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Interactive comment on “Remote sensing of size structure of phytoplankton communities using optical properties of the Chukchi and Bering Sea shelf region” by A. Fujiwara et al.

Anonymous Referee #1

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A. Fujiwara et al. discuss the results from applying a novel method for determining the phytoplankton size structure (cell $> 5\mu\text{m}$ and cells $< 5\mu\text{m}$) using optical properties from the Chukchi and Bering Sea shelf region. Unlike previous models that retrieve phytoplankton community structure using satellite data, the proposed model uses the shape of the phytoplankton absorption coefficient and the spectral slope of the particulate backscattering coefficient.

This manuscript addresses a relevant scientific question within the scope of BG and to a broad cross-section of the oceanography community. The size structure of phytoplankton influences certain aspects of the ocean biogeochemical cycle, such as nu-

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trient uptake, sinking rates and export. There are established connections between the size structure of phytoplankton and the marine food web and areas of high fish production. As highlighted by the authors, the Chukchi and Bering Sea shelf region are thought to be highly vulnerable to climate change, and therefore, monitoring the size-structure of phytoplankton using remote sensing has the potential to indicate corresponding ecosystem change. Furthermore, synoptic maps of phytoplankton size structure can be used for comparison with, or assimilation into biogeochemical models that use a similar size-class partitioning. Satellite remote sensing appears to be the only method currently capable of overcoming the sampling limitations of in situ data.

In general, the approach and results are interesting. The method is novel in the sense that it is combining two contrasting approaches, methods that use the spectral slope of the backscattering coefficient (e.g. Loisel et al. 2006; Montes-Hugo et al. 2008) and methods that use the shape of the phytoplankton absorption coefficient (e.g. Ciotti et al. 2006). There are advantages and disadvantages to using either of these approaches, as highlighted by the authors, suggesting results may improve if the two approaches were combined. However, below are some general and specific questions (or suggestions) that should be addressed before publication.

General Comments

Page 4992: Equation 2: Why do you use a high order polynomial equation for converting $>10\mu\text{m}$ chl-a to $>5\mu\text{m}$ chl-a. Does this equation give a significantly better fit than a linear equation? According to Equation 2, above 1.5 mg m^{-3} of $>10\mu\text{m}$ chl-a, the $>5\mu\text{m}$ chl-a begins to increase sharply as a function of $>10\mu\text{m}$ chl-a (is this realistic, or a feature of the high order polynomial equation?). If you use this equation I would be careful to clearly state the range of $>10\mu\text{m}$ chlorophyll-a it can be applied to (i.e. the range of data the model was fitted to), should others be inclined to use the equation.

Page 4998: Equation 8: While I find this equation elegant (I like the way FL is constrained to vary between 0-100%), as it is non-linear, I am concerned as to whether

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such a model should be applied directly to monthly averages of reflectance data from MODIS. Should the model first be applied to daily values then averaged over the month? Will this cause any differences?

Page 4998: Equation 8: It would be nice to provide some statistics which indicate that the performance of the model increases significantly when using both $\text{aph}(488)/\text{aph}(555)$ and γ as independent variables, and not just a single independent variable (e.g. $\text{aph}(488)/\text{aph}(555)$ or γ on its own?).

Page 4998: Equation 8: Is it possible to provide error estimates (or confidence intervals) on the parameters provided in Table 4. This could be very useful information should one need to run sensitivity analysis, and for additional error estimates.

