Final Author response: os-2012-97: From the chlorophyll a in the surface layer to its vertical profile: A Greenland Sea relationship for satellite applications. Cherkasheva, A., A. Bracher, E.-M. Noethig, E. Bauerfeind, C. Melsheimer

Dear Jill Schwarz and Anonymous Referee,

We thank you for your positive review and the helpful and critical comments. We appreciate the time and effort you invested into improving our manuscript. In the following, first, the referee's comments are stated in italics, then an answer to each comment is stated and the changes that were made to the manuscript are highlightened.

Kind regards,
Alexandra Cherkasheva and Co-authors

Detailed response to Anonymous Referee #1

General comments

1. Throughout there are many awkward sentences, I suggest the authors enlist a native English speaker to read the manuscript.
A native English speaker has read through and corrected the language issues in our manuscript.

2. 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 estimation of Greenland Sea primary production (PP) was the motivation for our study. We aim to implement the mathematical approximations of the CHL profiles obtained in the Antoine et al. (1996) model (Cherkasheva et al, in prep). Since primary production is not a trivial parameter to derive we intend to keep its detailed analysis separate. As recommended,we have included in section 3.5 a rough estimate of PP, mainly to study the underestimation in PP when the SCM is ignored.

3. 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.
Following your suggestion (in accordance to minor corrections #4 and #6) we have included in section 3.5 the analysis on a smaller region of the Greenland Sea (the “Hausgarten” area) where the sampling density was the highest. The analysis of the profiles of the whole Greenland Sea dataset has also been included in the same section. Such monthly profiles are not resolved in terms of surface CHL.

4. The authors should be made aware that the ARCSS-PP dataset is now published in Progress in Oceanography, and change their references accordingly.
The reference to the ARCSS-PP database was updated in section 2.1.

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).*
The correct references were added accordingly.

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"."
The slopes have been compared in section 3.1, paragraph 2. Unfortunately, we do not have access to the Morel and Berthon (1989) data to examine the error bars of the slopes. According to J.-F. Berthon (personal communication), the data were kept on a magnetic tape, which could only be read on an old HP computer that is not available anymore.

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.
The sentence "The clear relationship between Cpd and Ctot for the Greenland Sea proved that a mathematical dependency between these two parameters can be expected." has been removed. The accuracy of estimating Ctot at the low end of Cpd is now discussed in the conclusion in the paragraph 3.

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.
Figures 3, 4 and A1 are linked to each other and therefore we aim to keep them the way they are. In order to show the seasonal cycle we have added a figure with monthly profiles from the highly sampled smaller region ("Hausgarten") as well as from the whole Greenland Sea (Figure 6).

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 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.
Accordingly, Ctot for each range and the percentage of SCM relative to Cpd have been added in Table 1.

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.
As stated above (minor corrections #4 and general comments #3) we have also included the analysis on the "Hausgarten" region only and a figure with monthly profiles from this sub-region.

7. Page 3576 line 27 "Sticking" to the surface. Please rewrite this!.

Accordingly, the sentence has been rewritten to “maxima of the profiles gradually moved from greater depths in spring towards the surface in September”

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.

A detailed description of the method to estimate the euphotic layer depth has been added to section 2.3. The ARCSS-PP database does not contain any collocated light and chlorophyll profile measurements for the Greenland Sea. From our own database, only nine profiles containing both parameters were available. The Zeu calculated for them was situated roughly in the same range. Because of the limited number of collocations, instead of the regression coefficient the relative error was calculated (23%). This result gives us the confidence to use the Morel (1988) assumption because as we now discussed at the end of section 2.3, its error is comparable to other studies which compared the results of the two above mentioned estimations of Zeu. The nine light profiles with the corresponding CHL profiles, and Zeu calculated as 1% of the surface PAR and as of Morel (1988) are given in Figure A1.

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.

Accordingly, we have now added the information on the penetration depth “defined as optical depth at which the downwelling irradiance falls to 1/e of its value just below the surface” from Gordon and Morel (1983).

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.

