

Interactive comment on “Multi-year particle fluxes in Kongsfjorden, Svalbard” by Alessandra D’Angelo et al.

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We’d like to thank you for the helpful comments. Following, the answers for each point.
Answers to comments at “Introduction” section.

The Introduction seems rather small lacking references to similar works done elsewhere in the Arctic.

We agree with this comment, thus we added some references describing the influence on particles settling of tidal glaciers surrounding the fjord.

I think this is too detailed for the Introduction for it belongs to the Methods section, especially the second underlined paragraph.

C1

We reduced the methodological description of the mooring and left what strictly need to explain the aims of the investigation.

Also, there seems to be something wrong with the sentence "geochemical toolbox provided of...". Please check.

We removed the sentence.

I guess that six years is certainly a long time-series but perhaps not enough to say much about global change patterns.

We smoothed the assertion.

Answers to comments at “Materials and methods” section.

- 3.2 Trap sample treatment and analytical methods: the sentence "The total weight of the trapped sediment was converted to flux according to each sample duration and to the trap collection area" was moved above, as suggested by the reviewer.

- 3.3 Principal component analysis (PCA): a sentence was added to explain how the dataset was transformed (Standardized to a mean of 0 and standard deviation of 1).

Answers to comments at “Results” section.

- 4.5 Oceanography: I can see the negative values in 2012 but not in 2014. May you confirm please?

We used “negative peak” to mean two minimum values, not necessary temperature values below zero. This caused misunderstanding. To better clarify, we modified the word “negative” in “cold”.

- 4.8 PCA: One can see the loadings in the Table 2 and wind speed is opposite to all the other. However, from this sentence it is unclear which opposes to which. I suggest rephrasing.

We rephrased the sentence as follows: “The main coefficient for PC2 is the wind speed,

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whereas air and water temperatures, radiation, precipitation, (weakly) salinity and wind direction are in opposite loading."

Answers to comments at "Discussion" section.

- 5.1 Seasonal variability of particle fluxes: I suppose that wind direction may be a bit misleading in this analysis because extremely different values may reflect very similar directions when the wind comes from the north.

Honestly, we did not understand the comment, but it does not seem critical for the overall comprehension of the text.

- 5.2 Nature of collected particles: Why intriguingly? I can see that Burgeois et al. (2016) found higher $\delta^{13}\text{C}$ values in sediments of their inner station. Also, Kumar et al. (2016) found basically the same along the fjord axis, with $\delta^{13}\text{C}$ values increasing towards the inner station. Both authors found also higher C:N ratios in the inner fjord. So, I guess there is some consistency regarding the sediments: those near the glacier front are apparently more enriched in ^{13}C and impoverished in particulate nitrogen than those closer to the ocean. I can also see that values presented by Calleja et al. (2017) for suspended matter do not show any clear trend from the inner to the outer fjord regarding the $\delta^{13}\text{C}$ but they show that C:N ratios are lowest for the inner station which matches the results of the previous authors. So, the fact that you have a negative relationship between C:N and $\delta^{13}\text{C}$ (Figure 7) seems to contradict the results presented by the previous authors. I wonder if this results from the fact that all your measurements come from the same point in the fjord and because of that all your variability is "temporal" as you discuss a bit further down.

The term "intriguingly" was used as in the Kongsfjorden, Burgeois et al. (2016) and Kumar et al. (2016) found uncommon light $\delta^{13}\text{C}$ values for marine organic carbon and uncommon heavy $\delta^{13}\text{C}$ values for terrestrial organic carbon. This is in contrast with almost all previous studies in the world. Actually, different authors found different ranges of $\delta^{13}\text{C}$ values in Kongsfjorden, which makes this parameter very problem-

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atic to use in a mixing model to infer the origin of organic matter. Furthermore, many different sources of OM (marine, terrestrial by glaciers, surface runoff of permafrost, coal, coastal macroalgae, kerogene, etc.) were suggested in previous works, each one characterized by different $\delta^{13}\text{C}$ or C/N values. Hence, we decided to just describe the overall temporal trend, minimizing the interpretation of its origin. Anyway, we deleted the term "intriguingly".

You should explain how the ranges of values defining the various end-members were defined.

The areas for each end-member were defined based on reference values and by the distribution pattern of our data (quasi-triangular dispersion). We are aware this is a rather simplified approach. However, we did not use the end-member composition for any mixing model, our aim was just to make general inferences on the nature of collected particles.

The values I see in Figure 7 for OC associated with these "glacier" end-members seem rather low...why do you say values are relatively high? Also, in the PCA OC is opposed to variables associated with the glacier discharges (along PC1). The referee made this comment to the sentence: "The third end-member remains, though, elusive. Low opal contents do not support the hypothesis of in-situ diatom production while the relatively high OC content would suggest glacier outflow quantitatively enriched in fossil/subfossil bioavailable carbon...". This was exclusively referred to the third end-member (silicoclastic rich), which has 3 typical samples (the yellow ones in Fig. 5 – new version) with a relatively high OC content (in a range of 0.75-1.5%).

- 5.5 Annual fluxes and possible changes: "Annual total mass and main component fluxes are rather constant over time (Fig. 11), with the exception of 2013". The referee comment was: "This is not what Figure 11 suggests, with major inter-annual differences. 2015 seems also quite different...".

We agree with this comment, as the sentence can be misunderstood. Thus, we

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rephrased it.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-174/bg-2018-174-AC2-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-174>, 2018.