Dear L&O editor,
Attached you will find the revised version of the manuscript 'Observations of pigment and particle distributions in the Western North Atlantic from an autonomous float and ocean color satellite' in response to the' (L&O ms 07-399). The manuscript has benefited from the reviewers comments as well as careful reading by a professional editor (and ex-oceanographer). Below you will find detailed responses to ALL of the reviewer's comments. We thank both for the hard work they have put into the review which improved the manuscript significantly.
In the name of all the authors,
Emmanuel Boss

Answers to reviewer #1:

We thank the reviewer for the time and effort he or she put into the review. We have answered all of his/her comments and have modified the paper to reflect his/her insightful comments.

Below are answers to all his/her comments (provided in bold type):

**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.** – The reviewer has raised very good points which we will answer in 3 steps (and add the information and have added the appropriate information in the paper as well):

1. Chl_a fluorometers cannot be calibrated accurately to provide chlorophyll concentration in-situ under any circumstances (see Cullen, 1982 and ACT fluorometry workshop report, http://www.act-us.info/download/workshop_reports/ACT_WR05-03_In_Situ_Fluorometers.pdf). Changes in underlying algal composition (species, size, nutritional status, light history) can all change the ratio of fluorescence to the concentration of chlorophyll. Since the remote sensing reflectance (Rrs) is sensitive to absorption by phytoplankton using that for calibration is appropriate. Using the manufacturer calibration values will result in a bias (as it is based, at best, on a phytoplankton culture or a spinach extract). We have added more detailed about the

calibration including comparison to fluorometers that were calibrated against locally extracted chlorophyll by Pete Strutton.

2. The reviewer is right that the parameter (chl) so derived cannot be used to validate remote sensing. At best it can be used to fill in the gaps when clouds are present and extend the surface measurements to depth. We have made it clearer in the text.
3. WRT backscattering: our specific prototype sensor was an LSS (e.g. Baker et al., 2001). It does not measure the VSF at one angle in the back direction like modern backscattering sensors but rather a region of the VSF in the side direction. In several deployments we and Mike Twardowski have conducted we have found backscattering sensors and LSS to correlate extremely well. LSS have been calibrated with formazine to provide output in NTU and beads and compared with a beam transmissometer. We added the appropriate data in the paper. However, since the LSS has a large sampling volume it is very hard to calibrate it accurately with small vessels as is done with backscattering sensors. Cross calibration with three backscattering sensors that were co-deployed with the float are described (provided by P. Stratton), which agree to within 20% with the vicarious calibration against the satellite. Given the much longer data recorded wrt remote sensing we keep the same calibration but provide result of other calibration as well as propagate the errors in our approach in an uncertainty budget. In addition we note in the paper that the variation in the deep water backscattering is on the order of 10% of the backscattering by pure seawater. Using the float data as validation for satellite derived backscaterring will therefore entail that level of uncertainty.
4. The best approach, as the reviewer notes, is to use a properly calibrated backscattering sensor which we recommend in the discussion.
5. Post-calibration is not possible with sensors on floats as they disappear once their battery runs out.

**Introduction**:
**Not very ambitious regarding the new data that you provide and the interpretation that is done** –We believe we have improved it in the latest version.

**<u>Paragraph 1</u>, sentence 2. Very general and long. Should be cut into two sentences. NPP, for primary production. Why not simply PP?** –Corrected as suggested.

**<u>Paragraph 2</u>. 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.** – The paragraph was modified to better clarify the point.

**<u>Paragraph 3</u>. 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.** – Changed as suggested.

**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.** – This section has been rearranged as suggested.

**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)** – Agreed. Sentence was eliminated and uncertainties quantified.

• **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?** – We now provide all the different slopes from multiple calibrations, discuss the issues associated with fluorescence, and provide uncertainties.

<u>Page 6 Paragraph 2.</u>

• **We would like more detail on the vertical resolution (a table?)** – Added in text.

• **Fluorescence at midnight. What is the temporal shift in the midnight surfacing due to longitudinal drift of the float over time?** – Added in text.

