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 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.

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.

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).

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.
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.

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.?

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.
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.

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
- 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%
- Line 30, page 2: …part of the MALINA project…
- Line 26, page 4: The blank value seems a bit high to me. Is this common for the used instrument?
- 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.
- Line 8, page 8: …Only at station 394….
- Line 33, page 10: which transect is meant?
- Line 14, page 14: …which corresponds to the Mackenzie….
- Line 17, page 14: …to the northerly and rather weak….
- Line 6, page 20: …at a depth corresponding to an…..
- Line 10, page 20: …(Fig. 8d)….
- 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.