RESPONSES to the 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, Bruno Lansard, and Marcel Babin

We greatly appreciate the constructive comments from both reviewers. Here we provide our detailed point-by-point responses and any description of action taken in regards to the comments by Referee #1. The Referees' comments are shown in regular font; our responses follow each comment in blue font.

Response to Referee #1

General comments

The present manuscript reports on the distribution and patterns of suspended particulate matter (SPM) and associated optical properties in the Canadian Beaufort Sea. Specifically, the authors demonstrate the correlation between the particulate beam attenuation and the dry mass concentration of SPM and use it to extend the SPM data to stations where only beam attenuation measurements were done. The obtained SPM distribution is discussed in relationship with environmental forcing such as wind, river discharge and sea ice coverage. The authors show that these forcings result in different circulation modes, upwelling onto the shelf, downwelling return flow across the shelf and vertical mixing due to strong wind conditions.

The manuscript is clearly written, the methods well explained and the graphs mostly illustrate the data accordingly.
My major concern about this manuscript relates to its structure. The authors present in a first step the optical (beam attenuation) and SPM data obtained during MALINA 2009 cruise and use these data to develop the SPM algorithm. The algorithm is then applied to beam attenuation data obtained from 4 other cruises in the Canadian Beaufort Sea, in order to extend the SPM data set. The second step consists in presenting almost a new manuscript with a first description of environmental parameters and then of different patterns of SPM distribution.

Although the presented structure is clear, the different pieces (paragraphs) are rather isolated and their contribution to the scientific question remains unclear.
I would therefore propose a different approach, which consists in keeping the first part with the MALINA data and use the data of the second part to do a statistical analysis relating the SPM patterns to the different environmental scenarios. Not only would the findings be more robust by being "statistically" supported compared to the only descriptive presentation in the present manuscript, but also the manuscript as a whole would appear more coherent with respect to SPM patterns related to environmental forcing.

I will give more detailed arguments in the specific comments in order to better explain my proposition.

REPLY: We would like to thank Referee #1 for the insightful comments that spurred us to take a critical look at the structure of our manuscript. We agree mostly with the revisions that have been suggested by Referee #1. We agree that the link between the SPM algorithm development using MALINA data and the second part that involved comparisons to forcing conditions required clarifications. In the revised manuscript, we have put much effort into focusing the paper by rearranging its structure and removing unnecessary descriptions. However, considering unavoidable limitations in the available data sets, the possibility of conducting a statistical analysis of the kind suggested by Referee #1 appeared to us highly problematic. Instead of attempting such statistical analysis, we have followed the advice of Referee #2 and increased a focus of the manuscript on particle characteristics associated with freshwater inputs. This involved including a new data set of water oxygen isotopic composition. See also our reply to the comment on Paragraph 3.3.

Regarding the lack of exploitation of the effects of particle size and composition on the SPM vs. cp relationship, we point out that we have to rely on one relationship regardless of particle size and composition because we apply the relationship to the cp data measured during different field experiments when no ancillary data on particle size and composition were available. In contrast to cp, which is routinely collected in the field as a part of CTD casts, the particle size and composition data are rarely collected except during focused/dedicated field experiments. Thus, in this paper we use the particle size and composition data gathered on MALINA primarily to indicate that our relationships between cp, SPM, and POC are robust over a broad range of variability in the particle assemblage.

Given the quite extensive scope of revisions that we made with regard to restructuring the mansucript and making changes in the content of various sections, it would be impractical to describe each and every change related to the restructuring in this response. We believe, however, that these main changes are easily identifiable in the revised manuscript.