<u>Page 6 Paragraph 3.</u>

• **Not necessary to specify the wavelengths again. Already described in the first paragraph of the same page.** – Dealt with as suggested.

<u>Page 9 Paragraph 2.</u>

**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. –** This paragraph was revised in the text. The chl/$b_{bp}$ ratio in the upper ocean behaves as one would expect from phytoplankton photo adaptation. Application of Behrenfeld et al.'s equation results in a reasonable chl/phytoplankton ratio (based on cultures), suggesting things are indeed consistent. I have no doubt that this relationship can be improved. It is used here only in passing, to illustrate that a reasonable value of a physiological parameter is obtained.

**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. –** The satellite data provided a single constant for calibration. The correlation coefficient is independent of that constant.

**Thus the sentence "although the same…" should at least come before the previous one –**Changed the order as suggested.

**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.** – Changed to accommodate the reviewer.

**Figure 4 (and comments)**
**I suppose that data from the upper 300 m are the grey ones. This should be specified.** – Changed as suggested.

**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** – Data are not discarded as uncertainties are discussed in the text. While we have large uncertainties in the data the measurements are what they are. We have included in the figure caption a statement that chl data<0.04 are not significantly different from zero.

**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. –** We have removed the Wang relationship which does not agree with their own data and added that of Huot et al., 2007, instead.

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.** – The sentence was 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 section was rewritten to clarify the point that when the ocean is covered with clouds, we usually observe little variability in chlorophyll.

*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".** -- To the contrary. Except for the eddy event, to O(1), the two covary closely. Deviations seem systematic, possibly associated with nutrient limitation following the bloom (resulting in decrease in pigmentation). Obviously, below the euphotic depth one would expect a significant change in the correlation between the two, however the integrated signal is dominated by the near surface values.

**What is the size of eddy? –** about 1.5 degrees in diameter, see Fig. 9.

**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.** – The agreement we refer to is the trend in time (the temporal correlation), not the absolute value. We made this clearer in the text.

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) **–** dealt with above.

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**. Agreed

**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**. – Agreed.

Answers to reviewer #2:

We thank the reviewer for the time and effort he or she put into the review. We have answered all of his/her comment and have modified the paper to reflect his/her insightful comments.

Below are answers to all his/her comments:

1. **'However, the paper pushes its 'carbon relevance' much too far since scattering is not as useful a predictor of POC as is beam attenuation coefficient.'** -- Two main points make backscattering relevant to carbon science: a) Remotely sensed ocean color is proportional to backscattering. Thus, if we hope to predict POC from space we need to go through backscattering. b). Published (see below answers to specific comments) regressions between backscattering and POC have very similar correlation coefficients as those for beam attenuation. This should not be very surprising in open ocean environments where the particulate size distribution is relatively stable, and where the particulate backscattering ratio (the ratio of particulate backscattering to particulate scattering (which equals particulate attenuation in the near-infra-red) is relatively constant.

2. **'Claims of fouling free operation simply can not be verified by the data. Unlike moored optical sensors, which do fail quickly, sensors on floats are not rendered unusable by fouling. That's**

**about as far as they can go.'** -- Similar to the way physical oceanographers assess fouling, we look at changes in values measured at 1000m. We do not notice any drift in those values which would be expected for a fouled instrument (optical instrument strapped to moorings drift monotonously as function of time, e.g. the work of Dickey and Chang). Variability in the deep water over time is less than 10% of the backscattering by pure seawater. In addition, at the surface, the values match well remotely sensed values without drifting away from those values as function of time. Since fouling in optical instruments exhibits itself with change in signal as function of time, we conclude that no significant fouling is taking place.

3. **'Many of the discussion points in the conclusions regarding telemetry, and the suitability of floats for optical sensors have been published before. Other optical floats have gone on to perform for year long time scales with sensors operational. –** We were not aware of optical float that have performed for years (and could not find it in our searches). To our knowledge 8 mo is the maximum that has been reported by Bishop. The discussion regarding telemetry is useful to readers that are not in 'the know'. We looked for it in the published literature regarding optical floats and did not find it.

