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## **Soil carbon and nitrogen erosion in forested catchments: implications for erosion-induced terrestrial carbon sequestration**

### **Author responses**

Please modify author affiliation for C.T. Hunsaker to indicate “Albany, CA”, or preferably, “Fresno, CA” [Pacific Southwest Research Station, US Forest Service, Fresno, CA, USA]

### **Response to: Biogeosciences Discuss., 12, comment C165–C174, 2015, Received and published February 12, 2015. [www.biogeosciences-discuss.net/12/C165/2015/](http://www.biogeosciences-discuss.net/12/C165/2015/)**

Comments on the manuscript are followed by a response and pertinent changes

**Comment:** General comments “Overall, the discussion needs important restructuring and can be much more concise by focusing on the papers principal results, leaving out additional comments on issues not included in the results section (e.g. the whole discussion on roads). Moreover, at various points discussion is included and conclusions are drawn without providing data to support these. Either the data must be provided (if existent) or otherwise the issue must be removed from discussion and conclusions.”

**Author response:** Thank you for the thorough feedback and consideration for the manuscript. The abstract was rewritten (L2-22 in the revised manuscript) to more succinctly cover the study topic. In addition, the introduction and discussion have been revised for clarity and to ensure that we are now focusing on our study's principal results as suggested by the reviewer. We have also referenced (or added) data where need to justify the results and discussion.

**Comment:** It would be good to add some photographs of the catchments vegetation cover.

**Author response:** We agree with the reviewer's suggestion that it is useful to add photographs given the discussion of the differences in elevation, forested cover, and transport in montane forests. Hence, we have added representative pictures of the forest as Figure 3. Figures of the catchment elevation and size (previously Figure 2), and annual precipitation (previously Figure 3) were combined into one, Figure 2.

**Comment:** The statistical data analysis (section 2.4) as well as its results needs further elaboration explaining which relations were evaluated exactly and showing more results in a more concise manner.

**Author response:** We have revised section 2.4 (now section 2.5) and the results section for clarity. In the revised manuscript, section 2.5 now reads “Data are presented as mean  $\pm$  standard error ( $n = 3$ ), except where noted. Explanatory factors for C and N concentrations and the C:N ratio of sediment and soil were evaluated with a multivariate model to account for sampling year, catchment, sampling depth, and hillslope position. The strength of different model formats and interactions terms was evaluated using a stepwise regression run simultaneously in both directions, with the best model chosen according to the Akaike Information Criterion (Burnham and Anderson, 2002). The Tukey-Kramer HSD test ANOVA was used to test for significant differences between means of sediment mass, and C or N concentrations between sediment basins and collection years, and between hillslope position and transects for soils. For all statistical tests, an a priori  $\alpha$  level of 0.05 was used to determine statistical significance. Statistical analyses were conducted using R 2.14.1 (<http://www.r-project.org>).” (L204-215).

**Comment:** Soils were sampled until 80cm depth, but results are only given for the upper 20 cm in Table 2. Why is that? What about results for greater depth?

**Author response:** Thank you for the input. Samples from 40-80 cm depth were sampled but never analyzed for all characteristics due to cost and time limitations; Soil values for samples 20-40 cm in depth have been added to Table 2. The methods have been modified to refer to data presented in the revised manuscript (L179).

**Comment P2492L16:** check units for sediments should not be kg N ha<sup>-1</sup> but kg ha<sup>-1</sup>.

**Author response:** Thank you for the input. The revised manuscript now reads kg ha<sup>-1</sup> (L 14), as well as the units for total N transportation (kg N ha<sup>-1</sup>, L12).

**Comment P2494L3:** please correct sentence.

**Author response:** The sentence had confusing wording, but it was actually removed entirely to streamline the introduction.

**Comment P2494L1-8:** Moreover, different erosion processes and transport distances also affect the possible breakdown of soil aggregates during transport, affecting protection of C and N (e.g. Nadeu et al., 2011; Boix-Fayos et al., 2015).

