Second REVIEW of the manuscript:

"Patterns of suspended particulate matter across the continental margin in the Canadian Beaufort Sea", Jens K. Ehn, Rick A. Reynolds, Dariusz Stramski, David Doxaran, and Marcel Babin

General comments

The reviewed manuscript presents major revisions to the original version and has taken into account many comments proposed by the reviewers. In particular, the authors focused on the main findings and eliminated as they state, "unnecessary descriptions".
The introduction of new data on the oxygen isotopic composition is a valuable addition to their dataset. It clarifies some flaws from the original manuscript, especially on the origin and composition of the particulate matter by distinguishing between characteristics of particles from riverine input and open water particles, especially those contained in sea ice meltwater.
I still regret that the authors did not attempt a multivariate analysis of their data (e.g. PCA). However, the reviewed manuscript focuses now clearly on some major findings, which does not necessarily call for a more extensive data treatment. But in this case, the authors should limit their discussion to these major findings and eliminate sections that would only contribute to the scientific content if the data were treated as I had proposed. Otherwise, these data and discussions are too flaw and superfluous, since the core data do illustrate the main findings of this manuscript.
I will explain what I would consider to be these main findings in the following specific comments.

Specific comments

Introduction: The revised introduction gives a clear overview of the different aspects of particle dynamics in this very complex environment with a major riverine input and a continental shelf exposed to a dynamic pattern of currents, which strongly depend on wind forcing but also on the variable annual cycle of ice coverage. The revised text presents the scientific context in a concise and well-focused way, which facilitates the lecture and understanding of the rest of the manuscript.

Paragraph 3: This paragraph is clearly structured. It starts with the MALINA data on the hydrology (3.1.) and then on the particle distribution (3.2.), and it presents first evidence on some main findings, i.e., the driving forces (wind, ice coverage, meltwater) of a dynamic environment and the broad range of particle size, composition and concentration. This logically leads to the relationship between particles and beam attenuation (3.3.) and finally to a comprehensive description of suspended particulate matter (SPM) distribution in space and time (3.4.). The distinction of particle characteristics between river water and ice melt water, although not much surprising, is one of the main findings of this paper.

Paragraph 3.3.: The new text focuses much better on the relationship between SPM, POC and beam attenuation, by avoiding confuse and detailed descriptions of the regression analysis, which is now in the supplementary material, where it appears in a clear form.

I have one major concern about the mathematics of the relationship. On page 10, line 28, the authors present the “counterparts” of equations 2) and 3). Unless I misinterpreted the expression “counterpart”, I did not come up with the same equations. I had not seen this in my first review, but since that relationship is another major outcome of this paper, the “counterparts” need to be clarified, and I hope it does not concern equations 2) and 3), on which all subsequent calculations are based.

Paragraph 3.4.: This paragraph together with 3.6. contains the main findings of this article, the temporal and spatial variation of the particle distribution and dynamics, which depend on 1) river discharge, 2) ice coverage and meltwater and 3) wind forcing.

Page 12, lines 22-25: The argument that resuspension was insufficient to increase clear water beam attenuation values of wedges that reach far onto the shelf (lines 100 and 600 in 2009) contradicts their argument in paragraph 3.6.2., line 34, where resuspension of shelf sediments could explain the turbid surface waters. Errors of this type could be avoided in a multivariate analysis of the data.

I suggest integrating paragraph 3.4.1. into 3.4. and to remove paragraph 3.4.2. As I had already mentioned in my first review, this section was the least convincing. Although the authors removed the most critical part, the paragraph as a whole does not really contribute to the main findings, let alone the title of the manuscript, which focuses on the continental margin. I understand well their argument to keep the figure (former fig. 11, now fig. 9), but suggest that they only present a couple of contrasting SPM profiles to illustrate the shelf to basin differences and discuss this within the general context of nepheloid layers at the end of section 3.4. No need to go into details about thickness of these layers and particle concentrations and transport.

Paragraphs 3.5. and further: I don’t think that it is necessary to separately discuss the data on environmental forcing and oceanographic conditions. The data presented in these sections do not illustrate a specific finding *per se*, but help to interpret and explain the preceding data, which is done in section 3.6. That said, Fig. 10 can still be maintained and used in section 3.6., Fig. 11 at best be presented as supplementary material, and Fig. 12 remains a very complicated one despite the simplifications done by the authors. Not surprising that there were confusions in interpreting the data. On page 16, line 6, they talk about southwesterly winds at the end of July, which become southeasterly ones on the same page, line 18.The importance of this figure is to show the periods of upwelling and downwelling favourable wind conditions. Why not put the Figs. 12a and b to the supplementary material and make a graph (histogram type), which shows on two time axes the periods of easterly and the periods of westerly winds and on the y-axis the average wind speed? This would be sufficient to illustrate the discussion in section 3.6. together with references from the literature (Carmack, Dmitrenko, Macdonald, Forest, Mol).

Paragraphs 3.6.: As I said for 3.4., the main findings are presented in 3.6. and 3.4.

Page 18, lines 28 and further: I do not completely agree with the interpretations in this paragraph. As I said before, the resuspension hypothesis contradicts the text on page 12 and the temperature and salinity fields (page 18, lines 32-33) are modestly different in 2008 and 2009. 2004 is quite different with salinity values >30 and temperatures not exceeding 2 degrees Celcius, while they were >5 degrees and salinity <28.5 in 2008 and 2009. Again, a multivariate analysis may have shed a clear light on these interpretations. I would therefore suggest to only discuss the influence of light and SPM on primary productivity for the Amundsen Gulf and line 100.

Conclusion: By removing/modifying Fig. 12, the paragraph about the mooring data (lines 28 and further) could be more general and highlight the second part related to Fig. 8 (page 20, line 1 and further) and including Fig. 2 where the upwelling onto the shelf is also illustrated by the east-west salinity gradients related to easterly wind conditions.

Technical corrections

- Page 8, line 3: The percentage here is rather confusing since meteoric water fraction given in percent is discussed. I would give a salinity value (e.g.: >29 PSU).
- Page 9, lines 15,16: In Fig. 5a values are given as percentage. It is better to match the text with the figure.
- Page 18, line 16: Add “Fig.” to “8e”.
- Page 18, line 25: Fig. 8f not 7f.
- Page 19, line 31: cross-shelf (see also page 12, 14, 17 line 1, 30, 25: cross-section)

References

References Forest et al. 2010 and Spall et al. 2014 are not cited in the text.

Figures

The dates in Fig. 10 are rather confusing.