Page 5001: Line 16: The authors refer to the satellite model as capable of retrieving FL independently from Chl-a. Whereas the model is fitted using in situ measurements of $\text{aph}(488)/\text{aph}(555)$ and γ , its application to satellite data is different. Firstly, the approach estimates γ using a blue-to-green reflectance band ratio (equation 6) yet the OC4L (equation 7) also uses a blue-to-green reflectance band ratio to derive chlorophyll-a. In fact the QAA, used to estimate $\text{aph}(488)/\text{aph}(555)$ from satellite, is also very dependent upon blue-to-green reflectance band ratios (see Lee et al. 2002 Tables 2 and 3). Therefore, it would be interesting to know exactly how independent the satellite estimates of $\text{aph}(488)/\text{aph}(555)$ and γ are from the OC4L chlorophyll-a. Can the authors provide some quantitative statistics on this? One approach could be to add to Figure 5 histograms of $\text{aph}(488)/\text{aph}(555)$ and γ , as such it may become clearer what is forcing the differences between FL and Chl-a, is it coming from differences in $\text{aph}(488)/\text{aph}(555)$ or γ or the mathematical formulation of Equation 8? Note that a recent paper by Vantrepotte et al. (2011) showed that the global seasonal cycles for γ and Chl-a have a similar pattern, and highlighted inter-annual similarities between γ and Chl-a in the global ocean. Is this different in the Arctic regions?

Page 5000: Line 4 (section 3.2 generally): Although perhaps beyond the scope

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of this paper, it would also be interesting to try different methods for determining $\text{aph}(488)/\text{aph}(555)$ and γ from satellite data, and use these as input to your model to see how this may influence your satellite retrievals of FL. Although, the authors would need to be careful to use IOP models that do not assume a spectral shape for aph . This information could be very useful for mapping errors from satellite data.

Technical Comments

Page 4986: Line 15-16: (also Page 4999: Line 11-12) Not sure what is meant by the following statement “A validation study demonstrated that the SDM successfully derived an FL value of 69 % within an error range of ± 20 % for unknown data”. Can the authors clarify what error statistical tests are being used (mean absolute error, standard error)? Can the authors clarify what is meant by “FL value of 69 %”? Is the mean value for in situ FL (Figure 3 x-axis) the same mean value for the modelled FL (Figure 3 y-axis) i.e. 69 %?

Page 4989: Line 1: Not all these approaches estimate the dominant distribution of PFTs, some (e.g. Uitz et al. 2006; Ciotti and Bricaud 2006; Mouw and Yoder, 2010 and Brewin et al. 2010) estimate the fractional contribution of PFTs to the total biomass. I would also suggest including the references Bracher et al. 2009, Kostadinov et al. 2009, Devred et al. 2011 and Hirata et al. 2011.

Page 4989: Line 13-14: The IOPs do not always vary with the PFT composition in the water. Some PFTs can have similar optical signatures making them especially hard to discriminate using only optical measurements (e.g. some Harmful Algal Blooms are difficult to determine using only optical data as they have similar optical signatures to other non-harmful phytoplankton).

Page 4989: Line 18: The structure of this sentence suggests that Morel and Prieur (1977) quantified relationships between IOPs and PFT composition in case 1 waters, whereas I think the authors are using this reference to refer to the case 1 bio-optical principle. If so, the sentence needs restructuring.

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Page 4989: Line 28: For clarity I would suggest removing “Then,” at the beginning of the sentence and start the sentence with “Montes-Hugo et al.” Also I would suggest including the reference Loisel et al. (2006), who also used the spectral slope of the backscattering as an index for small and large particles, though this was conducted globally, not specifically for the Western Arctic Peninsula region.

Page 4990: Line 2: For clarity I would suggest removing the word “bloom” at the end of the sentence.

Page 4990: Line 10: I would suggest changing “validated in the area. . .” to “validated or tuned using data in the area. . .”?

Page 4990: Line 18: I would suggest changing the words “satellite observation. . .” to “satellite data. . .”?

Page 4991: Line 2-3: I would suggest removing the words “discuss and”

Page 4993: Line 4: I would suggest removing the words “as IOPs”

Page 4995: Line 11: The authors mention that data from turbid waters were omitted, using a ratio of aNAP(443) to a(443). Are turbid waters also omitted from the satellite pixels?

Page 5013: Figure 1: Green circles should be correctly referred to (the caption refers to purple circles for KH09-4?).

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