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

Anonymous Referee #1

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This manuscript presents an adaptation of the "Xing et al., 2012" method for non photochemical quenching (NPQ) correction. Xing et al., (2012) assumes that, in the mixed layer, chlorophyll concentration is homogeneous and it proposes to extrapolate up to surface the highest value of chlorophyll concentration encountered in the mixed layer. In this manuscript, authors supposed that subsurface chlorophyll maxima may be present in the mixed layer and because the quenching correction method of Xing et al. (2012) may destroy these features, authors proposed to use the euphotic depth as a threshold instead of mixed layer depth. This adaptation is presented on fluorescence profiles derived from animal-borne fluorometers which operated in the Southern Ocean.

Although the figure 2 of this manuscript displays interesting results about NPQ, this

manuscript does not significantly contribute to scientific progress. 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. In addition, although night profiles are available, there is no tentative to validate the method proposed here.

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.

Page 1246 / lines 19-25: The limits of the Xing et al. (2012) method should be presented here. The reference for the method is Xing et al. (2012) not Xing et al. (2011).

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.

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.

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.

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.

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”.

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.

Page 1251 / line 14: What is meant by “not homogenously mixed”?

Page 1254 / line7-18: This paragraph is confused. Authors explain why a deep chlorophyll maximum is not necessary 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.

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