In order to clarify the accuracy of retrieving Ctot in the lower end of Cpd, we have performed an error analysis (see the end of section 2.5 for the method and section 3.5 and Table 2 for results). The following text was added to the conclusion (paragraph 3): “The histogram of Cpd revealed that the majority of profiles have low CHL values in the surface layer. We have also observed that in the Greenland Sea, low surface CHL values correspond to a larger range of total CHL than globally (Figure 1), and that the estimation of total CHL from Cpd is less reliable here. The reason for this is that the values and positions of Subsurface Chlorophyll Maximum are variable. For the profiles with low surface CHL, Subsurface Chlorophyll Maximum is significant when related to Cpd. The error analysis for the profiles with low surface CHL ($Cpd<0.3 mgC/m^3$) showed that the use of the Gaussian parametrization, instead of the uniform CHL profile, reduced the underestimation of total CHL on average from 19% to 6%. At the same time, errors in the rough estimates of primary production reduced on average from 10% to 3%”

Comments on figures

If you add the ranges onto each of the individual figures it would help the reader.
Accordingly, the numbers of Cpd ranges were added to Figures 3,4 and A1

References

1. Antoine, D. and A. Morel (1996): Oceanic primary production: I. Adaptation of a spectral light-photosynthesis model in view of application to satellite chlorophyll observations, Global Biogeochemical Cycles, 10, 43-55.
2. Antoine D., André J.M. and A. Morel (1996): Oceanic primary production: II. Estimation at global scale from satellite (Coastal Zone Color Scanner) chlorophyll, Global Biogeochemical Cycles, 10, 57-69
3. Cherkasheva, A., Koeberle, C., Gerdes, R, Antoine, D., Gentili, B., and Bracher, A. (in prep): Greenland Sea primary production and its relation to the physical environment

Detailed response to Jill Schwarz

General comments

1. The value of the paper would be greatly enhanced if a little more information about the dataset were provided, including a map of profile locations, with some representation of the months during which biogeographically distinctive areas were profiled. This would help constrain the interpretation of the data in terms of surface chlorophyll variability/seasonality and nutrient/mixed layer regimes (e.g. in Section 4.1). A map of the categories assigned would likewise aid interpretation – does the high CHL category always occur to the edge/centre of the basin?

We have included the maps of the profile locations indicating the month when the data were sampled (Figure 3 left), as well as the map of the categories assigned (Figure 3 right). The categories map was underlayed with the sea surface temperature climatology for 2002-2012.

2. A graph showing surface chlorophyll against penetration depth would likewise be helpful for interpretation.

A graph showing chlorophyll in the surface layer (Cpd) against penetration depth has been added (Figure A3) and is discussed in the end of section 3.3.

3. Some statements about the results need to be qualified for accuracy (see detailed remarks below),

We have improved the analysis and the discussion of our results as highlighted in the answers to referee#1's comments (general comments #3, minor corrections #3, #4 and major corrections #3) and also as pointed out below in the answers to the detailed remarks.

4. the English needs to be corrected (see grammatical/typographical corrections below).

As also recommended by referee#1, a native English speaker has read through and corrected the language issues in our manuscript.

Detailed Remarks:

1. Page 3569, Line 4: Was any error calculation made, comparing the production estimated by some simple, standard PP model using the refined profiles vs estimation using Morel & Berthon (1989), for example? Although I realise it may be the subject of a future paper, a preliminary, indicative calculation and error budget would contribute significantly to this paper, especially given the degree of within-group variability that you found (Figure A1).

The error calculation has been made for Ctot, as well as for the rough estimate of primary production (Eppley, 1985). Calculations were applied to the in-situ profiles and then compared with those of: 1) uniform profiles with keeping the surface value constant; 2) profiles derived following Morel and Berthon (1989); 3) the Gaussian profiles which were produced in our study. Refer to section 3.5 and Table 2 for the results of the error calculation.

2. Page 3571, Line 3: It would be extremely helpful to include a map of the input data for this study. The introduction mentions sea ice, yet April and June profiles suggest deep maxima rather than ice margin blooms; did the study include profiles within the ice zone? Although you provide a reference to Matrai et al. (2010), that paper does not appear to have been published yet. Similarly, at Line 15, it would be helpful to report whether the surface values in the profile dataset spanned the same range of chlorophyll-a values as the underway dataset. These minor alterations would give the reader a clearer idea of how representative your data are of the Greenland Sea (and of arctic ecosystems generally).