**Author response:** We agree that the type of erosion processes and transport distances can have important effects on protection of C and N. We have now included this information in the revised manuscript (L59-62).

**Comment P2495L4:** remove ‘erosion’ from ‘eroded C and N erosion’

**Author response:** We acknowledge this was a valid correction, but the whole phrase was removed for clarity and to simplify the sentence (L60).

**Comment P2495L5-11:** what about different Carbon pools in forested versus agricultural settings, being more or less sensitive to oxidation?

**Author response:** While stabilization mechanisms are the focus of a different part of the project, we have expanded this section to address this valid point: “This same assumption may not be valid in forested ecosystems because upland forest soils typically have much higher concentrations of OM in topsoil (as litter or OM-rich mineral topsoil) and C in forested soils or undisturbed grasslands is likely to have a bigger unprotected (free light) fraction compared to agricultural soils where most of the C is typically associated with the soil mineral fraction (Berhe et al., 2012, Wang et al., 2014, Wiaux et al., 2013 Stacy et al., in prep). These forested or undisturbed grassland sites would have a lot of OM not stabilized within aggregates or bound to mineral surfaces.” (L62-70)

**Comment P2495L22-25:** please correctly phrase the research questions either as a question or as an objective.

**Author response:** This was also mentioned by the other reviewer, and we think it is a justified change. The research questions were rephrased as part of a larger reorganization of the objectives and justification at the end of the introduction. The objectives were changed to questions (L102-106), and additional modifications were made to the section on the scope of this work (L87-110) including the inclusion of hypotheses.

**Comment P2496L20-22:** you already mentioned that a few lines before.

**Author response:** The repetition was removed, and consolidated earlier in the paragraph (L117-118).

**Comment** P2496L25: please define KREW.

**Author response:** The acronym was added at the first mention of the Kings River Experimental Watersheds (L114).

**Comment** P2497L7: What about texture in the Cagwin series?

**Author response:** Texture in the soil series description is not as clear as it is for Gerle or Shaver; It is not mentioned in the Taxonomic class, the typical pedon states “loamy coarse sand”, and the range in characteristics gives “coarse sand, sand, loamy coarse sand, or loamy sand” in the control section. ([https://soilseries.sc.egov.usda.gov/OSD\\_Docs/C/CAGWIN.html](https://soilseries.sc.egov.usda.gov/OSD_Docs/C/CAGWIN.html)). The revised manuscript includes the loamy coarse sand from the soil series typical pedon (L144).

**Comment** P2497L14: how much is less than 78%

**Author response:** We have revised this section for clarity. In the revised manuscript, we have listed the dominant tree species and referred the reader to previous publications for more information on land cover (L133-138).

**Comment** P2497L21-...: all following is methodology and should not be under study site description.

**Author response:** Agreed and we separated methodology into a different section (L149).

**Comment** P2498L4: Figure 1 shows 21 sampling points (not 18)?

**Author response:** This disparity resulted during site selection and sampling. In the revised manuscript, we clarified: “Sites were selected to be comparable as possible however transect P2 had a non-representative, highly saturated meadow as the depositional location. Transect P2 was not evaluated in further analyses since other depositional locations were in the forest.” (L170-172).

**Comment** P2498L15: how was slope calculated for these small depositional areas, less than 10m long, with a DEM resolution of 10m?

**Author response:** Slope was calculated using ArcGIS spatial tools that interpolate between raster points using the change in slope from point to point. The depositional areas are often narrow, especially along the streams where two slopes converge. This is evident in Figure 6, where steep slopes converging at a stream are denoted as continuous (though the slopes on either side of the stream are facing each other). The slope analysis, built from the 10 DEM, does gloss over small depositional areas in these cases. Larger depositional areas are apparent on the maps.

