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Ocean Science Discussions

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

L. Biermann et al.

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We gratefully thank and acknowledge Referee #2 for their comments and suggestions. These have significantly improved and clarified parts of the manuscript. Detailed responses below:

Specific Comments

My main concern is your interpretation of a deep fluorescence maxima (DFM) as a deep chlorophyll maxima (DCM) driven by an increase in phytoplankton biomass. You mention in your discussion that not all DFM are a DCM. However I think more to the point is that not all DCM are biomass driven. One would expect that the light history of deeply mixed phytoplankton would result in adjustments to their chlorophyll content

and or cross sectional surface area such that their fluorescence signal would increase relative to the same population / biomass found in shallower waters. In this instance the DCM would be the result of a physiological response of the phytoplankton to low light at depth rather than the result of increased growth rates in response to nutrient (Fe) relief.

We realise that throughout the original manuscript, we were not clear on the distinction between biomass-driven DCM and packing-driven DCM. Thank you for your comments on this, as the changes you have suggested significantly improve this work. Both the introduction and discussion have been edited accordingly, and we feel the distinction between the two types of DFM (and thus DCM) is now clearer.

On page 1254 you discuss that deep mixed layers can create a DFM that is independent of biomass. This is true also of a DCM as the cells can adapt to low light environments by increasing their cellular chlorophyll content. You go on to say that you cannot confirm that the DFM observed are in fact biomass driven DCM. However you quickly move on to supply a number of references that support your hypothesis of a biomass driven DFM in the subantarctic. E.g. deep biomass maxima on the nutricline, heavily silicified diatoms accumulating in deep layers, abundant biomass of dinoflagellates below Eu.

Indeed, in part due to the ship-based studies done around our region of interest and in part due to the presence of regionally-persistent (between years) DFM in the night data (supplementary Fig. 4), these features might be interpreted as biomass-driven. However, we agree with your point here and we have moderated this paragraph to be more explicit about the uncertainty.

Your discussion states that if DFM were merely artefacts of chlorophyll packaging then we could reasonably expect maximum yields at depth to be more common, if not ubiquitous. But this is what we see. From all five profiles presented in figure 3, fluorescence is maximum below the Euphotic depth and above the mixed layer depth. To me the

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shape of these profiles are more suggestive of adaptation to light limitation driving an increase in fluorescence and cellular chlorophyll with depth than an increase in phytoplankton biomass.

We see DFM in only ∼30% of the full dataset, which is not ubiquitous (but is potentially regional). The five profiles that were chosen were selected as representative examples of distinct DFM. Whether they are due to Chl-a packaging or biomass is not possible to say for certain, but it is not impossible that they are the latter. This would be especially true for mixed assemblages, as the optimal gradient of light and nutrients is species-specific. We had included DFM from the night data as supplementary figures to support our interpretation that all not all DFM are due to packing effects (photoacclimation).

In addition, the three profiles with a shallower Eu (fig 3 a,b,c), i.e. those which would be expected to be more affected by light limitation by spending more time mixed out of the euphotic zone, show a bigger difference in the Eu versus MLD quenching correction. In other words surface fluorescence corrected by Eu when Eu is shallow gives lower surface values than MLD quenching corrected and thus drives a more frequent occurrence of a DFM. I think it may be worth investigating this further with a statistical analysis of whether or not shallow Eu's tend to drive DFM's rather than inherent vertical structure Light penetration is 'only' affected by water and phytoplankton in these waters.

Shallower Zeu are inherently representative of higher biomass and so do not 'generate' DFM. This is perhaps more evident in Fig.4. Please also keep in mind that fluorescence values in Fig. 3 are normalised (thus relative values), and not representative of actual yield (RFU).

I have additional concerns about the satellite derived Euphotic depth and in particular the impacts monthly averaged underestimates of Eu would have on this method of correcting quenching. However I agree on the use of existing products to make the correction more accessible. I was wondering whether it would be better/ possible to

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use the 8 day product where available and then fill in the gaps with the monthly composites so that you preserve the sub-seasonal and sub-mesoscale variability as much as possible?

We completely agree and have improved the method by first using 8-day wherever possible and then filling in with monthly, as per your suggestions. Thank you.

