Review of Douglas et al. "Organic carbon burial by river meandering partially offsets bank-erosion carbon fluxes in a discontinuous permafrost floodplain" (esurf-2021-80)

Synopsis

The primary focus of this study is to constrain the source, age, and flux of carbon that is entrained, transported, and deposited along an Arctic river underlain by discontinuous permafrost (Koyukuk River, Alaska, USA). In particular, the authors collected a suite of samples from eroding cut banks, depositional point bars, and floodplain deposits and compared their grain size, organic carbon content (TOC), and organic carbon $\delta^{13}C$ and ^{14}C activity (Fm). They found that finer-grained material is associated with higher TOC and higher Fm, but that carbon stocks are statistically identical in cut banks, point bars, and floodplains. As a consequence of this result, the authors argue that a large fraction of mobilized OC is redeposited and aged during fluvial transport (independent of permafrost cover), rather than being oxidized to CO_2.

Overall, I find this study to be highly topical and relevant to an import carbon-cycle question. In particular, I find the combination of techniques taken from organic geochemistry (i.e., $\delta^{13}C$ and Fm) as well as geomorphology/sedimentology (i.e., migration rates, stratigraphic columns, etc.) to be an exciting contribution. That said, there are several statements and calculations that are contradicting, circular, or warrant further clarification—these particularly relate to the interpretation of Fm results. Most importantly: (i) the calculation of individual Fm_{bio} estimates for each sample requires the authors to input TOC_{petro}, which is calculated by assuming a constant Fm_{bio} for all samples—this is circular logic. And (ii) the calculations used herein lead to incorrect (artificially high) estimates of the fraction of OC that is produced *in situ*. Still, these issues do not invalidate the main conclusion of the paper, which is that OC stocks in eroding cut banks and deposited point bars are statistically identical, independent of the presence of permafrost.

I detail these points below, along with some minor (line-item) comments. Once these issues have been fully addressed, I support publication of this manuscript in *ESurf*. Please do not hesitate to contact me for further details regarding this review.

Sincerely,

Jordon Hemingway
jordon.hemingway@erdw.ethz.ch

L14-15: “Radiocarbon content” and “radiocarbon abundance” should be replaced by “radiocarbon activity,” since this is the unit of currency used in radiocarbon measurements.

L14-15: “TOC” should be “TOC content” or “TOC abundance” or similar.

L75: This should read, “…fluxes of OC into *and out of* the river…”

L85: This is not the correct definition of Fm. This should instead be:

$$\mathrm{Fm} = \frac{\mathrm{A_{s,N}}}{0.95\times \mathrm{A_{Ox,N}}},$$

where A_s is the ^{14}C *activity* of the sample, A_{Ox} is the ^{14}C *activity* of the Ox-I oxalic acid standard, “N” indicates that both activities are normalized for isotope fractionation (to $\delta^{13}C$ = -25‰ for the sample and -19‰ for Ox-I), and the scalar 0.95 is added by convention. See Reimer et al. (2004) *Radiocarbon* **46**, 1299–1304.

Additionally, it sounds a bit strange to say “…with low fractions *of* modern radiocarbon…” I suggest rewording to “…with OC containing low radiocarbon activity, expressed as fraction modern (Fm = …”

L109: “…with a constant mass fraction of petrogenic OC.” State why OC_{petro} weight percent should be constant—i.e., since it is all eroded from the same bedrock units, independent of grain size.

L111-120: This set of equations (and possibly the subsequent regression and results) is not strictly accurate. If I read this right, Eq. (4) simply reduces to “$Fm_{meas} = Fm_{bio}(1 - f_{petro})$”. The authors state that the mass fraction of OC_{petro} is constant (which is reasonable), but this is not the same as f_{petro} being constant! Rather, f_{petro} is the fraction *of total OC* that is petrogenic. This should instead be rewritten as

$$TOC_{meas} = TOC_{bio} + TOC_{petro},$$

and

$$TOC_{meas}\times Fm_{meas} = TOC_{bio}\times Fm_{bio} + TOC_{petro}\times Fm_{petro},$$

such that

$$\mathrm{Fm_{meas}} = \mathrm{Fm_{bio}} - \mathrm{TOC_{petro}} \times \mathrm{Fm_{bio}}\left(\frac{1}{\mathrm{TOC_{meas}}}\right).$$

Then, a plot of TOC_{meas} (or $1/TOC_{meas}$) vs. Fm_{meas} yields an intercept that is Fm_{bio} and a slope (or curvature) that is proportional to TOC_{petro}, which is the metric that the authors assume to be constant across all samples.