To further clarify, text was amended to: “These depositional areas cover a limited surface, sometimes only a few meters wide where slopes converge; the catchments are steep and have minimal flat surfaces near the creeks and drainages. To estimate slope at each sampling point, Spatial Analyst tools from the ArcGIS software ArcMap 10.0 (ESRI, Redlands, CA, USA) were used to calculate slope from a 10-m digital elevation model (DEM).” L178-182.

**Comment** P2499L1-7: can you say anything about the trapping efficiency of these boxes (see Verstraeten and Poesen 2000)? How much sediment passed the box and what was their C and N composition?

**Author response:** These sediment basins were constructed to fit the available space, instead of maximizing trapping efficiency. As part of the larger KREW project, sensors (Forest Technology Systems DTS-12) were installed in the streams. The sensors provide turbidity measurements which are used to estimate the suspended sediment concentration in the streams by correlation with grab samples (Hunsaker and Neary, 2012). However, there is unknown level of uncertainty with the reported numbers since the turbidity sensors were installed upstream of the sediment basins and no comparable sensors were installed downstream of the sediment catch basins. More on this point has been added to the discussion (L400-413).

**Comment** P2500L23-24: so do we have a higher or a lower concentration of sand in sediments as compared to soils?

**Author response:** The comparison was incorrectly stated as "sand vs. sand and silt", where it should have read "sand vs. *clay* and silt fractions". We have elaborated on this in the revised manuscript: "Sediments exported from all of the study catchments had statistically higher sand concentration, and lower clay concentrations, compared to surface soils in the source hillslope ( $p < 0.001$ ; **Error! Reference source not found.** and **Error! Reference source not found.**)" (281-283).

**Comment** P2501L4: how was water yield defined and measured? This was not explained under methods. Besides, reference should be to figure 4 not figure 2.

**Author response:** Stream discharge was measured using a dual flume design with depth sensors; full experimental design and methods describe in the KREW study plan [Hunsaker et al., 2007. Kings River Experimental Watershed research study plan. Available at [http://www.fs.fed.us/psw/topics/water/kingsriver/documents/KREW\\_Study\\_Plan\\_Sep2007](http://www.fs.fed.us/psw/topics/water/kingsriver/documents/KREW_Study_Plan_Sep2007)]. Annual water yield was integrated from average daily flow rates. This information was incorporated in the revised manuscript at L150-152. The figure reference was corrected (L225).

**Comment** P2501: the whole analysis of relation between water yield and sediment yield is potentially interesting, but does not seem to be relevant for your study and overall objectives. Leaving it out may give you a clearer message.

**Author response:** The relationship between water yield and sediment yield is important, in that the total sediment mass changes, but sediment composition does not similarly change with changing precipitation (in answer to one of the main hypotheses). The discussion of outliers and obtaining the perfect fit did not serve the same purpose and was shortened (Section 3.1, L234-235). Figure 4 has been altered to also include sediment C concentration and C:N ratio.

**Comment** P2501L24-25: you already mentioned that at the beginning of the paragraph right?

**Author response:** This was not a repetition but highlighted a particular aspect of the variability. It was rephrased slightly in the revised manuscript. (L227-228).

**Comment** P2502: the whole paragraph 3.3 is difficult to follow and would benefit from better structuring of the text.

**Author response:** Sections 3.3 and 3.2 were reversed in the revised manuscript to provide for a better flow from the first section of the results.

**Comment** P2502L2: again: do we have a higher or a lower concentration of sand in sediments as compared to soils?

**Author response:** The comparison was incorrectly stated as "sand vs. sand and silt", where it should have read "sand vs. *clay* and silt fractions". We have elaborated on this (and removed the repetition) in the revised manuscript: "Sediments exported from all of the study catchments had statistically higher sand concentration, and lower clay concentrations, compared to surface soils in the source hillslope ( $p < 0.001$ ; **Error! Reference source not found.** and **Error! Reference source not found.**)" (L284-286).

