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Ocean Science Discussions

Interactive comment on "Suspended particles in the Canada Basin from optical and bottle data, 2003–2008" *by* J. M. Jackson et al.

Anonymous Referee #2

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Jackson et al. use a combination of bottle POC and TSS data and transmissometer and fluorescence data from two years (cruises) of studies in the Canadian Basin to derive relationships between POC and total suspended solids and the instrumental data. Where these relationships are found to be statistically robust, they are applied to sensor data available from other years (2003-2008) with the attempt to extrapolate to these years and discuss interannual variability. The overall aim of the study is to provide a baseline dataset of suspended POC concentrations in a system for which significant changes are expected with the warming of the Arctic and adjacent continents.

This is a very much descriptive work. Given the concept that sensor data (transmissometer, fluorometer) can be used to extrapolate from scarce bottle data in particular in the open ocean (e.g. work by Bishop, Gardner and others, cited by Jackson et al.,

2010), this study gives an excellent example of all the difficulties which one has to expect when applying this concept to the oligotrophic Arctic seas, in particularly those regions in proximity to the shelf slopes and/or large rivers. From 18 datasets tested (4.5 regions*2 years*2 methods), the authors identify 3 for which they are confident of the relationship between measured bottle POC and transmissometer. It is for these that they compute POC from transmissometer data from other years. It is in particular these computed POC data on which they build their discussion of interannual variability in the region.

If I understand their rational for accepting a certain relation (or not accepting others) correctly, one strong argument beyond r^2 is that $r^2$and RMS in the two years 2006 and 2007 are similar. Given this, I do not understand why the authors use the relationship for ESCB, as this is from 2007 only. I also miss a test, whether different regional boxes would yield different results when extrapolating to other years.

Given the 18 datasets at hand, the findings of the authors give good evidence that no robust relationships could be found for TSS (9 datasets) and to my understanding for only 2 out of 9 datasets robust relationships could be found for POC. At large, I would conclude, that hardly in any case robust relationship have been found at all (or are to be expected?) in this region. Hence, I would be very careful in interpreting the extrapolated data, for example in relation to predicted changes in that system (e.g. conclusions, last paragraph).

So, have the authors achieved their own goals? Are the data sufficient to provide a baseline in view of Arctic changes to come? Since, they hardly can extend their bottle data using the sensor data, I don't think they did.

I suggest to re-organize the paper, and to focus on the major result: the not-existence of looked-for relationships. I think the authors have good arguments, why this is the case. (It might be disappointing, I can imagine, but I don't think that the data allow for more or different.) Also, this result is worth publishing and as such the paper is

adequate for the Ocean Sciences journal.

Minor comments:

2.1, page 1020: Niskin and all sensors on same device, right? Please clarify.

2.2. page 1022: Spigot issue (Gardner, 1977, Limnol. Oceanogr.): How did you sample for the particles, from the spigot? This could explain to some extend the week correlations observed.

Page 1022, 21-22: give also as relative standard deviation

M+M in general: very detailed, well done.

3., 1024, 1-2: refer to Fig. xx?

3.1, when you discuss the depth of the fluorescence maxima, could you please mention somewhere (perhaps already in M+M) where nutrient data have been published

3.1., 1024, 10: why 'however'

3.4, 1027, 3: does not necessarily indicate higher primary production

5.2, 1034, 8: should be 150W, right?

1034, 10: refer to table 4

6, 1036, 25: replace 'the this' by 'such a'

Table 3: I think your units (slope and intercept) are wrong, at least if they are as given in the plot. Should be, e.g. for POC, m^{-1} $(\mu g C kg^{-1})^{-1}$ and m^{-1}, right?

Figs 2-7. The z-scale is non-linear, but that is not obvious. This needs to be clearly indicated and explained (mapping equation) in the caption.

Interactive comment on Ocean Sci. Discuss., 7, 1017, 2010.