4. **'I'm not sure what 'targeted experiment' means. The 2001 dust event described by Bishop et al. (2002) was not a targeted event. Bishop et al. 2002, 2004 used Carbon Explorers to provide a much more detailed description of POC variability than presented here, including (in the 2004 paper) links between POC and sedimentation. The integration of satellite data with float optics was fundamental in the 2002 paper, the 2004 study took place under perpetual cloud cover. These papers also examined the relationship of biomass and density structure and underwater light fields... That said, a delta sigma theta of ~0.03 to 0.05 is much more useful to define a mixing mixed layer.' –** We have much respect to Mitchell's and Bishop's pioneering works with floats and hope that our text conveyed it. What was meant by targeted experiment was that the papers and abstract that came out of these floats were limited in their analysis to times and space scale describing specific processes. The long-term monitoring capabilities of the floats were not the focus of the these studies. The integration of satellite and remote sensing in Bishop's 2002 paper was crucial but was used to add information to the study, not showcase how the two agreed in surface waters in order to expand satellite estimated variable to depth, the focus of our paper (since satellite and sensor measured/estimated different quantities). – The choice of delta sigma theta varies among researchers in physical oceanography. There is NO one best criterion. The one adopted here is based on a study we performed relating vertical distribution of optical and physical variables (Zewada et al., 2005). 0.125kg/m$^3$ is based on Levitus (1982) and was found to provide a good prediction of MLD in optical properties (Zewada et al., 2005), and hence in biogeochemistry. Since we are not analyzing the short term variability in the active mixing layer (in contrast to Bishop's studies) but rather longer term dynamics in mixed-layer depth the criterion chosen is appropriate.

5. **The authors describe benefits of floats and later make recommendations that have been already demonstrated by the Bishop et al. papers. In these papers, the optics enhanced floats observed a rare but important dust event and tracked a moving patch of iron treated water... for months and later went of the rest of the year; both sets of observations took place in substantially cloud covered areas. A little on this would be appropriate up front. It strengthens the argument for floats to "DO" biology.** – We have added several citations to Bishop's work. Sampling under cloud was discussed with respect to the use of float to fill gaps in ocean color products. This application was not discussed by Bishop.

6. **I'm sure the authors can find reference in AGU meeting abstracts (at the level of the Mitchell et al., 2000 report) to the fact that Bishop's Carbon Explorers have gone on for year time scales.** – We found it in the supplemental material to the 2004 and include it now.

7. **The unique contribution of Boss et al. is not going for years, but doing a good job with Chl and scattering. It's fine that they went for years as well. Tone down the depreciation of Mitchell and Bishop efforts.** --We disagree regarding the time scale. Our study was designed and funded to show the ARGO program community the value of adding biogeochemical sensors and showing that they do not hurt the physical measurements. We have achieved this goal. We did not mean to depreciate Mitchell's and Bishop's contribution and do not feel we have done so. In any case, we have added more citations to their work where appropriate.

8. **Title: "Robotic" little evidence presented to suggest other than blind following of a single mission. "Autonomous" makes more sense for vehicle described here.** – We agree and changed the title accordingly.

9. **p 5. "a custom flat-faced WET Labs hockey-puck size optical sensor that measures side scattering at 880nm (analogous to WET Labs LSS, e.g. Baker et al. 2001), and a chlorophyll a fluorometer (470nm excitation, 680nm emission, analogous to the commercially available WET Labs ECO fluorometer). " o) Not sure if the hockey puck has both scattering and fluorescence. If not say which product this was.** – As written, this is a single, prototype, puck with two sensors. Currently one can purchase a similar sensor (WETLabs' FLNTU) but these are not identical. We changed the wording to make it clearer.

10. **o) Which oxygen sensor was interfaced to the float? o) Did the O2 sensor provide useful profiles? Very relevant to the request to add O2 to ARGO floats.—**The O2 sensor was that of SeaBird. SeaBird claims it diagnosed the problem and fixed it for future sensors and we see no reason to drag them in the mud over past bugs. The profiles of O2 when it worked were useful and realistic. O2 sensors on float have been analyzed is several published

paper (e.g. those of Körtzinger) and we see no advantage here of showing only a fraction of the O2 profiles.