**Comment** P2502L6-12: sorry, I can't follow this sentence. Please re-write and simplify.

**Author response:** The sentence was rewritten and clarified in the revised manuscript: "Consistent with the coarser particles, sediment had lower specific surface area than for the soil. Of the three years evaluated, sediment from 2009 had the highest specific surface area ( $3.3 \pm 1.0 \text{ m}^2 \text{ g}^{-1}$ ; Table 2). Soil in the higher elevation B8 transect had a specific surface area of  $8.5 \pm 1.7$

$\text{m}^2 \text{g}^{-1}$ , while the lower elevation P4 transect had  $10.3 \pm 1.6 \text{ m}^2 \text{g}^{-1}$  (**Error! Reference source not found.**)” (L284-287).

**Comment P2503L24:** You probably mean Figure 5 a and b?

**Author response:** In the revised manuscript, we have simplified this discussion, letting the figure convey the data. The figure is referenced once and a few points are highlighted (L250-266).

**Comment P2503L26:** Interesting result, but where can we see this (figure, Table..)?

**Author response:** As part of the broader effort to clarify this part of the study, we have expanded Figure 2 to show the relationship between stream discharge and sediment C and C:N ratios (the correlation with N is not shown). The appropriate references were added to L250 and 252.

**Comment P2504L10:** what do you mean by ‘interactions between the variables’?

**Author response:** We have clarified in the revised manuscript: “An interaction term in the variables is a possible option in the general linear model for evaluating regressions, but only if there are sufficient samples. Without this term each annual sediment sample is considered an independent sample, but given the constraints of the sampling regime, we did not have enough samples to include this term.” (L258-262).

**Comment P2504L26-29:** Is this referring to Figure 6?

**Author response:** No, it is meant to refer to Figure 5e, highlighting the outliers in C:N ratio. The sentence was revised for added clarity (L266).

**Comment P2504L28:** what does this mean exactly: ‘For N, differences between each sediment year and the soil were even more pronounced’?

**Author response:** What was meant was that the difference between sediment and soil N concentrations was even greater than the difference between sediment and soil C concentrations. This has been simplified in the revised manuscript with a reliance on communicating data in the tables (L277-282).

**Comment P2505L5-8:** this is discussion, not a result.

**Author response:** This analysis was moved to the discussion in the revised manuscript (L389-390)

**Comment P2505L17:** I am not so convinced that climate comes out as an important factor. Your results do show that inter-annual differences in total annual precipitation is important, but no clear differences were found between higher and lower catchments, with more or less contribution from snow as compared to rainfall. So precipitation volume is important, no matter if it falls as rain or snow.

**Author response:** This is a good point. The discussion was clarified and is now more focused on stream discharge (data that was used in the analysis) and a distinction was made between the total precipitation amount, and precipitation form (L311-333).

**Comment P2505L21:** Where can we see the results of this analysis correlation analysis?

**Author response:** Catchment size, elevation, and elevation group were eliminated as part of the stepwise regression process referenced in the methods. The revised manuscript more clearly references the method (L252-254).

**Comment P2505L23:** Which subset? Please provide some more information.

**Author response:** The sediment basins were constructed over a period of years, and so early years only included data from a few of the basins. Timing of sediment collection was clarified in the revised manuscript in L319.

**Comment** P2505L25: I am not sure what you mean here by an ‘extreme sediment yield response’. There is a good correlation between water yield and sediment yield, but sediment yield values in your catchments are surely not extreme.

**Author response:** This part of the discussion was rewritten to highlight outliers, instead of “extreme” “Some catchments, particularly P304, had high sediment export rates that were disproportionately high.” (L369-383)

**Comment** P2506L1: better than what?

**Author response:** That sediment composition is better correlated with catchment characteristics than stream discharge; this addition was made in the manuscript. (L315-317).

**Comment** P2506L8: remove ‘sediment’. In fact, the entire sentence is unclear (Results from WY 2005–2011 supported. . .). Which hypothesis? Above you stated that the hypothesis was that catchment characteristics are more important. So what is your hypothesis? If you have one (or more) it would be good to include these in the introduction together with a better description of your objectives.

