

Interactive comment on “An optimised method for correcting quenched fluorescence yield” by L. Biermann et al.

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We gratefully thank Referee #1 for their comments and suggestions, which have improved and clarified parts of the manuscript. Detailed responses below:

General comments:

Although the figure 2 of this manuscript displays interesting results about NPQ, this manuscript does not significantly contribute to scientific progress.

We think there is perhaps a misunderstanding, which we hope to clarify here. The main aim of this manuscript is to present a methodology which is an improvement/alternative to one already published and applied. This will hopefully improve existing scientific progress; not to improve science per se. We have now reinforced this in the final

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paragraph of the introduction.

I get the feeling that authors do not understand the main concepts mentioned. Especially, there is confusion about the concept of “mixed” or “stratified” water column which could have important consequences. Indeed, authors proposed to extrapolate up to surface the highest chlorophyll concentration value observed within the euphotic depth, but they never mentioned that this is only valid if the euphotic depth is shallower than the mixed layer depth. Otherwise, the chlorophyll value at DCM would be extrapolated up to surface.

The main concepts described in this manuscript are quenching and light attenuation, which we understand and feel we explain thoroughly. We made no distinction between ‘mixing’ and ‘stratified’ in the original submission. However, we agree that this simplification did not add value. Thus, we have now added a significant amount of detail to address this comment, and we believe this has improved the manuscript. We thank reviewer #1 for improving our explanation of the method’s strengths and limitations.

In addition, although night profiles are available, there is no tentative to validate the method proposed here.

Potential day DFM are corroborated whenever night profiles are available, as mentioned in the methods and discussion sections. However, because this is unfortunately not always possible and because the seals are constantly moving in space and time, the quenching correction method cannot rely on the night data for validation.

Specific comments:

Page 1246 / lines 5-6: This sentence is unclear. What is meant by DFM? All the quenched fluorescence profiles are not similar to fluorescence profiles with a DCM.

When testing uncorrected quenched (day) data for subsurface maxima, 90% of the fluorescence data returned by all tagged seals were flagged as ‘DFM’. Thus, we feel confident in our statement that quenched fluorescence profiles are similar in shape to

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distinct DFM/DCM. We have, however, added a reference by Mignot et al. (2011), who asserts the same, and moderated the sentence in question.

Page 1246 / lines 19-25: The limits of the Xing et al. (2012) method should be presented here.

The limits of the Xing et al. (2012) method were briefly introduced in this paragraph but we have added more information about stratified vs. mixed waters.

The reference for the method is Xing et al. (2012) not Xing et al. (2011).

Corrected accordingly. Thank you.

Page 1247 / lines 8-11: The description of the study area is too short. This hinders the interpretation of data by the reader. For instance, a description of the climatology of mixed layer depth is missing.

We respectfully disagree. Because this is a method paper, a detailed description of the region would be irrelevant. We have already provided a map of seal locations with Africa and Antarctica included for reference (Fig. 1). Furthermore, a description of the climatology of the MLD in this region is unnecessary. We use near-real time in-situ temperature and salinity data to determine the density-derived MLD, following the method described in detail by Xing et al. (2012 and 2013). However, we have now included a sentence about the general summer MLD climatology for the region.

Page 1247 / lines 15-24: Although many details are given on the way that fluorometers were glued to elephant seals, which is of limited relevance in this study, there is no information about the sampling strategy of elephant seals.

Agreed; the information on how tags are glued to the seals is indeed of limited relevance to the method. However, in order to allay concerns about potential harm that tagging does to these animals, we include these few sentences to assure readers that ethical protocols and procedures are strictly adhered to. Again, as this is a method paper, we feel that information on sampling strategies would be highly irrelevant. Firstly, it

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is not useful for understanding the method itself, and secondly there appears to be no quantifiable relationship between foraging behaviour of these deep-diving top predators and primary producers in the surface waters.

Page 1248 / line 12: Is the standard deviation of the 10 meter averaged values was recorded by fluorometers. If yes, it should be represented on the graphs. In addition, without this indication nothing can be concluded about the existence of "true" Deep Fluorescence Maximum. It is possible that these maximums are just due to instrumental noise.

The Standard Deviation of the 10m binned depths is unfortunately not recorded by the instrument. However, we disagree that nothing can be concluded about 'true' DFM without this information. As explained by Xing et al. (2011), raw vertical fluorescence data is too noisy and must be smoothed using a median filter with a moving window of 7 points. The weighted, binned data that we use is essentially already smoothed on-board the tag before being relayed to satellite.

Page 1249 / line 3-20: Is the choice of the method for determining Zeu so important when monthly composite are used? Why not use a larger spatial window for matchup? It would be more consistent with the monthly composites.

Yes, there is a difference between Morel and Lee's Zeu products as one is empirical and the other is based on IOPs. This is explained in the methods and also detailed eloquently by Shang et al. (2010). The method has been amended based on the insightful comments of reviewer 2 and is now generated using a combination of 8-day and monthly values.

Page 1259 / line 1: To my mind, the discussion about the choice of density criteria for MLD should be centered on the concept of "mixed layer" and "mixing layer".

Firstly, the MLD method is not our method and we seek only to improve on the excellent work that Xing et al. has already published. Secondly, without in situ data on

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turbulence, it would be remiss for us to make assumptions about mixing layers. Thirdly, motile phytoplankton have been shown to actively aggregate, despite turbulent mixing. We use in situ salinity and temperature data recorded by the tags to calculate a density-derived mixed layer (as per Xing et al.) and can thus delineate the upper layer of uniform density. Furthermore, light attenuation is 'only' affected by particulates (CDOM, phytoplankton) and distinguishing between the terms UML and 'mixing layer' is not necessarily relevant. However, your point is understood and appreciated, and we have added Sangra et al. (2014) and Durham et al. (2013) as additional reference material for clarification.

Page 1251 / lines 12-19: Without error-bars around fluorescence measurements (Figure S2 and Figure 3), anything cannot be concluded about the vertical complexity of the fluorescence profile.

Thank you, your point is valid. The error is used as a threshold, as now better detailed in the methods. For any profile to be flagged as a DFM, the deep yield with the error subtracted must be higher than the surface value with the error added. This error differs between tags.

Page 1251 / line 14: What is meant by "not homogeneously mixed"?

We mean: not homogeneously distributed within the upper mixed layer, as delineated by the density-derived MLD.

Page 1254 / line 7-18: This paragraph is confused. Authors explain why a deep chlorophyll maximum is not necessarily a deep biomass maximum but, line 7, they wrote "Not all DFM are DCM". So the concepts of fluorescence, chlorophyll and biomass are mixed up.

Thank you, we have clarified this.

Interactive comment on Ocean Sci. Discuss., 11, 1243, 2014.