11. **o) very little specifics regarding these sensors. What is the precision of the Chl detection? That for scattering? –**We added much information about the sensors calibration and uncertainties.

12. **"Side scattering was vicariously calibrated to backscattering units at 440nm [m-1] using remotely sensed ocean color as described above." o) Explain vicarious calibration a little better. o) Describe the equation for conversion of NTU to B440. Linear? o) What was the impact of sensors on the float's power budget and telemetry time scales? o) WHAT WAS THE EXPECTD LIFE TIME OF THE FLOAT WITH AND WITHOUT THE SENSORS? –**We added most of the information to the paper. The lifetime is battery and mission dependent and could not be generalized in a way that would make sense to the readers and thus elected to omit it.

13. **How was float surfacing time determined? UTC midnight? Local solar midnight (longitude). Was the float outfitted with GPS to do this? –**Local solar midnight at the deployment location. As the float drifted so did its time of surfacing (a total of less than two hours). We added the information to the paper.

14. **p 9. "Scattering has been found to be a good proxy for particulate volume, mass and particulate organic carbon (Spinrad and Zaneveld 1982; Babin et al. 2003). " o) Can't have this all ways since particulate mass and POC vary in their percentages with depth. Bishop (1999; DSR) has a nice illustration of why POC and MASS do not covary. Carbon relevance is oversold and not supported here. –** Carbon relevance is not supported here in the sense that we did not measure POC directly. Work on relationship between bbp and POC has been published (Starmski et al., 1999 and see below) and is currently being pursued in several studies (by the groups of Stramski, Balch, Babin, Behrenfeld), given the obvious link to ocean color (e.g. William M. Balch, David T. Drapeau, Jennifer J. Fritz, Bruce C. Bowler and Jessica Nolan, Optical backscattering in the Arabian Sea--continuous underway measurements of particulate inorganic and organic carbon, Deep Sea Research Part I: Oceanographic Research PapersVolume 48, Issue 11, , November 2001, Pages 2423-2452. // Stramski, D., Reynolds, R. A., Babin, M., Kaczmarek, S., Lewis, M. R., Röttgers, R., Sciandra, A., Stramska, M., Twardowski, M. S., and Claustre, H.: Relationships between the surface concentration of particulate organic carbon and optical properties in the eastern South Pacific and eastern Atlantic Oceans, Biogeosciences Discuss., 4, 3453-3530, 2007. // Stramska, M., and D. Stramski. 2005. Variability of particulate organic carbon concentration in the north polar Atlantic based on ocean color observations with Sea-viewing Wide Field-of-view Sensor (SeaWiFS). *Journal of Geophysical Research*, 110, C10018, doi:10.1029/2004JC002762//The latest paper on the

subject as just beem accepted to biogeosciences by Starmski et al.). Some of these last citations have now been added to the reference list to make the case that bbp has been observed to be a good proxy for POC though much more work needs to be done, in particular in establishing the variability in global relationship (just as has been observed for cp). Since the ratio bbp/cp is constrained in the ocean (e.g. Whitmire et al., 2007) it should not be surprising that bbp can work as a proxy for POC. Variability due to size and composition should be expected, just as with the cp-POC relationship. It is true that the theory associated with beam attenuation is simpler than that associated with backscattering, but all indication suggest that as we learn to build, calibrate and use backscattering sensors well, the relationships with POC in the environment are as constrained than those obtained with beam transmissometers.

15. **p9. "Recent work suggests that the phytoplankton contribution to the total particulate material in the upper ocean is often constrained between 25 and 40% across seasons at one location (DuRand et al. 2001) and across different trophic regions (Oubelkheir et al. 2005). " o) Please review the recent publication (Marine Chemistry 105 (2007 - pp208-228) on POC biases. Plankton carbon % may be low because there is an adsorbed DOC blank that impacts POC analyses.** – this statement was removed as it did not contribute to the main point of the paper.

16. **p 10 "Except for rare spikes in the back-scattering coefficient,..." p 13 "The data quality was maintained with no fouling observed."o) Since the source and detector of backscattering or fluorescence sensors cannot see each other, some fouling of either or both source and detector surface areas is very hard to detect.** –Our estimation of detectable biofouling is based on the fact that the values at depth were very stable throughout the life of the float and that the relationship to remotely sensed variables did not drift in time.