As answered above in comment #1, the maps of the input data have been included (Figure 3) and discussed in section 3.2 paragraph 2. Our database includes the profiles from the sea ice zone, which are mostly the profiles of the lowest Cpd category. The reference to Matrai et al. has been updated (section 2.1, paragraph 2). A histogram of the underway dataset compared to the profile data set has been attached to the current document (Figure 1). The samples that were considered for underway dataset are single measurements (and not profiles) for depths shallower than 15 m from both our dataset and ARCSS-PP database. The data points with low surface CHL are not as clearly dominant in the underway dataset as in the profile dataset. The samples are, however, still concentrated in the low CHL area (< 1mgC/m^3). The mean value of the underway dataset is 1.31 mgC/m^3, while the mean Cpd of the profile dataset is lower, 1.04 mgC/m^3. The underway data are also more variable, with a standard deviation of 1.48 mgC/m^3 as opposed to 1.04 mgC/m^3 in the profile dataset. The underway dataset consists of 333 data points only, which is significantly less than the profiles dataset (N=1199). We thus prefer not to include this information in the manuscript.

3. Page 3573, Line 4 & Page 3574, Line 16: Do I understand this method of choosing the bounds of eachcategory correctly: You ordered the profile by surface chlorophyll concentration, then divided them into equally sized bins, placing the bounds automatically every ∼200 profiles, so the category boundaries emerge as the spacing between your chosen number of bins (categories) ? It's useful that you report alternative approaches (lines 13 to 17), but what is the result of relating Csurf against depth of chlorophyll maximum – would this provide a more immediate approach to answering your main question? Again,some further exploration of variability seems appropriate given the degree of spread inFigure A1. Was the dataset checked for heteroscedasticity?

You have correctly understood the method applied. A relation of Csurf to CHL maximum would have been a more simple solution than the method we had chosen. However, it is actually included in our method, as two of the variable parameters of the Gaussian are the offset and magnitude of the maximum. The approach of taking these two parameters only would not account for the spread of the maximum and seasonal change of the profiles, the parameters highly variable within our dataset. As suggested by you, we have further explored the variability using the heteroscedasticity test (by White (1980)). We have identified by the test, that the month was the only parameter causing the change in the Cpd vs Ctot variability (with p=0.05). This supports our choice of categorizing the data by season. Other predictors tested were latitude, longitude, year, euphotic layer depth and penetration depth. For all of them the null hypothesis of heteroscedasticity was accepted at the confidence level of 0.05. This information has been added to section 3.1 paragraph 2

4. Page 3574, Line 20 & Page 3577, Line 6: "Whilst the correlation between Cpd and Ctot is certainly significant for the full dataset, values of Cpd <∼ 0.7 may indicate Ctot anywhere within an order of magnitude of Cpd, and vice versa. I suggest qualifying this statement, perhaps with reference to confidence bounds which could be added to Figure 1.

The sentence "Based on the clear correlation of Cpd to Ctot for the Greenland Sea, it is in principle possible to estimate the CHL in the whole water column based on the CHL in the surface layer only" has been deleted. The discussion on the reliability of Cpd-Ctot relationship in the lower part of Ctot values has been added to the conclusion (see paragraph 3). The confidence bounds have been added to Figure 1.

5. Page 3575, Line 10: I strongly recommend including the supplementary Figure A1 in the main text, since it enables the reader to gauge much more clearly the variability in the data. I would also recommend including information about the median/std. Dev. penetration depth values for each category, perhaps as horizontal lines on Figure A1 or within Table 1.

The supplementary Figure A1 has been included in the main text as Figure 5. The information on the penetration depth has been added to the Table 1 and discussed in Section 3.3.

6. Page 3577, Line 20 to Page 3578, Line 17: "It could be inferred from the beginning of this argument that salinity stratification drives mixed layer depth throughout the Greenland Sea, which is misleading. Thermal stratification dominates in the central basin away from the seasonal ice zone and East Greenland Current, and these two zones (ice zone and always ice free) represent quite distinct biogeochemical regimes. This should be made more clear on Page 3578. The interpretation of profiles within the rest of the paragraph needs to consider the known domain of the profiles explicitly – the co-ordinates of the profiles are known, so even a simple map of category locations would help here, and if you are prepared to invest more time in it, then overlaying ten-year mean monthly surface SST contours, for example, might help. I am continually wondering whether you observe geographical coherence in your categorisation.

The statement on the two different zones within the Greenland Sea (ice affected and always ice free) has been added to section 4.1 paragraph 1. The map with the locations of the categories (Figure 3 left) showed that south of 74°N, in the area of the warm Atlantic waters, the category with the highest surface CHL (Cpd>0.7 mgC/m^3) is prevalent. On the other hand, in the north-west part of the basin, the area affected by sea ice at the Greenland shelf, the category with lowest surface CHL (Cpd<0.3 mgC/m^3) occurs more often than others. These two categories, however, also appear throughout the basin. The other two intermediate Cpd categories are spatially more evenly distributed. The location map was underlayed with the sea surface temperature climatology for 2002-2012 (Figure 3 left).

