Dear Dr. Abril,

Based on the two reviewers' valuable comments, we have thoroughly revised the manuscript. Please see below the detailed responses. All suggested changes to wording have been incorporated into the revised manuscript. Major changes have also been highlighted.

Many thanks for accepting our manuscript for publication in BG.

Best regards
Lishan Ran, on behalf of all co-authors

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Referee #1

The manuscript is greatly improved with answers to my questions. I think this manuscript can be published with small corrections (see below).

The use of the terms, "concentration" and "content" is still confusing. For example, line 23: "POC%" is concentration not content. Please double check the use of the words throughout the manuscript.
Reply: We measured the bulk POC content of the total suspended solids (TSS). According to Tolhurst et al. (2005. Content versus concentration: Effects of units on measuring the biogeochemical properties of soft sediments. *Estuarine, Coastal and Shelf Science*, 63, 665-673), the *content* denotes the mass fraction or mass per unit mass and is a unitless ratio. Therefore, here the POC%, a percentage of the TSS (dry weight), is content. Similar expression can also be found in literature (e.g., Alin et al., 2008. Biogeochemical characterization of carbon sources in the Strickland and Fly rivers, Papua New Guinea. *Journal of Geophysical Research*, 113, F01S05). To make it clear and consistent throughout the manuscript, we have used the term 'POC%' throughout the manuscript.

Lines 45-: The range of CO2 emissions needs to be expanded to be conservative. Please check out "Lauerwald et al., 2015, Global Biogeochemical Cycles, Spatial patterns in CO2 evasion from the global river network".
Reply: The recent estimate of global CO_2 emissions from rivers of 0.65 Pg C $year^{-1}$ by Lauerwald et al. (2015) has been added into the text. Many thanks for your constructive suggestion.

Referee #2

Review of Ran et al. "Riverine carbon export in the arid-semiarid Wuding River catchment on the Chinese Loess Plateau" (bg-2018-51), first revision

The authors have addressed the majority of my earlier comments and suggestions.
Therefore, I now have only a handful of minor comments that I feel should be addressed before

publication. Again, please do not hesitate to contact me for further discussion regarding this review.

Sincerely,

Jordon Hemingway
jordon_hemingway@fas.harvard.edu

Dear Dr. Hemingway,

Many thanks for your insightful comments and suggestions, which have greatly improved our manuscript. Please find below our responses to each of your comments.

Minor Comments

L7 (and throughout, including Table 1, Table 2, and Fig. 10): The significant figure in the tenth's place appears to be dropped, presumably when ending in a zero. For example, "7±1.9" on L7 should read "7.0±1.9", etc. Please update the significant figures to be consistent throughout.
Reply: This is because all these figures end in a zero. We have updated all the figures to be consistent throughout the manuscript. Thanks a lot.

L33: I'm still slightly confused about how lateral transport is "significantly offsetting" NEP. Perhaps re-word to something along the lines of: "It appears that a significant fraction of terrestrial NEP in this arid-semiarid catchment is laterally transported from the terrestrial biosphere to the drainage network." (or similar)
Reply: Based on your suggestion, this sentence has been re-worded (lines 32-34). Many thanks.

L70: It's not immediately clear what the "three pathways" is referring to. Consider re-wording to: "…among its three pathways; that is 1) downstream export to the catchment outlet, 2) CO2 evasion from the water surface, and 3) organic carbon burial…"
Reply: Based on your suggestion, this sentence has been re-worded. (lines 70-73)

L87: Insert a comma before "generally" and after "soils".
Reply: Added.

L96: Saying "once suffered" sounds like a single event, while the time period 1956–1969 implies a sustained phenomenon. Consider re-wording to something like: "… the Wuding River catchment has experienced a maximum, decadal averaged soil erosion rate as high as 7000 t km-2 yr-1 (1956-1969)" or similar.
Reply: Based on your suggestion, this sentence has been re-worded. (lines 96-97)

L143: Were these Gran titrations or end-point titrations? This should be specified.
Reply: The alkalinity was determined by triplicate end-point titrations by using methyl orange as the indicator. This has been specified in the text.

L149: Add "and pestle" after "mortar".

Reply: Added.

L157: Beta Analytic measures d13C using an off-line IRMS, not simultaneously on the AMS (AMS-derived 13C compositions are generally neither precise nor accurate). See: https://www.radiocarbon.com/dietary-isotopic-analysis.htm
Reply: The 'isotope ratio mass spectrometer (IRMS)' has been added into the manuscript. Many thanks for providing very accurate details. (line 157)

L169 (and 176): Remove the comma after "where".
Reply: Removed.

L174: Add a line that says "and" between these two equations.
Reply: Added.

L222: Change "calculated" to "calculate".
Reply: Changed.

L240: Change "averaged" to "average".
Reply: Changed.