17. **o) The fact that floats spend much of the time in deep water and can minimize such fouling and has been noted before.** – true. We added a citation for that.

18. **o) The apparent scatter of Chl in Fig 2 suggests some contamination. What explains the 'spikes' of scattering. Approximately 50 instances of spikes in deep scattering seem not to be rare out of 222 profiles. Chl noise seems even more frequent and initiates in 2 nd year. Why is this not fouling?** – What we are displaying is all the data below 970m. We are measuring scattering off a limited volume of water. Rare big particle appears as spikes. We believe that the separation of the chlorophyll data into two clumps may be due to an instrumentation warm-up issues, as they are seen throughout the sampling period between two consecutive measurements. This effect is not getting worst with time, is small relative to the surface signal and is therefore deemed negligible and does not seem to be due to continuous biologically mediated fouling.

19. **Conclusions: "Eddies such as that sampled by the float and those observed at BATS could be very important (even dominant) in the global biogeochemical inventory of carbon and its flux to depth. However, currently we cannot account for such contributions due to our inability to sample the subsurface ocean from space and the limited space and time scales observed using ship board observations or single point moorings which cannot capture many realizations of such eddies. ..." o) There is a big leap to suggest enhanced scattering means enhanced POC. Equally and much more likely is enhanced coccolith abundances.** – We address this issue in the text. According to a coccolith expert we talked to (and now attribute the discussion to) it is unlikely that coccolith will make it to 1000m depth within 5 days (the elevated signal is coherent from surface to 1000m). We do not observe a bloom of coccolith at the surface. Where are would they be coming from?

20. **"Newer communication technologies such as satellite cell phones (e.g. iridium) can significantly improve future float missions; since they allow for significantly shorter stays at the surface (to less than an hour per profile), two-way communication (allowing for adaptive sampling), and provide for higher vertical resolution of data for the same amount of power. " o) Already demonstrated by Bishop et al. papers. a description of what they did seems appropriate.** – our discussion was designed for the non-initiated and appreciated by the editor. We kept it and added reference to the Bishop papers.

21. **Figures. Fig. 2. Does not demonstrate fouling free behavior. Reconcile with Fig 7. What are 'rare' spikes** – we clarified the meaning of spikes. Fig. 7 contain median over bins of depth while Fig. 2 the individual data point (thus the 'noise'). – we disagree. See our answer to point 2 above.

22. **Fig. 3 Nice results, explain why float seems to lead the satellite record for both scattering and chl.** –thank you. We do not see the lag. And it does not show up as an hysteresis in the property-property plot.

23. **Fig. 6 Would be nice to see profiles to 1000 m, the sign of sensor contamination is sudden onset of surface to km depth signals.** – this is how we initially plotted the results. The problem is that in most of the water column (between 300-1000m) very little is happening. We though it will not be informative and elected to use Fig. 2. to highlight the no fouling issues. We stand by that decision.

24. **Fig. 7. Integrated Bp 750-1050 db seems not symmetric about eddy, explain why you think the eddy was important. The peak seems much better correlated to frontal transition. See comment of Fig. 9.** – We added lines to Fig. 8 to show the relationship between the integrated bbp with the eddy. The frontal transition denotes the initial increase in bbp not the peak. The largest values in bbp do not occur where the density or temperature gradients are largest as would be expected when associated with frontal features.

25. **Fig. 8. Not very instructive, one could make profiles more frequent and thus gain better statistics.** – the point of this figure is to show that variability in surface optical properties is lower during cloudy period in the Western North Atlantic, and thus does not result in a large bias in ocean color. Obviously the more profile one can make the better are the statistics, however, the shorter is the total life span of the float. We change the text a bit to highlight better these points.

26. **Fig. 9. Would like to know correspondence of scattering peak and float position, can you do the surface elevation map in gray scale and superimpose the position, time,, and float scattering in color like in Fig. 1.** – We modified the figure as suggested.