7. Page 3578, Line 278: "The phytoplankton bloom begins during March/April at the seasonal ice zone, and later in the open Greenland Sea. To interpret your observation of April peak CHL mixes up the lateral with the temporal variability – you need to clarify the location of sites with an April CHL peak in order to decide whether your dataset concurs with Behrenfeld's (controversial on account of semantics) interpretation. It's not at all clear to me from what you currently show in the paper that you have sufficient temporal resolution within any year and biogeographical domain to draw firmconclusions about bloom timing.

The map with the locations of the profiles (Figure 3) showed that the majority of the April samples are concentrated in the Eastern Fram Strait (78°N-80°N, 5°W-10°E). This area is north to the area Behrenfeld (2010) studied (40°N-65°N) and thus does not represent the same seasonal cycle. This is now discussed in section 4.1. Nevertheless, this area was a partly ice free zone in April 2003, when most of the samples were collected (see Figure 2 in this document). For the seasonal variability analysis we added the monthly median profiles averaged for all the Cpd ranges and alternatively the profiles for the small but highly sampled region at "Hausgarten" (77°N-82°N, 5°W-10°E), in order to minimize the effect of spatial differences on the data (Figure 6).

The listed minor/typographic errors have been corrected.

References

1. Eppley, R., Steward, E., Abbott, M., Heyman, U. 1985. Estimating ocean primary production from satellite chlorophyll: introduction to regional differences and statistics for the Southern California Bight. Journal of Plankton Research 7, 57–70
2. White, H. 1980. A heteroskedasticity-consistent covariance matrix estimator and a direct test for heteroskedasticity. Econometrica 48 (4): 817–838. doi:10.2307/1912934. JSTOR 1912934.

Figures

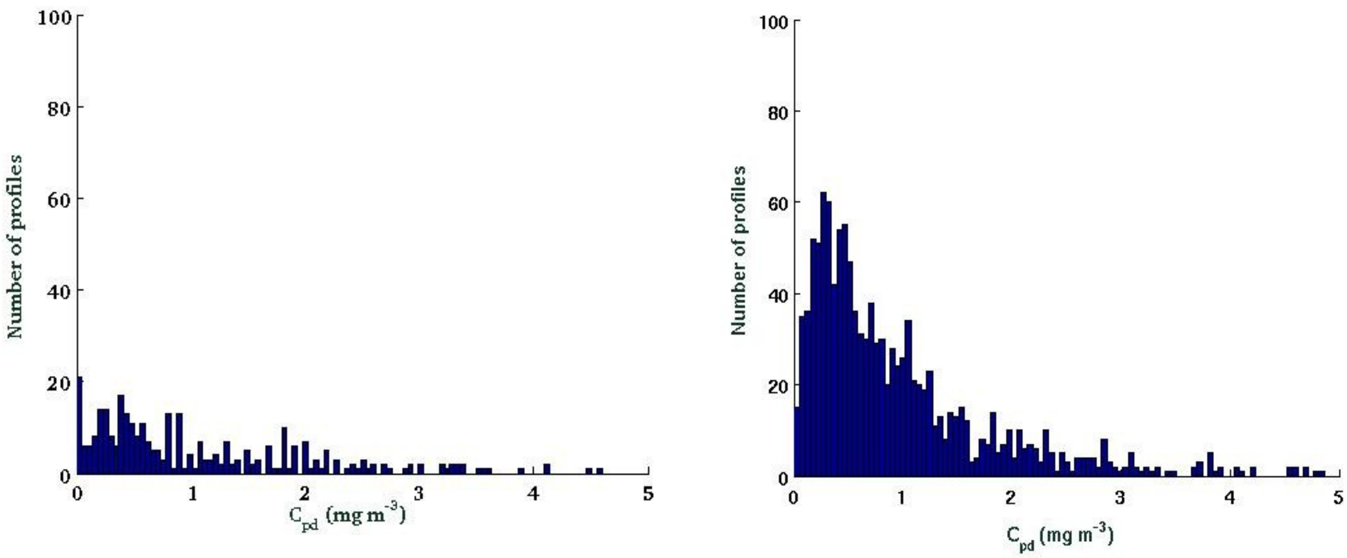

Figure 1. Left: histogram of CHL for the underway dataset (depths 0-15m). Right: histogram of Cpd for the CHL profiles dataset (which is Figure 2 in the manuscript).