L290: I'm a bit confused by these sentences. I think the authors are saying that spring and autumn CO2 outgassing fluxes summed to 246 million mol, summer ingassing flux was 208 million mol, and these add up to a *net outgassing* flux of 38 million mol. Then, when added with the river efflux estimate, the *catchment total* adds up to $(3.7\pm0.5)x10^{10}$ g C in the year 2015. I would re-word these sentences to clarify this. Additionally, the reservoir CO2 emissions estimates appear to have large uncertainties, which should be reported and addressed here. For example, I calculate the net outgassing flux to be 38±280 million mol, which is, of course, indistinguishable from zero. Propagating this error, I calculate a catchment total value of $(3.7\pm0.6)x10^{10}$ g C (note the higher uncertainty).
Reply: Just as you have mentioned, the spring and autumn CO_2 outgassing fluxes were summed to 246 million mol (i.e., 81 million mol in spring and 165 million mol in autumn; Table 1), the summer CO_2 ingassing flux was 208 million mol, and these added up to a net outgassing flux of 38 million mol. Based on your suggestion, we have re-worded these sentences to make them clearer. While for the reservoir CO_2 emissions estimates, the great uncertainties as shown in Table 2 were largely because CO_2 effluxes in reservoirs were characterized by large temporal variations, particularly in spring. While the sandy subcatchment reservoirs in spring showed net CO_2 outgassing (28 mmol m^{-2} d^{-1}), the loess subcatchment reservoirs in spring act as a net CO_2 sink (-2.9 mmol m^{-2} d^{-1}). This caused the great uncertainties in the CO_2 efflux estimate in spring, which propagated to a great uncertainty in the annual total CO_2 efflux estimate as you pointed out. However, because the CO_2 efflux from reservoirs accounted for only about 1.37% of the river CO_2 efflux estimate $(3.65\pm0.5)\times10^{10}$ g C) or 1.35% of the total CO_2 efflux (rivers + reservoirs), this uncertainty is not likely to significantly affect the catchment total CO_2 efflux estimate. Furthermore, we have updated the uncertainty of the total CO_2 efflux estimate (i.e., $(3.7\pm0.6)\times10^{10}$ g C). Thanks a lot for your valuable comments and suggestions. (lines 291-297)

L299: There appears to be a typo in reporting these numbers (e.g. "-30.2±‰").

Reply: These numbers have been updated.

L300: “conventional” should be replaced by “radiocarbon”
Reply: Changed.

L301 (and throughout, including Table 2 and Fig. 5): “years” should be “14C yr BP”
Reply: Changed throughout the manuscript.

L359: “leached” implies going from the solid to liquid phase. Consider changing this to “adsorbed within deeper soils…”
Reply: Changed.

L475: I would recommend noting the possibility that secondary OC sources (namely, phytoplankton) could contribute to that observed in check-dam sediments.
Reply: Many thanks for your insightful comment. We have added the possible contribution from secondary OC sources (e.g., phytoplankton) into the text. (lines 480-482)

L495: A d13C value of 0‰ for carbonate-dominated rivers is the DIC value, not the CO2 value. Keep in mind that CO2 will be more depleted than DIC.
Reply: Yes, the $\delta^{13}C$ value of 0‰ for carbonate-dominated rivers is the dissolved inorganic carbon value, not the emitted CO_2 value. Affected by the fractionation process (preferential outgassing of $^{12}CO_2$), the $\delta^{13}C$ value of the emitted CO_2 is more depleted than DIC. We have discussed this in the manuscript (lines 520-523).

L561: I feel that this could be expanded a bit. Specifically, how would the uncertainty in SR and Rh propagate in to the estimated percent of NEP that is laterally exported? It would be useful to know how certain the authors are in their “16% of NEP” number. There appears to be uncertainty about the NEP number as reported in Fig. 10, but this isn’t included in the text.
Reply: Considering the land cover and land use type in the Wuding River catchment, we further evaluated the uncertainty associated with the S_R and R_h estimation: “If the ratio is reduced to 35%, the proportion of lateral export to NEP would decrease by 5.6%. Further research involving field experiments and remote sensing technique is thus needed to constrain this estimate”. Based on your suggestion, we have expanded the uncertainty discussion, including the NEP results in Fig. 10, into the manuscript. (lines 564-569)

L604: I would be careful in claiming that this is a “typical” study area. It is likely quite unique due to its location on the loess plateau as well as the large anthropogenic disturbance (including check dams).
Reply: Revised. Thanks a lot.

Table 1: What is the timescale for the “million mol” columns? Is this “million mol CO2 yr-1”?
Reply: The timescale is $year^{-1}$. The unite has now been revised to ‘million mol CO_2 yr^{-1}’.

Fig. 7: This is a lot of significant figures for the NPP legend! Do the authors really trust these values to be so precise?

Reply: Based on your comment, we have revised the legend by reducing the figures to 3 decimal places. Thanks a lot.

Fig. 9: In light of the authors' response to my earlier comments, I think it might actually make sense to keep this figure in 14C yr BP rather than pMC. The authors stated that they wanted to use 14C yr BP in this study in order to compare to previous studies, which makes sense and, for consistency, it would be logical to report this figure in the same units. Note that, in that case, a linear regression between 14C yr and d13C likely doesn't make much sense and could be removed. This seems okay to me since the authors don't really need (or discuss) this regression trend.

Reply: Based on your suggestion, we have re-plotted this figure in ^{14}C year BP against $\delta^{13}C$ for comparison with previous studies. Also, this figure is now consistent with Fig. 5 in which the same units (i.e., ^{14}C year BP) were used. In addition, because we have discussed the relationship between radiocarbon age and $\delta^{13}C$ in section 4.3, we choose to retain the linear regression trend as you suggested in your earlier comments. Many thanks for your very insightful and valuable comments and suggestions.