Specific comments

Introduction: The scientific context is well presented. Particle origin and transport ways, as well as the different factors to which beam attenuation is sensitive (concentration, size and composition of particles) are introduced, and one would expect that these factors would be discussed accordingly within the manuscript. Even if at the end of the introduction, the authors solely talk about particles, the reader would suppose that they mean organic and mineral but also different sizes of particles. Also, clearly, the authors admit temporal variations of particle characteristics but intend to relate the distribution patterns to oceanographic conditions (last sentence of the introduction).
This is exactly what could be answered by my above proposition: A robust, statistical relationship between environmental parameters and particle distribution takes into account the different variabilities and overcomes at least at a certain probability level such uncertainties.

REPLY: To improve the description of the effects of different factors on beam attenuation we included a more detailed description in paragraph 3 of the Introduction section. Earlier this text was part of the first paragraph of the original section 3.1.2, now 3.2.2, which has now been shortened. As mentioned above, with regard to statistical analysis, we have followed the advice of Referee #2 and increased a focus of the manuscript on particle characteristics associated with freshwater inputs. We believe this is an important aspect which improved the manuscript. See also our reply to the comment on Paragraph 3.3.

Paragraph 3.1.2.: The paragraph could be removed and the beam attenuation results presented together with the data from the other cruises.

The fluorescence is certainly an important parameter for the particle characteristics, but the authors do not discuss these data (paragraph 3.4.5.) very extensively. E.g. they could use them to see how autochthonous production of particles and the related difference in distribution dynamics influences the general particle distribution pattern. Also, there is no discussion on its influence on the beam attenuation data, although the authors clearly state it (line 30, page 7).

REPLY: We have considerably rearranged this part of the text. The original sections 3.1.2 and 3.1.3 have been combined into a new section 3.2. The new sections 3.1 and 3.2 still focus on MALINA observations to show the ranges in water and particle characteristics, which underlie the development of statistical relationships presented in the section 3.3. Measurements of chl-a fluorescence are used throughout the revised manuscript (e.g., sections 3.2, 3.4) as an indicator of particle origin and characteristics.

Paragraph 3.1.3.: As said before, several characteristics of particles that influence the beam attenuation are presented, but this aspect is not really included in later discussions.
Some interesting findings are presented about mineral and organic dominated particle composition, but none of this is being considered when it comes to a general discussion on the particle distribution patterns, unless I have overseen this point.

Routine beam attenuation measurements during the Arctic expeditions used in our analyses have not been accompanied with specialized analysis aimed at determining the particle composition and PSD characteristics (with the exception of a subset of MALINA data set). Because of this lack, we feel that speculations regarding the potential effects of particle assemblage properties (such as composition and PSD) on the discussion of general SPM patterns is unwarranted in the context of these additional cruises. The subset of MALINA data is used to indicate that our developed relationships are applicable over a wide range of variability in the particle assemblage.

The same accounts for the particle volume distribution and the particle size distribution (PSD). The data of the former are not so much of a surprise to me and I do not think that they contribute substantially to the science of this manuscript. However, the data about PSD deserve more attention than given by the authors. The description (lines 1-6, page 9) is rather confusing and a table or a graph would shed much more light on them. Also, the authors could use these data to discuss points like optical properties of different size spectra, is the chosen wavelength (660 nm) appropriate for all types of spectra etc. Some of the co-authors (Reynolds, Stramski) have signed a very nice article in L&O, 61, 2016, which I would consider as a model case of thorough discussion related to the same subject. I could imagine that this opens many possibilities of parameters to be used for statistical treatment.

REPLY: We have included a reference to Reynolds et al. (2016) and indicated that this study includes a detailed discussion of the PSD data collected in Arctic waters, including results from MALINA. In the revisions we have focused on improving the presentation of the relationship between the particle composition and size characteristics and freshwater composition. In Fig. 5 (formerly Fig. 4) we have added two graphs illustrating how POC/SPM and PSD shape are related to meteoric water fractions present in surface water samples. In our view, these revisions address the points made above and focus the discussion on differentiating particle characteristics between sources (fluvial, sea ice melt, and pelagic).