You conclude in your discussion that "while we cannot confirm with certainty that these DFM are also DCM (i.e. bulk phytoplankton biomass settling at depths where both nutrients and light are sufficient), without insight into the physics and the phytoplankton dynamics in the region, it is likely that they are." I am afraid that I disagree. But the fact that I have a different interpretation to your data set than you do is not the point here. It may be that I am wrong. The point is that with the data at hand you cannot reliably interpret the DFM as being biomass driven and so you shouldn't.

There is literature supporting both arguments, and I fully respect your interpretation of my results. However, equally, I also cannot reliably interpret all DFM as being entirely driven by Chl-a packaging, thanks to ship-based studies done in the region and also night data. I do really respect your insights and comments, and I have moderated my writing to be more accommodating to both interpretations.

This does not mean that this research is not relevant and that your Eu quenching correction does not add value to the scientific community. As you say in your discussion this method of correction "conserves phytoplankton dynamics on the vertical scale which may provide useful insights into mixing and settling of different species or differences in chlorophyll packaging in the same species".

Thank you!

Your one seal from Marion was part of an initiative to find a DFM hypothesised to support a biomass driven DCM. My feelings are that with this in mind your conclusions are biased to support this hypothesis. I think it would be better to avoid such definitive con-

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clusions with regards to the DFM your data supports and instead focus on discussing all the different drivers of both a DFM and DCM with equal weight (i.e. community structure, biomass, light acclimatisation etc).

Your point is taken - we have removed the paragraph pertaining to the Marion seal.

I think that an additional way to visualise whether your Eu method of correcting is better than the MLD method is to add an additional plot to figure 3a-e of the nearest dark unquenched profile for each profile presented in figure a-e. If the nearest dark unquenched profile looks more similar in shape / vertical structure and gives a more similar surface chlorophyll concentration to your Eu quenching corrected profile this will further support your point.

Unfortunately, we cannot always rely on there being a corresponding night profile in space and time. However, we have now changed Fig. 3 to include an example.

Finally I think it might be a good idea to mention that given the limitations of your data it would be prudent to try to incorporate backscattering and PAR sensors on future autonomous platforms wherever possible.

In the introduction, we do state that other autonomous instruments like gliders are able to incorporate PAR and backscatter, and that this method is specific to those which 'only' collect fluorescence, salinity and temperature data in remote regions. Tagged seals are able to venture into regions of this hostile ocean that ships, gliders and floats cannot. However, it's unlikely that we would be able to tag marine mammals with a veritable array of sensors without crossing ethical lines!

Also I would recommend in your paper that scientists wishing to use this method on future data sets do both the MLD and Eu quenching corrections to compare the outputs and also do sensitivity analysis on the criteria chosen to depict the MLD.

We agree and we have done so for stratified conditions, when such a step would be most useful.

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Technical corrections

Pg 1252, ln 9-11. I would suggest moving this sentence up in the text so that uncorrected fluorescence is presented in the text before the corrected data. This has been done.

Pg 1252, ln 13: appears to be (insert be) The missing 'be' has been inserted, thank you.

Pg 1252, ln 16: "ensuring fluorescence yield is representative of phytoplankton abundance" be careful not to say this. Even if you accurately correct for fluorescence quenching you are not able to ensure that the fluorescence yield is representative of phytoplankton abundance! Agreed. Sentence changed.

Pg 1253 ln 13: unlikely to be true (add to) The missing 'to' has been inserted, thank you.

Pg 1253 ln 18 rather than the use (add the) As suggested, 'the' has been added.

Pg 1253 ln 21: Change to "DCM features may occur" This sentence has been altered.

Pg 1254 ln 5: change to "and it is unlikely that they are result of errors arising from the correction method" This sentence has been altered.

Pg 1254 ln 7: Change to "However, not all DFM are DCM" For purely pedantic reasons, I'm uncomfortable starting a paragraph with 'however'. The sentence itself has been changed as a compromise.

Pg 1254 ln 21: delete also The word 'also' has been deleted in line 26.

Pg 1254 ln 29: at what time of year were these high latitude measurements made? The month of January has been added here.

Pg 1255 ln 3: the euphotic depth (add the) Again, for purely pedantic reasons, I respectfully don't agree with the addition of 'the' here.

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Pg 1255 ln 24: Change to “where DCM features may occur” This sentence has been altered.

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