

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

L. Biermann et al.

lb66@st-andrews.ac.uk

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We thank Referee #3 for their review of our method paper, but regret that some comments tended to be opinions and not constructive criticisms (i.e.: not adhering to point 7 of General Obligations for Referees). The two major criticisms outlined by this review are a lack of direction within the manuscript and not delivering a flow chart of the procedure presented. The other two reviewers do not have this problem. Indeed, both reviewer 1 and 2 clearly summarize the aim of the manuscript and also understand the procedure used in correcting quenched fluorescence. Nevertheless, we have reworked the text to make parts of the manuscript clearer. Detailed answers to review #3 follow below.

Major comments and questions:

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1) What is the major question or problem that the authors want to address in this MS? I do not see any question or problem stated by the authors. Instead it seems to be merely a discussion of the possible use of some remotely sensed data that they have acquired.

The authors present an improved method to correct fluorescence quenching, as stated in the title. The introduction also states that the existing method based on MLD masks potential deep fluorescence maxima and vertical dynamics within the upper mixed layer. The method we introduce attempts to address this problem. This manuscript also does not discuss the possible use of “some remotely sensed data”, but uses freely available remotely sensed satellite data and in-situ fluorescence data. We have altered the text in some places to make this clearer (see comments to reviewer #1 and #2)

2) What exactly is the “optimised method” that the authors put forward in this paper? I see no equations or description of any method, other than to repeat the laborious and expensive remotely acquired data as as they hav done.

We are not sure what the reviewer means here. Why does a method need an equation? Section 2.3 of the manuscript clearly describes the method used to correct for quenching based on Z_{eu} . We are not sure what is meant in the last part of the comment. Research methods are often laborious to achieve the best data quality. However, we do not think that our presented method is in any way laborious and the remotely sensed data is freely available thanks to a range of funders. Nevertheless, we clarified passages in the manuscript based on the more constructive comments of reviewers #1 and #2.

3) What is the major goal of this MS? From most of the pages in this MS, it seems that the authors see this ‘method’ as being of prime importance to measure chl-a in deep water columns and then somehow to related this to rates of primary production to be used in studies of food web dynamics, etc.

Again, as stated in the manuscript, the major goal of this manuscript is to present

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an alternative method to correct quenched in-situ fluorescence data. Reviewers #1 and #2 clearly recognise and state this major goal in their reviews. The method we present is NOT "of prime importance to measure chl-a in deep water columns and then somehow to related this to rates of primary production to be used in studies of food web dynamics, etc". It is, however, useful when measuring in-situ fluorescence collected during periods of high light intensity (day). We do not attempt to quantify [Chl-a] and nowhere do we relate Chl-a to rates of primary production. Nevertheless, reviewer #3 is correct that better knowledge of fluorescence in the ocean would help us to better understand primary productivity, which we had acknowledged with references in the introduction.

4) I find it a bit shocking that the authors come from four well known oceanographic institutions which should have given them the opportunity to get some ship time in Antarctic waters.....which would have enabled to obtain reliable, direct ship-board data in deep ocean profiles for essential physical, chemical, biological, and optical conditions (including direct measurement of chl-a, chl-a fluorescence, particulate organic carbon, etc.).

This comment does not address the manuscript, but we thank the reviewer for this compliment. The THREE BIOLOGICAL institutions involved in this work are very glad to hear that they are well-known. We also find it shocking that our work did not receive unconditional funding. However, as with all other biological and oceanographic institutions, we have to bid for ship time and funding. Fortunately, a range of innovative platforms are now available to collect a range of data from our oceans, without being limited to narrow transects in time and space, or fairer sampling conditions in the more 'hostile' oceans (seasonal bias). However, reviewer #3 does not appear to agree that data collected by tagged southern elephant seals and other autonomous platforms is reliable, relative to data collected from ship-based work. It is not within our scope to address this opinion, but we respectfully refer the reviewer to the work we reference in the introduction and discussion, as well as:

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a. Wunsch et al. 2009. The Global General Circulation of the Ocean Estimated by the ECCO-Consortium Oceanography, vol. 22, pp. 88.

b. Roquet et al. 2013. Estimates of the Southern Ocean general circulation improved by animal-borne instruments, Geophysical Research Letters, 40 (23), pp. 6176-6180. doi: 10.1002/2013GL058304.

We do agree with reviewer #3 that simultaneous measurements of a range of ocean properties are beneficial, but they are also not essential for this method.

5) In the Discussion, the authors state that their aim is "to create an algorithm that is easily applied...." Why don't they show us this algorithm?

An algorithm is a step-by-step procedure or method for solving a problem, which can be found in the methods section of the manuscript. Maybe the reviewer #3 uses the very narrow definition of an algorithm which is used in computer science. However, we think that a flow chart or the actual code is unnecessary to implement the procedure. In order to prevent any possible confusion or misunderstanding, we have changed the word 'algorithm' to 'method'.

Minor comments and questions:

1) The authors keep mentioning the 1% surface value for light. Do they mean the PAR solar irradiance incident upon the sea surface, or the irradiance immediately below the sea surface? They must realize that there is a big difference between these two values, particularly in high-latitude waters.

We have amended the text accordingly to be more clear.

2) In the Abstract, 4th liner, the authors mention the "in situ yield".....yield of what?

Fluorescence. We have altered the abstract to make this as clear as possible.

3) It does not appear that the authors are very well versed re published data from ship board work in the Antarctic. They show relatively little knowledge of the characteristics

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of the upper mixed layer or the usual depth of the euphotic zone (defined as 1% of the incident solar radiation) or of the basic characteristics of DCMs in regard to distribution or formation and maintenance of the elevated biomass at these depths. Also, DCMs are not 'rare' in the Southern Ocean or in vast regions of temperate and tropical waters.

We would be grateful if Reviewer #3 could share their knowledge or give some references supporting these statements (additional to "published data from ship board work in the Antarctic" that we already referenced and discussed in detail), so that we can add this information to our manuscript.

4) I do not like to see all this work go to waste.....have the authors ever considered mounting a good sea-going effort to combine actual profiles from ship-board studies to build upon and enhance the value of their thoughts and ideas as discussed in this MS?

We are not sure how this comment relates to the manuscript. However, we not only considered this suggestion, but put a proposal forward asking for funds to support such an undertaking. More ship-based data as well as data collected by autonomous instruments would give valuable information about the connections between fluorescence, Chl-a, biomass and primary productivity. However, we do not see how additional data would improve the method presented here.

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