Paragraph 3.2.: The relationships and different regressions are presented in much detail. While some of them are not necessary, others add more confusion than clarity. E.g. what do the two measurements, RMSE and MNB, add to the regression coefficient? The latter is rather well known, but the former may need some explanation in order to be evaluated by the reader, e.g. reference values for the two (0, 1) would permit an evaluation of the presented results.

The explanation of the regressions of the c_p (660) and c_p (676) vs. SPM data (lines 18-24, page 10) are confusing. It is not clear which points were used for the two analyses, red points for red regression? but red stands also for mineral-dominated, i.e. are there only mineral dominated data for 676 nm measurements? In this case, it is maybe worth to explore if the measurements for the two wavelengths can be merged, which would at the same time better justify the argument that equation 2) is used for high SPM values (lines 21,22, page 10).

Lines 20-23, page 9: If differences in r^2 are not significant, there should be a better argument than just "appears to best match" for choosing a linear power function fit, unless the RMSE and MNB measures are better explained.
In the same sense, what conclusion can be drawn from the fact that a non-linear power function fit is best for SPM data and a linear regression to log-transformed data best for POC data? This brings me to a general question about establishing relationships between optical and biological measurements. Is it possible to attribute some functional meaning to a given class of data fits? For example, if the fit is a power function, is this related to growth rates of phytoplankton and if it is a linear fit, is it related to cell density etc.?

REPLY: In response to these comment and to clarify the issues related to regression analysis, we have moved the detailed description of the regression fits to the Supplementary Materials, where we also provide the RMSE and MNB equations. In the revised manuscript only the two final chosen regressions are shown, which are then applied to beam attenuation data from the three Arctic expeditions. We have also made it clear that all data points, regardless of their "colour", are used in the final regression fits. For more details about the regression analysis, the readers are referred to the Supplementary Materials. This additional material also includes results for different types of regression fits. With the regards to functional meaning, we point out that the various models (linear, power) were not statistically different, and we chose the power function as this has been the most common approach used in the past. These are simply empirical best-fits to the relationship.

Paragraphs 3.3. and further: It appears as if the SPM data from the other cruises are used to discuss the patterns from MALINA by choosing the contrasting or similar situations. Examples:
1) Wedges of clear water found over the shelf due to near meltwater from extensive ice

coverage, as opposed to low ice coverage in other cruises where clear water is absent (paragraph 3.4.1.).
2) High near bottom SPM concentration during MALINA related to downwelling return flow as opposed to 2008 upwelling situation with high river plume extension and low bottom SPM concentration (paragraph 3.4.2.).
3) Similar SPM patterns between MALINA and CASES 2004, but higher SPM concentration during CASES due to timing of the year (recent break up of land fast ice cover) (paragraph 3.4.3.).
These examples together with the points discussed in paragraphs 3.4.4. (high SPM concentrations in a well-mixed water column due to upwelling) and 3.4.5 (primary production depends on sea ice coverage (light availability), nutrient availability and river plume extension related to wind conditions) are all criteria which could be generalized and chosen as parameters for a statistical analysis to explore relationships between the main environmental factors sea ice coverage, river discharge and wind and the typical patterns of SPM distribution quantified by the dry mass concentration of SPM across the shelf and into the Canada Basin.
Since the descriptions given in these paragraphs are rather clear, I could well imagine that a statistical analysis will yield significant results, which is in my view the ideal way to apply statistical analyses to environmental data: First, you inspect the data in a rather subjective manner, then you are able to apply the appropriate statistical analysis to obtain an objective result with a given amount of error.

REPLY: We agree that a statistical analysis would be ideal, however, it is unclear to us how to implement a statistical analysis with the actual limited availability of field data in this particular study to make this analysis quantitatively meaningful. Although the transect lines we have chosen are probably the most sampled in the Canadian Beaufort Sea, this is still a limited number of data. We note that past studies, including recent studies using extensive mooring timeseries such as Forest et al. (2015) and Jackson et al. (2015), take a similar approach to our study and use inference to understand processes on the shelf. A numerical model sensitivity analysis of different factors affecting SPM distributions would, in our opinion, provide probably the best way forward to deduce statistical relationships. This would, however, constitute a separate study on its own and is beyond the scope of our study. Our result could be useful for evaluating such model and we have added this statement at the end of Conclusions section in the revised manuscript.

