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Biogeosciences Discussions

Interactive comment on "Towards a merged satellite and in situ fluorescence ocean chlorophyll product" *by* H. Lavigne et al.

H. Lavigne et al.

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Response to reviewer #2 for the manuscript "Towards a merged satellite and in situ fluorescence ocean chlorophyll product" by H. Lavigne et al.

We have modified the manuscript according to your suggestions and those of the two others reviewers. We think that the new manuscript has been accordingly improved.

In the following, we write each of the reviewer #2 comments and answer below:

General Comments

Point 1. The authors compare their method to that of Boss et al. (2008), which is very similar, but uses a single set of "calibration coefficients" to transform all the flu-

orescence profiles, as opposed to profile-specific calibration coefficients. I am not surprised that the results are similar (which is good, but not all that insightful). I think a far more useful comparison would be one that might be employed in the absence of in situ profiles at all. For example, how does the method compare to taking the satellite surface value and generating a profile using Morel and Berthon (1989) or Uitz et al. (2006)? If the results are similar, then it mitigates the importance and novelty of the approach outlined here. This should be a fairly easy exercise to carry out and would be much more instructive. This essentially tells us how important having the in situ data and employing the authors' proposed method actually is. Considering the error using the approach outlined here is still âĹij30%, the Gaussian methods might not be much worse? Perhaps this is untrue, I am not sure, but I think this would add tremendous value to the analysis. I realize that column-integrated Chl used to determine the alpha parameters are derived, in part, through use of Uitz et al. (2006), but I don't think this would be a circular exercise. Please correct me if you think otherwise.

Authors response: We are not sure that the comparison of our method with the one of Morel/Berthon or Uitz et al (2006) deserve to be deeply investigated, even if both methods use remotely detected surface [Chl-a] as an input variable. Our method is indeed developed for a case by case correction of fluorimetric profiles. The two other methods were developed for global application (inferring global vertical distribution of phytoplankton size classes, computing global primary production). The application of these methods on a case by case basis or even regional one is not recommended (Uitz et al., 2006). To not apply these methods out of their range of application, we did not compare our approach with them.

Point 2. This is somewhat of a philosophical point, but warrants some careful thought and possibly inclusion into this manuscript. By accepting the premise that in situ fluorescence profile data are in error and need to be "corrected" to be consistent with satellite ocean color data, are we rendering the utility of this same profile data as

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ground truth for satellite data invalid? That is, there is currently emphasis put on use of autonomous platforms (moorings, profiling floats) as tools to validate satellite estimates of Chl and other bio-optical properties. However, this manuscript is a demonstration that this is a dangerous proposition. How can we reconcile this contradiction?

Authors response: This point is quite interesting and it generated several discussions between the authors before the submission. Let us explain our point of view. First, we are convinced that an accurate validation of the ocean color chlorophyll product cannot be based on fluorescence data only, as they don't reach the scientific requirements needed for this kind of exercise. In this context, fluorescence profiles have been used to improve the reconstruction of the vertical profile of chlorophyll, although the primary data used were from HPLC (as for example in Morel and Maritorena, 2001). Second, and more specifically for autonomous platforms, no general consensus exists about the best method to obtain "accurate" chlorophyll from fluorescence data, when classical methods of correction (i.e. HPLC calibration) are inapplicable. To our knowledge, the first method proposed to correct fluorescence data on profiling floats (Boss et al.,2008) used satellite correction. However, other approaches were recently proposed (Xing et al, 2011; Mignot et al., 2011) that make use of ancillary information and can represent an alternative to the methods based on ocean color estimations. Therefore, even if we show that satellite is a possible way to correct fluorescence observations, other methods should be tested, to avoid the "dangerous proposition" that you highlighted. Although our method could be used as a calibration method for fluorescence, it should be considered as a "last" instance, when other methods failed or are inapplicable. Finally, our approach is rather a method to derive merged products rather than an operational method to correct in situ observations. For this reason, we paid attention to present it as a merging procedure, although the practical way to do this is that we correct the in situ fluorescence profiles. Overall, the aim of the paper (as also explicitly stated in the title) is to provide an efficient method to generate in quasi-real time an improved satellite/in situ merged product. Hence, a robust Quality Control (QC) system will have to accompany the future network of autonomous platforms (as pro-

