by what the percentage numbers represent. This should be clarified in the figure caption.

Reply: The percentage above each order in (b) represents the proportion of CO₂ emissions from that order streams to the total CO₂ emissions. This has been clarified in the figure caption.

Figure 5: Why has the nomenclature and color scheme changed for this figure? Why not use the colored bars from Figures 2-3 and the “spring”, “summer”, “autumn” notation that is used throughout the text? Also, what is meant by “conventional age”? Is this equivalent to 14C yr BP?

Reply: The nomenclature and color scheme for this figure have been adjusted for consistency by using the same notation in the text. In addition, the caption has been revised to ‘Seasonal variations in radiocarbon ages (year before present, BP) for the emitted CO₂ from the Wuding River catchment’.

Figure 7: Why is this figure showing NPP when the authors are interested in NEP? Why not show NEP directly? Also, please include the river network, subcatchment outlines, labels, etc. as in Figure 1.

Reply: Because NEP is calculated from NPP by subtracting the heterotrophic soil respiration (R_h) and the R_h is just a single numerical figure, we presented the spatial variation of the NPP within the catchment. The river network, subcatchment outlines, labels, etc. have now been included in the revised figure.

Figure 9: Why are units of pMC (which is not the same as $\Delta^{14}C$!) used in this Figure but 14C years used in Figure 5? Is this dashed line a regression line? If so, please include the regression equation and statistics. Technically, “young” and “old” only correspond to the y-axis and should point vertically, as the x-axis of this figure says nothing about age.

Reply: Based on your earlier comment, we chosen to use the percent modern (pMC) to describe the ¹⁴C results throughout the manuscript. To compare our results with Wang et al (2012. *Global Biogeochemical Cycles*, 10, 26, doi:10.1029/2011GB004130) that investigated the ¹⁴C age of DOC and POC in the Yellow River, we kept the ‘¹⁴C yr BP’ results in Figure 5. In addition, we have included the regression equation and statistics (r^2) in the figure.

Figure 10: I'm confused by the inset pie chart – what does 100% represent? Is this all of the carbon in the river network? If so, at what time points, or does this represent the relative annual fluxes? Again, more detail in the caption would be very much appreciated.

Reply: The inserted pie chart denotes the partitioning of riverine carbon among its five phases with the sum (100%) representing all the carbon entering the river network (i.e., $(18.5 \pm 4.5) \times 10^{10}$ g C year⁻¹). We have added more details into the figure caption.

Figure captions: In general, I would like to see significantly more description in this figure captions.

Reply: We have significantly improved the figure captions on the basis of your comments and detailed information has been added. Please refer to the highlighted changes in the revised manuscript.