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Discussions

Interactive comment on "Two databases derived from BGC-Argo float measurements for biogeochemical and bio-optical applications at the global scale" *by* Emanuele Organelli et al.

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The processes and dynamics that define the climate sensitivity of the biological carbon pump are not well understood. This is due in part to our lack of understanding of this complex problem through chronic under sampling of the world's oceans, which do not resolve inter-annual variability and seasonal and intra-seasonal dynamics. Autonomous technology promises to overcome the space-time gap in ocean observations with bio-optical sensors on platforms that are able to profile the water column providing highly cost-effective measurements at high frequency that can characterise the vertical biogeochemistry at smaller scales, but also for sufficiently long periods that may help

to reduce uncertainties associated with carbon budgets at longer time scales. As such, I recommend this highly useful data set for publication and commend the efforts of the authors in all the steps that such an achievement requires; from securing the funds to purchase the numerous floats to arranging for their deployment in a globally diverse manner all the way through to the significant efforts in processing and collating the data into a succinct repository.

However, although I see very obvious benefits in the use of such a database both for ocean colour product validation and to further our understanding of ecosystem dynamics, I have one major concern with regards to utilising the chlorophyll (chla) data for validating ocean colour. The uncertainties in the BGC-Argo chla data are typically large and poorly characterised – often larger than the satellite derived chla estimates (mainly due to the globally applied factor of two bias in the conversion of fluorescence to chl and the simple quenching correction which is difficult to evaluate without night time profiles). This raises some serious concerns with the use of float derived chl a data for match-up based validation application with regard to uncertainty budgets. That being said however, I do not have a problem with the use of the other bio-optical variables (e.g. Kd, bbp, Zeu) for ocean colour validation, which are not susceptible to the same kinds of mismatches in the uncertainty budgets.

Inline with the above, I would recommend some changes to the manuscript that need to be addressed before being suitable for publication and provide some suggestions to improve the database. In addition, I provide a list of minor corrections and suggestions to improve the manuscript and attach a pdf with detailed comments and typos.

1. Major comments

1.1. Using BGC-Argo chla data for ocean colour validation

Although none of the BGC-Argo chla versus satellite chla matchups are presented in the manuscript, the implications to do so for validation purposes are implicit both in the text. (e.g. pg 3 line 4: "data presented in BOPAD-surf are compared with existing bio-

optical models and used in conjunction with products derived from satellite platforms in order to show applicability for validating ocean-color bio-optical products at the global scale" and pg 9 line 5: "measurements collected by BGC-Argo floats are a fruitful resource of data for bio-optical applications ….. as well as the validation of ocean color reflectance (Gerbi et al., 2016) and bio-optical products (IOCCG, 2015)" and pg 10 line 25 "…ocean-color algorithm and product validation can routinely be performed in several regions so that errors and possible causes of failure …. can be assessed and/or solved, and algorithms be refined for improving the quality of retrievals.") and even more so in the data base itself (see http://seasiderendezvous.fr/matchup.php) where chla is the default product for match up locations and the colours of the data points represent the % relative error between float and satellite chla matchups. Given the inaccuracies in the float chla data I am not convinced that such a comparison is meaningful, in particular without any indication of the errors implicit in the BGC-Argo chla data. That being said however, I do not feel that the inaccuracies in the chla data render them ineffectual, on the contrary, these data will provide extremely useful information towards an improved understanding of the biological response to physical drivers and our understanding of the sensitivity of the biological carbon cycle to climate change that will ultimately lead to improved estimates of long term trends. For example, although the Southern Ocean bias in satellite estimates of chlorophyll is well known it does not render the data any less useful, it is however important that the user is well aware of the quantitative limitations of the data.

From my understanding, one of the primary drivers of the errors in BGC-Argo chla is the variable relationship between fluorescence and chla which is not accounted for in the quality control step that divides all chla data by a factor of two to correct for the global bias in the factory calibration. Although Roesler et al., 2017 recommends to do so in order to improve the global accuracy of chla measurements from WET Labs ECO sensors, they acknowledge the regional variability in this factor, which ranges from 0.56 in the Arabian Sea to 7.75 in the Southern Ocean. As such, the global application of a factor of two can create errors that range from an underestimate of "actual" chlorophyll

by $\pm100\%$ in the Arabian Sea to an overestimate of chlorophyll in the Southern Ocean by $\pm250\%$. Would it not be possible to use some of the regional variability evident in the relationship between chla from HPLC and ECO-fl (Roesler et al., 2017, their Figure 1) to derive a more regionally robust factor for correcting the factory calibration bias?

1.2. Quenching correction

Another area that can introduce a significant amount of error into both the profile and the surface chla data is the choice of quenching correction that is applied. The Xing et al., 2012 method of correcting quenching is robust and effective, so long as the assumptions it relies on are valid. The Xing et al. (2012), method relies on the assumption that a) chlorophyll concentrations within the mixed layer are uniform and b) that quenching processes do not affect depths below the depth of maximum fluorescence within the MLD. This method does not allow for sub surface fluorescence maxima to occur within the mixed layer. The method of Biermann et al. (2015) attempts to overcome this limitation by instead finding the maximum fluorescence within the euphotic layer and extrapolating this value to the surface. A comparison of their method with that of Xing et al. (2012) identified occasions (when the MLD was deeper than the euphotic depth) where quenching was corrected without masking subsurface fluorescence signals. However, as with the method of Xing et al. (2012), this method assumes homogeneity, but in this instance within the euphotic zone as opposed to the mixed layer (i.e. it does not allow for daytime subsurface maxima to be present within the euphotic zone). When these assumptions are not met (i.e. chlorophyll is not homogenous within either the mixed layer or the euphotic layer and quenching occurs below the mixed layer) the result will be a typical underestimate of daytime surface chla in the case of Xing et al. (2012) and an over correction of surface chla when the Biermann et al. (2015) method is applied. As mentioned in Xing et al, 2012, for multi-instrumented platforms with both fluorometers and backscattering sensors, Sackmann et al. (2008) proposed an elegant method that made use of the backscattering profile (as independent proxies of phytoplankton distribution) to correct the fluorometric one.

