

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

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.

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)

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...”

L87: Insert a comma before “generally” and after “soils”.

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.

L143: Were these Gran titrations or end-point titrations? This should be specified.

L149: Add “and pestle” after “mortar”.

L157: Beta Analytic measures $\delta^{13}\text{C}$ using an off-line IRMS, not simultaneously on the AMS (AMS-derived ^{13}C compositions are generally neither precise nor accurate). See: <https://www.radiocarbon.com/dietary-isotopic-analysis.htm>

L169 (and 176): Remove the comma after “where”.

L174: Add a line that says “and” between these two equations.

L222: Change “calculated” to “calculate”.

L240: Change “averaged” to “average”.

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 $(3.7 \pm 0.5) \times 10^{10}$ 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 $(3.7 \pm 0.6) \times 10^{10}$ g C (note the higher uncertainty).

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

L300: “conventional” should be replaced by “radiocarbon”

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

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

L475: I would recommend noting the possibility that secondary OC sources (namely, phytoplankton) could contribute to that observed in check-dam sediments.

L495: A δ¹³C 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.

L561: I feel that this could be expanded a bit. Specifically, how would the uncertainty in S_R and R_h 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.

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).

Table 1: What is the timescale for the “million mol” columns? Is this “million mol CO₂ 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?

Fig. 9: In light of the authors' response to my earlier comments, I think it might actually make sense to keep this figure in ^{14}C yr BP rather than pMC. The authors stated that they wanted to use ^{14}C 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 ^{14}C yr and $\delta^{13}\text{C}$ 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.