

Interactive comment on “Physical and chemical evolution of dissolved organic matter across the ablation season on a glacier in the central Tibetan Plateau” by Lin Feng et al.

Anonymous Referee #2

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This manuscript presents some very interesting results on the evolution of organic matter in the snowpack from DKMD glacier on the Tibetan Plateau. It is becoming apparent that organic matter may behave differently from atmospheric aerosol species in the snowpack as it ages, and the authors present a detailed description of some of these changes and the possible mechanisms involved.

Despite these interesting results, I find that the manuscript as written is very confusing and the authors omit key descriptions of how and where the snow was sampled. This leaves the reader without a context in which to frame the results that are presented. Specifically, the authors have sampled snow from a large number of sites

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above and below the equilibrium line (5600 m) on the surface of DKMD glacier, yet refer to changes in the snowpack as it "melts" over the summer season. Surely, melting, and the generation of meltwater and subsequent snowpack flushing and mobilization of DOM within the snowpack, will differ greatly between snow found below the ELA (in the ablation zone) and snow found above the ELA (in the accumulation zone). I would even question the existence of a firn layer within the snowpack in the lower ablation zone, yet the authors seem group all of their samples together and discuss them as being equal in structural composition, and by extension, the generation of meltwater. Further, I question the use of the term "melting" throughout the manuscript. The first line of the abstract mentions snowpack "metamorphism", which implies processes that occur as the snowpack ages, and does not necessarily include the production of meltwater. The use of the term "melting" throughout the remainder of the manuscript implies the transformation of the snowpack from the solid to liquid phase, or at the very least, the generation of some meltwater which would be expected to mobilize a significant fraction of the DOM due to flushing. I believe that "metamorphism" would be the more correct term to use here, at least in the accumulation zone, unless melting truly is happening, in which case the authors would need to specify this and integrate it into their interpretation. Additionally, do you have a multi-year snowpack in the accumulation zone? It would be interesting to hear about the patterns that you see over several years.

Specific comments are as follows:

Line 7: what are the "stages of snowmelt"?

Line 51-52: not sure what "more efficient to be released" means.

Line 52: "Polar Regions" doesn't need to be capitalized.

Lines 62-64: unclear what the difference between ablation and melting is here.

Line 74: again, is this really snowmelt or metamorphism, firnification?

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Line 170: "widely used", yet you only provide a single reference. Can you list a few more?

Line 256: why "gradually increased for coarse firn"? This suggests that [DOC] increased with depth in the coarse firn layer. Is this what you mean?

Line 272: "component"?

Line 275: again, "melting stage"? Is it melting?

Line 282: you can delete "as described in the Supporting Information section".

Line 285: "important microbial origin". I think that what you mean is that C1&C2 have been associated with microbially-produced DOM, and it constitutes a large % of the total DOM fluorescence.

Lines 325-327: Is this true of all layers, or an integrated average for the snowpack? "snowmelt could have lost DOC" is misleading.

Line 328: I remain confused by the word "melted". Are you claiming that in May, all that you were sampling was unaltered snow, and that over the course of an ablation season this snow was metamorphosed into firn? I don't think that this is what you mean. . .

Line 334: are you suggesting that the proglacial stream has received the nutrients flushed from the snowpack during summer? Surely this would be from the snow in the ablation zone only?

Line 366: all studies using fluorescence to characterize glacier melt have found that protein-like fluorescence dominates supraglacial DOM.

Line 369: why use the Lake Taihu comparison? Why would it be relevant to glacier snow?

Line 422-424 loss of DOM from the snowpack? I don't get it. Where's the DOM going? Is it being flushed from the snowpack, volatilized, . . .? Likewise "enrichment" in lower

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levels? Is DOM being produced?

Line 431 Lignins increasing with depth. Isn't this just an accumulation of recalcitrant DOM with downward flushing through the snowpack?

Line 436-438 This last sentence is very general to the point of being inconsequential.

Table 1: you need to provide references for your fluorescence peak assignments

Fig 1: the lines and labels for the mountain ranges are distracting and don't add any useful information

Fig 3: was fresh snow sampled in the summer? Did you have any summer snowfall?

Fig 3: the x-axis for abs in figs 2-3 don't match which is distracting when comparing the spectra

Fig 7 I don't think that this figure adds anything to the manuscript. What are we supposed to get from it, that there's overlap?

Generally- I think that C1 and C4 are probably the same. Have you run a 3 component PARAFAC model?

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