**Author response:** The reviewer makes a good point here. The hypotheses were not clearly stated in the introduction. We added to the introduction, clarifying objectives and hypothesis for the work. Building from this, changes were made to this section of the manuscript to respond to your question (L313-317 and 334-336)

**Comment** P2506L16-18: what exactly do you mean by this? The catchments have high surface roughness and high spatial variability in processes? How do you know that? Your study did not assess spatial variability within catchments right?

**Author response:** Our study did not assess spatial variability specifically. The vegetation cover in the catchments is variable, though there is generally an organic horizon that protects the soil surface. Because we did not specifically consider it, these points were removed as part of the larger restructuring of the discussion.

**Comment** P2506L27: what are ‘native surfaces’?

**Author response:** Native surfaces in the context of the cited publication are roads with no gravel or pavement – graded roads with exposed but hardpacked dirt. The discussion of roads was mostly removed in favor of focusing on the discussion on hillslope erosion (L397).

**Comment** P2506L8: connectively = connectivity

**Author response:** Yes, corrected in the revised manuscript (L396).

**Comment** P2506L5-10: what has the distance of the road to streams to do with the erosion rates on roads? The distance determines how well sediments originating from roads are connected to streams and to what extent their existence may be reflected in the catchment sediment yield, but it does not affect erosion on the road itself. In fact, the whole discussion here on the importance of roads, does not seem to be relevant for your study and is probably better removed.

**Author response:** Discussion of surface roughness and road production was minimized as this particular study did not focus on geomorphology. Previous work in this area has noted the large discrepancy between sediment production on roads and the paucity of sediment hillslope sediment production; as a counterpoint, the comparison between road and hillslope sediment production rates are presented to show total sediment export is comparable to hillslope sediment production (remaining discussion L395-399)

**Comment** P2507L17: you are referring here to mean annual sediment yield? It may be that this catchment shows highest sediment yield since it also is the smallest catchment. Area specific sediment yield tends to be higher for smaller catchments due to less possibility for deposition losses during transport.

**Author response:** P304 (49 ha) is only slightly smaller in size than B201 (53 ha). Both have long, narrow geometries and yet P304 has a much larger sediment yield. Further discussion was presented by Hunsaker and Neary (2012). Discussion of this paper was rewritten (L372-383).

**Comment** P2507L25: 'core stones' = coarse stones? And, how would the presence of many stones cause high erosion rates? Usually, stone cover is associated with lower soil erosion rates (e.g. Poesen et al., 1994).

**Author response:** Core stones were meant as stones within the soil profile. Discussion of Hunsaker and Neary (2012) was poorly worded; the discussion in that paper does not disagree with your point. The section has been reworked (L374-377) to better present the prior work.

**Comment** P2508L1: where can we see the results of this comparison between sediment yields?

**Author response:** Sediment yields have been added as Table 1 in the revised manuscript.

**Comment** P2508L20-23: preferential erosion refers to the fact that preferentially the finer soil fraction is eroded that is also associated with higher C and N concentrations.

**Author response:** Yes, this was reworded in the revised manuscript to better express the point – transport of fine material with its associated OM from shallow soils resulted in OM-rich material at collection points (L386-389).

**Comment** P2508L8-15: do you have any information on sediment trapping efficiency of the boxes? That is quite crucial for the interpretation of your results.

**Author response:** We did not mean to convey that the sediment basins trapped transported sediment with a high efficiency. Though it would have been beneficial, the trapping efficiency of the sediment basins was not measured in the project due to labor and budget constraints. A note on the basin construction: to meet regulatory restrictions and be as benign as possible, the sediment basins were not engineered to the highest capture efficiency possible. The T003 sediment basin was constructed in the 1940s as a deep, cement-lined basin with a high downstream dam. The dams on the other seven streams, constructed for the KREW project, are less than 1 m high, and the sediment capture basins were constructed in the stream and adjacent banks without removing a lot of sediment. Basins were not expected to capture all fines, particularly in high flow years. However, Hunsaker and Neary (2012) report that silt constitutes as much as 16% of the dry sediment mass in P304. While it may not catch all, fine particles are still settling out in the basins. Trapping efficiency is discussed L404-417.