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filing floats) observing chlorophyll. In the Argo Temperature and Salinity network, one of the steps of the QC system is the comparison of the acquired data with reference databases and/or climatology. For the chlorophyll parameter, the lack of sufficiently vast and coherent databases could prevent that kind of QC, critically slowing the set up of a possible Bio-Argo system. Our method is a first attempt to fill this gap and, in this sense, our application to profiling float was more a demonstration of feasibility for this type of data rather than a QC proposition.

To better explain our thoughts, we added a sentence in the Conclusion section: "In this framework, our approach could be considered one of the steps for a future Quality Control system for a network of profiling floats. However, it should be used only when other methods fail or are inapplicable, to prevent any redundant information or circular exercise if a validation of satellite ocean color products is attempted with the profiling floats observations."

Point 3. Are there any systematic variations in the alpha parameter that would be informative? Does alpha vary with time of year? With column integrated Chl? If so, this might tell us what is modulating the remaining variability which is unaccounted for and provide an avenue to improve the accuracy of the approach. The dataset the authors have employed should be extensive enough to explore this idea

Authors response: We analyzed the evolution of alpha parameter as a function of the time and of the integrated [Chl-a], separately for the three test stations. No specific patterns appeared. Our interpretation is that the variability of the fluorescence data due to different instruments used in the three stations is much larger than the variability induced by the environmental conditions. Consequently, we decided to not join additional analysis/discussions to the manuscript.

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Interactive Comment

Point 4. The correction for NPQ that has been employed should be considered a possible underestimate. Relaxation kinetics for certain types of NPQ can take a long time (several hours), so that the fluorescence maxima observed in the mixed layer may still have a significant degree of quenching, particularly in shallow mixed layers. Is there a seasonal bias observed in the HPLC data versus the satellite-corrected profiles that support or disprove this notion?

Authors response: The comparison of "satellite-corrected" [Chl-a] estimation with HPLC did not show any particular seasonal biases, which could be ascribed to the NPQ correction method. However, we are aware that the NPQ correction method we used presents some weaknesses (see also point 11 of response to referee 3). Especially, we agree that quenching could be underestimated if the mixed layer is shallow. Nevertheless, statistical tests reveal that the quenching correction method has a significant positive effect compared to a situation with no NPQ correction (paired t.test on two sets of 776 points, pvalue < 0.01). Consequently, we decided to keep the NPQ correction method we used even if it is not totally satisfactory. The NPQ issue is, however, presented in more detail in the new version of the manuscript, with a whole new paragraph in the Introduction section and a more in-depth analysis in the "Method" section (see point 11 of response to referee 3).

Point 5. I appreciate the use of 1.5x the euphotic zone as the depth domain of choice, but I disagree with the statement that ". . .important phytoplankton biomass is often present below the euphotic layer (Uitz et al., 2006)." There may be Chl down there, but it is not the result of significant biomass, but rather extremely high intracellular Chl (e.g., photoacclimation). To determine this you may ask what is the integrated beam attenuation (as an example) in the layer 1-1.5xZeu relative to that within <Zeu. In terms of actual biomass or productivity, I think it is more often than not, insignificant at these deep depths and low light levels. Please distinguish between biomass and pigment when you make this statement.

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Authors response: We agree with the referee and we changed the text as you suggested.

Technical Corrections

Page11901, line 17, Sentence should read, ". . .variability is of. . ..", not "on" Page 11906, line 29, Sentence should read, ". . .see Table 2 for . . .", not "to" Page 11912, line 26, Sentence should read, ". . .of whom were associated with. . ..", not "to" We corrected the text as suggested.

Just a suggestion, but you may be able to remove Table 3, which is just a reproduction of coefficients from Uitz et al. (2006). We agree with the referee and we removed Table 3.

Interactive comment on Biogeosciences Discuss., 8, 11899, 2011.