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

Anonymous Referee #2

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General Comments

This manuscript describes a generic approach for transforming in situ chlorophyll fluorescence profiles into quantitative estimates of chlorophyll (Chl) concentration in the absence of reliable instrument calibration. The method requires that 1) some deep value in the profile be set equal to zero Chl, and 2) a single parameter can adequately characterize the relationship between column integrated Chl estimated from satellite ocean color data and the column integrated fluorescence profile. This is, in effect, a vicarious calibration which uses satellite data as the “true” Chl concentration. The method is applied at three well-characterized sites: BATS, HOT, and DYFAMED. Performance is compared to actual profiles of HPLC-based Chl using metrics such as the semi-interquartile range, the median percent difference, and r^2 among others. As

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an independent check, the authors also compare the approach with a similar existing method reported by Boss et al. (2008). In general, the authors show that method works well, although without significant improvement over earlier work (e.g., Boss et al., 2008).

I see no major flaws in the thinking or presentation of this work. It is a very tractable problem, and the solution described is very straightforward and could easily be implemented by a reader inclined to do so. Therefore it might be a useful tool for someone who wishes to exploit fluorescence profiles that, as the authors point out, are often underutilized. I recommend this paper for publication after minor revisions, which I feel need to be addressed.

Specific Comments

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 fluorescence 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 ~30%, 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.

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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 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?

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.

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?

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 1-1.5x Z_{eu} relative to that within $<Z_{eu}$. 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.

Technical Corrections

The manuscript is well prepared and I noticed very few mistakes.

Page 11901, 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”

Just a suggestion, but you may be able to remove Table 3, which is just a reproduction of coefficients from Uitz et al. (2006).

Figures are clear, well documented and easy to understand.

References

Boss, E., Swift, D., Taylor, L., Brickley, P., Zaneveld, R., Riser, S., Perry, M., and Strutton, P.: Observations of pigment and particle distributions in the Western North Atlantic from an autonomous float and ocean color satellite, *Limnol. Oceanogr.*, 53(5), 2112–2122, doi:10.4319/lo.2008.53.5 part 2.2112, 2008.

Morel, A. and Berthon, J.: Surface pigments, algal biomass profiles, and potential production of the euphotic layer-relationships reinvestigated in view of remote-sensing applications, *Limnol. Oceanogr.*, 34(8), 1545–1562, 1989.

Uitz, J., Claustre, H., Morel, A., and Hooker, S.: Vertical distribution of phytoplankton communities in open ocean: an assessment based on surface chlorophyll, *J. Geophys. Res.-Oceans*, 111(C8), C08005, doi:10.1029/2005JC003207, 2006.

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