**Comment** P2510L1-2: please check grammar ('..or thinning due if these..').

**Author response:** We have removed the sentence because of the discussion on forest treatments, which was out of place in the scope of the study (regarding: other comments on this aspect).

**Comment** P2510L3-4: what exactly do you mean to say here?

**Author response:** Erosion mechanisms, and the resulting characteristics of mobilized sediment, are explored in the revised manuscript in L384-391. We have shortened the discussion and focused on sheet or surficial erosion processes, which we think dominate here.

**Comment** P2510L13: which data you found are you referring to?

**Author response:** The texture and specific surface area of the sediment mineral fraction are presented in Table 2. Discussion of the potential sediment sources is limited to L356-361.

**Comment** P2510L12-17: sorry, can't follow your argumentation here. Especially for the lower catchment group, no differences in ER between different transect positions were found. This makes it impossible to identify the source of sediments.

**Author response:** We acknowledge your point. In addition, differences in the ER ratios are partially influenced by the wide range in the soil values. The contribution of upland sediment sources was briefly pointed out in the revised manuscript (L359-360).

**Comment** P2510L27: indeed, finer particles may be transported further, but still in the Bull catchments, the ER compared to depositional sites is below 1, meaning that we have a lower C concentration in sediment than in soils there, so most C stays at the depositional site, and so either a relatively high fraction of source material originates from sources with low C contents, or there are important C losses by oxidation during transport and after deposition in the box.

**Author response:** Alternatively, lower C concentrations are the result of low trapping efficiency in the sediment basins; this and other possible explanations were streamlined L384-417. In addition, depositional locations in Bull Creek were the most variable of all sampled, with a range of 3-16.7% C; one site disproportionately influences the patterns in ER in this case.

**Comment** P2511L11: I don't see how we can have preferential detachment or transport of coarser fractions? In fact, the whole sentence L10-15 does not make much sense. Please revise.

**Author response:** The text was supposed to explain how preferential loss of fine and light fractions from the sediment basins would have appeared as coarser sediment in the sediment basins. This section was revised for clarity, see L384-417.

**Comment** P2511L15: what is a sediment basin approach?

**Author response:** The study approach used sediment settling basins designed for state permitting guidelines but not necessarily high trap efficiency. The concerns here relate to the trap efficiency and the possibility of preferential deposition in the basins, without the confirmation of measurements of suspended and bedload sediment. Section heading was removed as part of rewriting the discussion, but discussion of the trap efficiency is included L404-417.

**Comment** P2511L19: this suggests well known non linearity in the relation between discharge and sediment yield.-

**Author response:** The distinction between sediment yield and water yield responses was reduced, but discussion of each sediment yield in particular is in the first paragraph of the Discussion (L311-333).

**Comment** P2511L26-28: please revise this sentence; it does not seem to make much sense.

**Author response:** Discussion of event-based differences in the sediment composition was left at an acknowledgement that event-based sampling would have been useful for trap efficiency numbers but was not executed due to cost and time constraints (L408-409).

**Comment** P2512L2: the basins characteristics?

**Author response:** Here we were referencing "...the geometry of the sediment basins." A more refined discussion of the basin characteristics and trapping efficiency is included L408-413.

**Comment** P2512L16-18: I don't see how the following statement relates to your discussion on the importance of trapping efficiency: 'where the low C and N capture efficiency in the basins would be attributed to local deposition of particulate C and N within the catchment?'. Please explain or rephrase. Also, what follows (sorption. . .) does not connect to first part of the sentence.

**Author response:** This statement was rewritten as "Another potential loss, dissolved OM, is low in these streams (unpublished data). The consistent C:N ratios support trapping efficiency as the

primary driver of the inverse relationship between C and N concentrations and sediment yield; potentially more material of the same composition was lost in suspension during high flows. Total OM export may thus have been higher than reported”(L413-417.