Finally, the discussion in paragraph 3.4.6. was the least convincing. Examples: 1) line 6, page 18: Fig. 12 does not show the cast-to-cast variability.
2) line 13, page 18: it is rather difficult to define the bottom layer thickness from the presented profiles.
3) lines 20-26: the authors may be able to see flow patterns of INLs, but the reader may as well see other patterns.
Again, a statistical analyses would (or not) remove any doubt about the proposed explanations of the different patterns of nepheloid layers.

REPLY: We have made significant modifications of this section. We no longer mention cast-to-cast variability. The text in lines 20-26 has been deleted. We have, however, kept the figure (originally Fig. 11, now Fig. 9) and a brief discussion of this figure, as we want to show one graph with individual cast (other cp data are shown only as contour plots) and illustrate the SPM concentrations on the shelf within a context of what is observed in offshore Canada Basin waters.

Figures: By consequence of my proposition, the figures 6, 7, 8, 9, 12 and maybe 11 would need to be modified or even removed and figure 10 remains the key figure.

Technical corrections

- Lines 1, 4, page 2: Mass units are generally given in g, i.e. Tg instead of Mt
REPLY: We have changed to Tg although we note that the source reference Macdonald et al. (1998) uses the units of Mt.

- Lines 5-6, page 2: If 50% are deposited in the delta and 40% on the shelf then the fraction across the shelf break is not poorly known, but should most likely be 10%
REPLY: The sentence has been rewritten as follows: "Macdonald et al (1998) recognize that sedimentation rates on the shelf are poorly known, but estimate that about 40% of the sediment input to the shelf is deposited while about 13 % is transported across the shelfbreak either in surface river plumes, near the bottom in nepheloid layers, or by ice rafting." The cited paper includes large ranges in these values.

- Line 30, page 2: ...part of the MALINA project...
REPLY: Added "the".

- Line 26, page 4: The blank value seems a bit high to me. Is this common for the used instrument?
REPLY: We have not been able to ascertain the typical blank values for this instrument. Note that our POC measurements were made on the same filters as were used for SPM. Thus, the blank filter preparation also followed the SPM protocol steps such as weighting, rinsing with milli-Q, etc. This may have contributed to higher blank values than what might be otherwise expected. Nevertheless, we filtered sufficient volumes of sample water such that the carbon signal on the filter was significantly higher than the blank values.

- Line 6, page 7: Instead of the questioned Matsuoka reference, I would suggest: McDonald et al., 1989, JGR and/or Carmack et al., 1989, JGR, which are the refs. mentioned in Matsuoka.
REPLY: Indeed, we did have the reference to Carmack et al. 1989 but unfortunately misspelled the citation reference in LaTex (hence the **?**). We have kept the reference of Matsuoka et al because it uses the same dataset as our study. This is now corrected.

- Line 8, page 8: ...Only at station 394....
REPLY: Corrected.

- Line 33, page 10: which transect is meant?
REPLY: Changed to "all the ship-based transects (Fig. 1)"

- Line 14, page 14: ...which corresponds to the Mackenzie....
REPLY: Corrected.

- Line 17, page 14: ...to the northerly and rather weak....
REPLY: Changed to: “to the northerly and, then later, weak winds”.

- Line 6, page 20: ...at a depth corresponding to an.....
REPLY: Corrected.

- Line 10, page 20: ...(Fig. 8d)....
REPLY: Corrected.

- Line 15, page 23: The reference Guay et al. is not cited in the manuscript - Line 25, page 24: Timmermans et al. should appear after Stroeve et al.
REPLY: Thank you. We removed Guay et al. and moved Timmermans et al.