However, this method still relies on certain assumptions such as a regular association between particulate backscattering and chlorophyll concentration. Regardless, if both backscatter and fluorescence sensors are available then methods that utilise both parameters are perhaps more likely to retrieve accurate estimates of chlorophyll during the day.

Either way, it seems to me that there are at least two other methods of correcting quenching which ought to be applied to your data set and the results compared to try to determine which is the best method to use and when. Or at least have an idea of the different surface chla concentrations that the different methods produce in order to get a handle on the possible range of error that this quality control step can introduce. A major problem with having all the profiles in the data set being performed at midday (apart from the obvious issues with quenching) is that it is very difficult to quantify whether or not a daytime profile has been corrected correctly. As such, I would recommend that future float missions consider doing both midnight and midday profiles in order to improve the quality of the chla data (even if this means a reduction in the longevity of the float life span).

1.3. Error estimates

Given that validation is a quantitative assessment of uncertainty and that the BOPAD-surf data set is intended to be used for satellite validation, I feel that it is important to provide some indication of the anticipated errors in the derived variables. If, in the case of chl, you are wanting to validate a satellite product to within ~35% uncertainty (in Case 1 waters) then it is important to know when your in situ product has an error of >100%. A short quantitative analysis of the expected uncertainties in the float derived chla data would be very useful and is necessary here. Similarly, I think that a more open discussion is required around the limitations and weaknesses of the published database together with its strengths.

2. Minor comments

Page 4 line 5: What about positive spikes in chla? I appreciate that they were retained as they could represent "real" data. However they appear to have been removed from the BOPAD-surf data base. If so please provide details. Also, if the spikes were remove, were they interpolated over in the vertical or left as NaN's?

Page 4 line 10: is it possible to please clarify how you systematically determined the profiles that were affected by non algal fluorescence with depth? e.g. an increase in chla with depth for how many meters beyond what threshold depth? how do you ensure that you are correcting for non-algal increases of fluorescence with depth and not "real" subsurface increases in chla e.g. via a subducting water mass?

Page 5, line 11, step 3: this step is not clear to me? it has already been implied that positive spikes in some data were retained (e.g. chl and bbp) as they can represent "real" information. As such it is not clear to me how you used sharp gradients with depth to test for instrument drift?

Page 5 line 21: how did you get the PAR value just below the surface? please provide method? e.g. fitting an exponential Page 6, line 1: was a similar median filter applied to the chla data to remove positive spikes from the chl data set? If so please describe the method used.

Page 6, line 15, figure 3: please include the MLD and Ed on the example profiles

Page 6. Line 23, figure 3c: t is not clear to me how it is possible to retain this shape of profile if the Xing quenching method is applied. unless the MLD is very shallow. in which case what is the Ed relative to the MLD? as it is possible to still have significant quenching below the MLD if the Ed is deeper

Page 7, line 2: there is no mention in the methods section on quality control about positive spikes being removed from the bbp profiles. On the contrary in the methods section is says that the positive spikes are retained. Please describe the method used to remove positive spikes. Also, please clarify whether the positive spike data set was

retained separately so that flux estimates as per Briggs et al., could still be performed?

Page 10, line 10: I am not convinced that you can say anything concrete about the representativeness of the previous model without having a handle on the errors in the bio-argo chla data? if the errors in the chla data can be as much as 100% it is likely that they would significantly affect the shape of the 3 order polynomial fit. In particular all the SO data points which lie above the Morel fit are likely to "in reality" all be shifted to the left (i.e. lower chla) and closer to the Morel model line?

Page 10, line 20: I think that a really interesting discussion here would be the regional range in errors in chlorophyll associated with the global application of dividing the chla data by 2.

Page 11, line 1: The discussion does well to highlight the number of profiles, the regional coverage etc but I think what is lacking is a discussion of the benefits of a high resolution long term time series of biological and physical parameters that bio-argo can provide e.g. showing both seasonal and sub-seasonal variability....and in some cases perhaps even inter-annual variability (depending on the life time of the float or the succession of floats in a similar water mass). I would suggest that this ought to be highlighted with an example time series from one of the floats showing physics (e.g. temp) and biology (e.g. chlorophyll).

Page 11, line 15: I think that it would be good to mention some of the other bio-argo data bases that are currently available e.g. SOCCOM and perhaps plans to integrate them if any?

Table 2: Perhaps add the abbreviations to the Basin section of the Table to reflect those in Figure 1.

Figure 1. It is hard to see both the surf and prof stations on this Figure. I wonder if they would be clearer if you reduced the size of the blue dots slightly and outlined the red diamonds in black.

Please also note the supplement to this comment: https://www.earth-syst-sci-data-discuss.net/essd-2017-58/essd-2017-58-RC2-supplement.pdf

Interactive comment on Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2017-58, 2017.