L121: Right, here it is clear that TOC_{petro}, not f_{petro}, is constant!

L130: Note that this mixing model is only valid if $Fm_{bio,is} > Fm_{bio}$, which might not always be the case due to the decreasing ^{14}C activity of the atmosphere since nuclear bomb testing ended in the 1960s. That is, biospheric OC fixed ~60 years ago will have a higher Fm value that *in situ* biospehric OC being fixed today. Incorporation of OC fixed over the past ~60 years would therefore artificially increase the calculated $f_{bio,is}$. These complexities need to be addressed and discussed.

L131: "…Fm_{bio} is the fraction modern of biospheric OC for each sediment sample…" But won't this be the same for all samples, since a regression of Eq. (4) yields only a single Fm_{bio} value for the whole sample set? Then, if $Fm_{bio,is}$ and $Fm_{bio,cb}$ are assumed to be constant, I don't see how Eq. (5) doesn't yield a constant value for $f_{bio,is}$ for all samples.

L134: But is the "oldest woody debris from a cutbank" actually representative of the cutbank end member? Should this not instead be the *average* Fm value of all cutbank samples (or at least the average of the cutbank woody debris samples)? Again, the choice used by the authors here will artificially increase the calculated $f_{bio,is}$ (i.e., since the true Fm of all OC inputted from the cutbank is almost certainly higher than that of the oldest woody debris). This should be discussed.

L189: Which values—LANL or NOSAMS—were used in all subsequent calculations? For some metrics in some samples, this appears to make quite a difference.

L217: This should read "Sediment TOC and radiocarbon Fm varied…" (remove "measurements")

L231-232: Can the authors elaborate on what they mean by "Fitting to TOC:TN weight ratios…"? Wouldn't this yield the (somewhat odd) result of $TOC_{petro}:TN_{meas}$? I'm confused by the motivation or benefit of calculating the regression in this way.

L249-251: But the regression in Fig. 4d requires the assumption of a *constant* Fm_{bio} across all samples in order to calculate TOC_{petro}. So, how can this result then be used to estimate variable Fm_{bio} values for each sediment sample? This seems circular and will, by definition, yield estimated Fm_{bio} values that are the same across all grain size classes.

L273: Remove one of the redundant "of" instances

Fig. 4: Again, I'm not really sure Fm_{bio} can be calculated for each individual sample, when the calculated inputs for this calculation (i.e., TOC_{petro}) *require* the assumption of constant Fm_{bio} across all samples (i.e., Eq. 4). This seems circular to me.

L349-350: Clarify by changing to "…reflects the greater *proportional* petrogenic OC contribution in coarser material"

L350-352: How can biospheric OC simultaneously have a “similar Fm for all grain sizes” but “fine sediment … contain higher Fm_{bio}”? These statements are contradicting. I’m also not convinced by the evidence for *in situ* biospheric production (as detailed above and below).

L354: Space between “being present”

L376-380: As mentioned in my above comments (L130, L134), this calculation will lead to an artificially high estimate of $f_{bio,is}$. Rather, the average Fm_{bio} for cut bank inputs—which is more representative for the calculation of $f_{bio,is}$—is much higher than 0.2319±0.00152.

This again comes back to the regression from Eq. (4) used to calculate a constant Fm_{bio} and TOC_{petro} across the entire sample set. Given this calculation, all samples will by definition have the same calculated Fm_{bio} and thus there is no “room” to oxidize and replace some TOC_{bio} with ^{14}C-enriched *in situ* material.

Furthermore, how is it possible that “cut banks have 72% of sediment TOC produced *in situ*” (L380), but cut bank Fm_{bio} ($Fm_{bio,cb}$) is also used as the non-*in situ* end-member in Eq. (5) (L130)? According to Eq. 5, shouldn’t cut banks have 0% *in situ* TOC by definition? This needs to be clarified and discussed further.

I am also now somewhat confused as to what specifically the authors mean by “*in situ*” production. Does this refer only to TOC produced during transport within the river itself (I think this is the case), or does this refer to any non-permafrost TOC_{bio} (which I think is what is implied by using the oldest woody debris for the non-*in situ* end member Fm value)? If the former, then why would the authors expect their results here to deviate so far from previous estimates (e.g., Scheingross et al., 2021), which conclude that there is very limited oxidation and *in situ* production during fluvial transport?

I think the assumptions and calculations used to calculate $f_{bio,is}$ need to be critically reevaluated. This also warrants updating some discussion points later in the manuscript (e.g., L390, L398, L461)