L&O 07-399 -- Reviewer #1

Review of the manuscript "*Robotic in-situ and satellite based observations of pigment and particle distributions in the Western North Atlantic*" submitted to Limnology and Oceanography by Boss et al.

This study presents the results of ~ 2.5 year acquisition of biogeochemical (Chla, backscattering) and physical (T, S) variables using the new technologies combining ARGO-like floats together with bio-optical sensors. This paper has two merits :

(1) it definitively illustrates the expected power of such technologies for durable autonomous acquisition of physical / biogeochemical data with an unprecedented temporal / vertical resolution.

(2) it presents very interesting observations in the North Atlantic. Although these observations are sometimes not necessarily conclusively interpreted (e.g. eddy enhanced backscattering), this is a minor point I believe. The merit of such new types of observations is that they allow discoveries at new scales, until now not well resolved by classical sampling strategies : this should be the purpose of subsequent field studies to better characterize the physical / biogeochemical processes at the origin of these observed "anomalies".

This paper is suitable for publication in L &O provided that significant revision in response to my comments below are taken into consideration.

I have a general concern : by using remotely detected biogeochemical products (b_b and Chla) as variable to calibrate the float's sensors, it implies that float's measurements cannot be used as sea truths of remote sensing measurements. Furthermore it seems to me much more evident to calibrate the sensors in absolute units before the deployment and then to develop techniques to compensate for any potential drift over the three years. I believe this is potentially a better alternative to the approach used by the authors. I suppose that the authors have thought about this issue as well; at some point this should be clearly stated and discussed in the ms.

Introduction:
Not very ambitious regarding the new data that you provide and the interpretation that is done

Paragraph 1, sentence 2. Very general and long. Should be cut into two sentences. NPP, for primary production. Why not simply PP?

Paragraph 2. There is a list of potential problems with ocean colour to access stocks and/or rates. The last sentence of this paragraph looks so much like a "standard and generic sentence" that it does not help very much to focus the message / topics. Especially when the "global elemental cycling" has been already quoted in the first paragraph.

Paragraph 3. too early. I would rather describe ARGO floats (paragraph 4) first, then describe what has been achieved in the past with optics (paragraph 5, excluding last sentence), and finish with the present paragraph 3 (or reworked) which presents the aim of the paper.

Materials and methods

I feel the MM section should be reworked. At present there are some topics (significance of the sensor measurements, calibration) that are found in various places of this section. They should be regrouped in more dedicated sections for (1) the LSS sensor (hardware, what is measured, how to calibrate…) and (2) the fluorescence sensor (idem). Extraction of ocean colour product is another section

There are two sentences that bother me a little: "no data presented here depend on a high level of accuracy in the estimated chlorophyll concentration" (more ore less the same sentence is given for b_b). Two comments :

- You should argue why (one always expects to tend towards the best accuracy)
- If you have such accuracy doubts for Chla, I would suggest that calibration using the manufacturer "slopes" is not worse. In this case the calibration would provide an independent estimation of Chla that could subsequently be compared to satellite Chla. You do not follow the manufacturer calibration for a reason, but how can you better trust the Chlorophyll concentration extracted from space?

Page 6 Paragraph 2.

- We would like more detail on the vertical resolution (a table?)
- Fluorescence at midnight. What is the temporal shift in the midnight surfacing due to longitudinal drift of the float over time?

Page 6 Paragraph 3.

- Not necessary to specify the wavelengths again. Already described in the first paragraph of the same page

Page 9 Paragraph 2.

I feel that your closure regarding C/Chla of phytoplankton is tentative at most, if at all. First the turbidity sensor LLS is argued to be a backscattering proxy which is calibrated by a remotely detected b_b which is further converted into phytoplankton carbon using Berhenefeld et al. This represents a lot of steps!!! In any case I do not consider that this argumentation is in any way a demonstration that your data are good, regardless of me trusting your data.

Results

Page 10 Paragraph 2.

Not very clear to me

Of course "Surface data also correlate…", because from what I have understood, satellite data has been used to calibrate the signal.

Thus the sentence "although the same…" should at least come before the previous one

Figure 3 (and comments)

Regarding the plots Chla vs Chla and bb vs bb, it would be better to have Log-Log plots to evaluate how both quantities compare for low concentration values.

Figure 4 (and comments)

I suppose that data from the upper 300 m are the grey ones. This should be specified.

From Figure 2, I suppose that the detection limit of Chla is at least above 0.04 mg m^{-3}. Data below this threshold (or a threshold that you would determine less empirically than me) should thus be discarded on Figure 4
Your data agree with the Reynolds data for the Ross Sea but not with any other relationship. The slopes of Reynolds for APFZ is not steep enough. The two slopes of Berhenfeld are not confirmed, especially if you remove the Chla data below your detection limit. By the way in Berhenfeld et al (2005), below Chla = 0.14, b_b is constant and not decreasing, as reported here. Furthermore, I wonder if there isn't some confusion between the Reynolds Ross slope and the Wang one.

Page 11 Paragraph 2.
You have no argument to specify that the observed increase in Chla b_b^{-1} is more a photo-acclimation effect than a change in community structure (involving size change that affect b_b). Perhaps you are right for the summer (but still there is huge stratification of community structures following water column stratification at such a period). This paragraph should be more nuanced.

Effects of Clouds:
Sorry but the Figure and its interpretation is not clear for me. Coefficient of variation of what ? satellite chlorophyll or float chlorophyll? Does a higher coefficient with high satellite coverage means "catching more spatio-temporal events" or "there are more spatial temporal events in summer than in winter"?

The eddy event:
Figure 10 is very nice. But it clearly stresses the decorrelation between Chla and b_b which do not follow the rule enunciated in page 11 paragraph 2: "backscattering being dominated by phytoplankton and particles that covary with phytoplankton".
What is the size of eddy?

Discussion and conclusions
Page 13 Paragraph 2.
Again this is circular. The agreement would be possible to establish only if independent calibration of the sensors have been done.

Page 13 Paragraph 3.
This paragraph has to be connected with the previous one. I am not as conclusive and affirmative as you regarding Chla vs bb (see my comments for Figure 4).

Page 13 Paragraph 4.
Not necessarily due a physiological response to light and nutrients. I would suggest that this short decorrelation time scale reveals the dynamics of phytoplankton biomass change (growth, grazing mortality). In fact it is what you develop in the following paragraph.
Last sentence. I agree. In fact the Chla vs bb relationship is valuable to the first order. But nuances do exist at some specific scales and this is the interest of these new technologies in illustrating a deviation from the overall rule (e.g. the eddy event) by accessing new observations and processes. Such ideas should be clearly stated at some point in the manuscript.

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