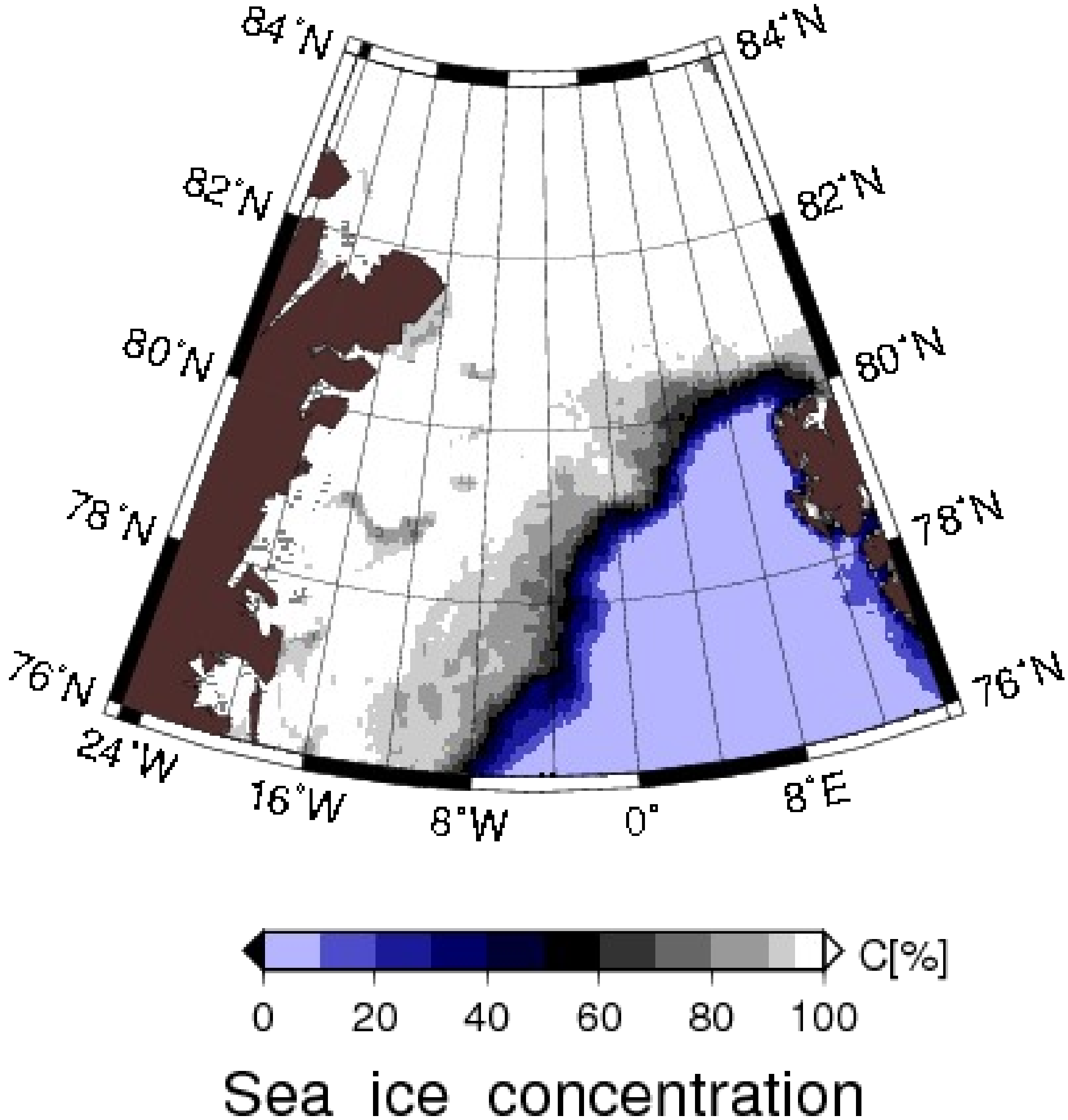

Figure 2. Satellite-based sea ice concentration for April 2003. Data obtained from the PHAROS Group University of Bremen, retrieved using AMSR-E with a 6.25 km resolution.