**Comment** P2512L22: can we see the data to support this statement?

**Author response:** Reference to this has been removed since the study did not focus on event-based sampling.

**Comment** P2513L1-5: Yes, but the sediment yield values for your catchment are very low, so also total C and N exported is low. What may be important is that C and N stability in sediments with higher concentrations is different from stability and burial efficiency of sediments with lower concentrations. See also previous studies (e.g. Wang et al., 2014; Van Hemelryck et al., 2011; VandenBygaart et al., 2015) for more details on stability after deposition.

**Author response:** We agree with the reviewer on the importance of stability and burial efficiency. However, these topics are beyond the scope of this manuscript. In fact, we are currently preparing another manuscript that deals with these exact issues (where the suggested references are referenced and their results discussed). The revised manuscript is better focused on the data presented in this study, with a brief discussion of stabilization mechanisms during the discussion of the study's implications for the C balance (L427-432). Overall, we found the sediment yield is only a small portion of C storage in these ecosystems.

**Comment** P2513L17: remove ‘slopped’

**Author response:** That would have been a good change, but the whole sentence was revised in the new version. Steep slopes are mentioned in the revised manuscript L424.

**Comment** P2514L2-4: on what information is this based? How do you know C replacement and C mineralization potential is high?

**Author response:** Our study did not evaluate these particulars, beyond published productivity for dense mixed-conifer forests. This discussion was hypothetical since we do not have a numerical basis for the potential C replacement. This discussion was removed in the simplified discussion.

**Comment** P2514L14: what are ‘light carbonations materials’ and where are the data to support this statement? We only saw data regarding total C and total N, but not regarding different C fractions.

**Author response:** Misspelled word (carbonaceous). Another part of this work, in preparation for publication, evaluated the relative contributions of aggregation and mineral bonding as OM stabilization mechanisms. We removed the parts discussing C fractions as these will be included in a future publication.

**Comment** P2514L18: If it’s not addressed, leave it out.

**Author response:** Thank you for the feedback. The postulation was removed.

**Comment** P2514L21-25: while this is certainly true, it does not relate to any of your results. You did not discuss or provide data to highlight anything with respect to stability of C during transport or deposition.

**Author response:** A portion of this work to follow evaluated stabilization mechanisms, and it is briefly mentioned in the revised manuscript (L428-430).

**Comment** P2515L8-10: this may be true, but by looking at annual data, rather than events, based on your data you cannot say anything about the expected impacts of changes in rainfall distribution or intensity.

**Author response:** As mentioned in other responses, we did not evaluate event-based sediment or water yield, and the reference to this was limited to a brief call to explore event-based data at these sites (L446-447).

**Comment** P2515L11-12: as above, you did not evaluate the impacts of changes in land cover so you can't make conclusions about that either.

**Author response:** It is true that we did not evaluate the impacts of land cover changes as a part of this portion of the study. However, as it is widely recognized that land cover changes have important implication for sediment mobilization, and this was one of the original motivations of the KREW study. These sediment sources will serve as a baseline for future years. In the revised manuscript, this is only alluded to in L96-97.

**Comment** Table 2: what about the transect in B203 as indicated on Figure 2?

**Author response:** These values in Table 2 are not averages of all soil samples. Due to costs, only a subset of the soil samples were analyzed for texture – these are transects P4, and B8 (indicated on the table). An additional clarification was added to the Table 2 caption: “*Physical and biochemical characterizations of the soil material (air-dry < 2 mm) for a **subset of the sampled soil transects**, including  $pH_{water}$  (1:2 w/v), carbon (C) and nitrogen (N) concentrations, and particle size distribution.*”

*We appreciate the thorough comments from the reviewer. The manuscript is better because of these constructive comments. Thank you!*

*~Erin Stacy (on behalf of all co-authors)*