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The Cryosphere Discussions

Interactive comment on "Carbonaceous material export from Siberian permafrost tracked across the Arctic Shelf using Raman spectroscopy" *by* Robert B. Sparkes et al.

Anonymous Referee #1

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This paper uses Raman spectroscopy to investigate the composition and source of carbonaceous material (CM) exported into the East Siberian Arctic Shelf (ESAS) in comparison with those in terrestrial and coastal erosion samples. Based on peak characteristics using deconvolution techniques, they classified CM into Disordered, Intermediate, Mildly Graphitised and Highly Graphitised groups and observed an enrichment of Intermediate CM in sediments exported from the Indigirka and Kolyma rivers versus Highly Graphitised CM in distal samples. As Raman spectroscopy examines a slightly different pool of carbon materials (and properties) as those previously analyzed by chemothermal oxidation on the ESAS sediments, this paper adds complementary information on the fate of terrestrially derived ancient (graphite like) organic matter in

the Arctic Ocean. As these materials are considered to be highly recalcitrant derived from fossil (rock) carbon and very old, it is important to tease them apart from biologically synthesized, permafrost OC to precisely assess the fate of permafrost carbon in the Arctic. I think the dataset is unique and original that deserves publication to improve our understanding of the Arctic carbon cycling.

That being said, I have a few questions and/or suggestions on the statistics and techniques used in the paper.

1. The main conclusion of the paper is based on the comparison of CM in samples from different locations. While a quite large number of Raman spectra were collected, there is no description of how statistical analysis was carried out in the Methods sectionâĂŤfor instance, how were differences determined for datasets with different number of samples? What statistical analysis was used? How was error propagated for datasets with analytical errors and replicates? How was PCA carried out? Were the data normally distributed? These are key questions regarding the robustness of the conclusion.

Actually, looking at Table 2, I would think that the statistical differences only occur for the Highly Graphitised CM in distal ESAS samples relative to the others and for Mildly Graphitised CM in terrestrial samples relative to the others. The others are mostly similar with a big standard deviation. I recommend using box graphs showing the median, mean and percentiles for each sample group rather than using Figure 3 (which is redundant showing less information than Table 2).

2. Regarding the presentation of data, I prefer to see the distribution of CM drawn in the format of Figure 2 rather than in Figures 4 and 6. While the latter is truly impressive, how reliable are the schemes given the scattered and uneven distribution of sampling locations?

3. The authors mentioned that black carbon particles smaller than submicron size is not detectable by Raman spectroscopy. However, it is also mentioned in the methods

that hours of grinding does not affect CM crystallinity, suggesting no effects on Raman spectroscopy. I am a bit confused here. How big is the pool of black carbon "undetectable" for Raman in the total CM or black carbon budget? Is it possible that, during transport and winnowing, CM may be physically ground to smaller particles to escape the analytical window? How would this affect your data interpretation? In the end, I think it is very important to frankly point out drawbacks of the method as no method is perfect.

4. For the discussion part, I think it makes more sense to introduce PCA analysis first, followed by comparison of group mean values. I also think that some descriptions are repeated and can be shortened to increase the readability.

There are some minor mistakes: Page 13: Line 15: . . .have been caused by. . . Line 29: . . . is that it is preferentially. . . Page 14: Line 7: no offshore trends

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