**Ocean Science
Discussions**

Interactive comment on "From the chlorophyll a in the surface layer to its vertical profile: a Greenland Sea relationship for satellite applications" *by* A. Cherkasheva et al.

Anonymous Referee #1

Received and published: 3 January 2013

Overall Comments: This analysis is important in terms of our ability to retrieve biomass and primary production within Arctic waters. Overall this is a good paper, although a bit short. Throughout there are many awkward sentences, I suggest the authors enlist a native English speaker to read the manuscript. In order to make this work significant the authors need to look into the literature and use their data to make an estimate of how much primary production the SCM may be contributing and how this affects the bottom line of PP retrieval. Right now this last step is missing. The attempt to draw seasonal patterns from this data is also not quite right. The authors have separated the profiles into categories based on surface CHL, this really represents different phases of phytoplankton growth, so looking at monthly profiles within each of these categories

is not the same as a seasonal cycle of SCM. I would suggest that the authors look at the profiles within a specific region to determine seasonal patterns.

The authors should be made aware that the ARCSS-PP dataset is now published in Progress in Oceanography, and change their references accordingly.

Minor Corrections

1. Page 3570 line 9. The Matrai et al 2010 paper is now published in Progress In Oceanography (http://www.sciencedirect.com/science/article/pii/S007966111200170X), as well as a companion paper detailing the vertical profiles found in the entire ARCSS_PP database. (http://www.sciencedirect.com/science/article/pii/S0079661112001711; Hill et al Synthesis of integrated primary production in the Arctic Ocean: II. In situ and remotely sensed estimates)

2. Page 3574 line 12. Do a statistical test between the slopes in Fig 1 in this study and Morel. Do not simply state that they are "similar".

3. Page 3574 line 18. You say that the clear relationship between Cpd and Ctot shows a mathematical dependency that can be expected. Although I am sure your r2 for this relationship would be high, the use of this regression would result in errors of several orders of magnitude at the low end of Cpd. Therefore you cannot use it and expect an accurate Ctot. I would remove this sentence and discuss further in the conclusion.

4. Page 3575 line 15. I would argue that your figure 3D, might not be showing a seasonal cycle from higher to lower Cpd, as these profiles could be occurring in different regions of the Greenland sea with differing nutrient or physical conditions. Therefore to say that it is a seasonal cycle is not really true. It may also be easier to show this data as a separate figure for each month instead of for each Cpd range.

5. Page 3576 line 1. I would also like to see Ctot for each range in Table 1. This would show us whether there is more CHL in profiles with SCM or not, important when

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considering its effect on satellite PP. Again in line 10, putting the actual % of SCM relative to Cpd would help with your agreement. Actually looking at Cpd versus Ctot would give you a value for how much CHL you would miss from a satellite.

6. Page 3576 line 15. When talking about seasonal cycles it would help to show the monthly profiles for a particular region which is heavily sampled. 7. Page 3576 line 27 "Sticking" to the surface. Please rewrite this!

Major Corrections

1. Page 3572 line 11. Concerning estimating the depth of the euphotic layer. Please describe this method here, do not make the reader go and find the reference to see how you did it. If some of these profiles have a measured 1% light level associated with them I would like to see the correlation between this and the Morel equations. In order to use this you need to convince me that it is accurate for the Greenland Sea as phytoplankton specific absorption could change this relationship, plus CHL is not the only factor in light absorption determining Zeu.

2. Page 3572 line 14. Concerning the calculation of Zpd, I was initially confused about Zeu/4.6 until I realized that you were calculating the first optical depth. You should be clearer here, you can also reference Gordon and Morel 1983 (Remote Assessment of Ocean Color for Interpretation of Satellite Visible Imagery – A Review) as well.

3. Page 3577 line 7. Your first statement here does not agree with your results. Yes, you do find a relationship between Ctot and Cpd, however you also state that there is a lot of scatter at the low end of Cpd, which means that you cannot predict Ctot. Your profiles also back this up, with significant SCM's. So sure you can use the Ctot vs Cpd relationship but you will not have accurate retrievals of Ctot in the presence of SCMs. Your conclusions are that SCM are prevalent and then state how much CHL the satellite is missing by only seeing Cpd. To make this relevant you need to make a statement about how this will affect the retrieval of PP as that is the reason you give for this analysis in the introduction.

Comments on figures

Figure 3. If you add the ranges onto each of the individual figures it would help the reader.

Interactive comment on Ocean Sci. Discuss., 9, 3567, 2012.