

## ***Interactive comment on “Global variability of phytoplankton functional types from space: assessment via the particle size distribution” by T. S. Kostadinov et al.***

### **Anonymous Referee #1**

Received and published: 31 July 2010

#### GENERAL COMMENTS:

The manuscript leverages Kostadinov et al. 2009, describing trends in PSD in relation to SST and chl at specific time series sites and also globally considering time frames not significantly influenced by ENSO and then the entire record (first 10 years of the SeaWiFS mission). The authors take the opportunity to briefly describe the Kostadinov et al. 2009 method and state some very important assumptions of the approach. They present a validation of the Kostadinov et al. 2009 approach with the Vidussi et al. 2002 HPLC-based estimate of phytoplankton size and compare their PSD results with Alvain et al. 2008 and Uitz et al. 2006.

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The manuscript is very well written, clear and well organized and needs only minor clarification at specific points in the manuscript (described in the specific and technical comment sections). In spite of these strengths, my biggest reservation with the manuscript is there seems to be two mixed agendas: 1) to bolster confidence in the Kostadinov et al. 2009 approach through additional validation with the Vidussi et al. 2002 HPLC estimate of size and comparison to two other PFT approaches; 2) to describe PSD trends in comparison with SST and chl. While I think both of these aspects deserve publication, I am not sure that combining them into the same manuscript is the best approach. I suggest that the authors consider separating these aspects into two separate manuscripts and develop each aspect more fully. One on hand, the validation paper could compare the Kostadinov et al. 2009 approach more fully to additional PFT methods in the literature beyond just Uitz et al. 2006 and Alvain et al. 2008. On the other hand, it seems that the trend aspects could use more in depth investigation of the relationship between PSD and chlorophyll beyond just correlation analysis. The manuscript mentions connections to physiological state and also the need for further work to assess the sensitivity to size limits and the feasibility of operational choice of dynamic ranges regionally (paraphrased from page 4312), in addition to further work to carefully validate and study parallel trends in the PSD products and their uncertainties to address biomass changes (paraphrased from page 4318), among others also mentioned. These aspects could be developed in greater detail if the manuscripts were split, as suggested.

**SPECIFIC COMMENTS:**

Page 4302, lines 16-21 – Did you use the single 9 km pixel that contained the HPLC sample or another means of match-up (3x3 box, etc. . .)? Please clarify.

Page 4303, line 15 – it would be helpful if you could incorporate the geographical bounds of the regions you considered into a figure.

Page 4304, line 5-6 – the use of “inclusive” should be explicitly called out. I think what

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you are talking about is the use of data from 1997 – 2007, whereas data from 2000-2007 was considered separately due to the influence of ENSO. Please clearly define in sections 2.4 and 2.5 and find a term to refer to each timeframe of data.

Page 4304, lines 14-15 – How to you define satisfactory, good and poor?

Page 4304, lines 21-22 – “Namely, the SeaWiFS PFT retrievals showed a cluster of results near 45% ...”. This is not clear. Are you still talking about nanos? Why do you think this happens? This seems more like a ceiling in the retrieval. Elsewhere in the paper you mention a maximum of 51% for nanos, the figure looks to be clustering closer to 50% rather than 45%.

Page 4304, line 24 – “validation should be considered as satisfactory” While I agree with the points that you bring up here and in section 4.2, declaring the statement above seems like hand waving. I think the definition of satisfactory needs to be stated clearly and the aspects that lead to a “satisfactory” conclusion needs more explanation.

Page 4310, line 18 – Please indicate why the 30W and 140W meridional sections were selected. Why not a different location?

Page 4314, line 9 – “generally satisfactory” Again, please define following comments for pages 4304.

Page 4317, line 11 – you may want to also consider Gregg et al. 2005 in this discussion. Gregg et al. Recent trends in global ocean chlorophyll. Geophysical Research Letters (2005) vol. 32 (3) pp. L03606, doi:10.1029/2004GL021808.

Page 4317, lines 17-21 – Regarding the point brought up in these 2 sentences. . . I feel that this could be accomplished with here, but needs more through analysis!

Page 4318, line 1 – “units of per century” Why was century chosen? This is confusing and misleading.

Page 4318, line 18-20 – “Further work needs to be devoted to more careful validation

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and study of the parallel trends in the PSD products and their uncertainties in order to address this important issue.” What is stated as needing further work – isn’t this the point of the manuscript? This should be fully developed in the two manuscript approach that I suggest!

Figure 10 – I find this figure confusing. In a bar graph, one expects to compare the values between this study and Alvain et al. 2008 side-by-side. This doesn’t seem to be the case. It is confusing which values are Alvain and which are Kostadinov. Please find another way to present this idea/data.

#### TECHNICAL CORRECTIONS:

Page 4298, line 1 – there needs to be a better transition regarding the spatial difference between HPLC ship observations and satellite imagery.

Page 4300, lines 14-15 – the authors use “was then”, “were then” repetitively. Please reword the second occurrence.

Page 4301, line 10 – “very rare” Can this be defined quantitatively?

Page 4302, line 14 – NGDC ETOPO2 – should be defined

Page 4306, line 1-2 – “(note the same color scale is used).” I suggest adding “for all size classes” to the end of this statement.

Page 4308, line 15 – “first-order correspondence” Define what you mean.

Page 4308, line 13 – “North Atlantic Drift Providence” I assume this is one of Longhurst’s provinces, but you should probably reword to clarify. Each other use of Longhurst’s provinces was clearly attributed.

Page 4309, line 13 – “w/chl” Please spell out.

Page 4309, line 19 – “large differences” is used repetitively. Please find another way to state the second occurrence.

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Page 4309, line 22 – “Fe” use “iron” instead.

Page 4310, line 6 – “This fell to almost 0% . . .” I assume you are talking about microplankton here, but should state to be clear.

Page 4310, line 11 – degree symbol should be added to 3C and 8C

Page 4313, line 29 – missing “)”

Page 4314, line 1 – “This is an encouraging result. . .” Clearly state what you are referring to, low uncertainty?

Page 4315, lines 16-18 – The sentence “The x-axes. . .” should be in the figure caption not in the text.

Page 4315, line 24 – “(Fig. 11b)” I believe this should be Fig. 10b.

Page 4316, first paragraph – I believe all references to Fig. 12 should actually be Fig. 11.

Page 4317, lines 4-5 – Please reword this sentence. How can a warming be global if it is significant at only two locations? Remind the reader that significance is determined by the p-value.

Page 4317, line 29 – “decrease of -0.02/decade” Decrease of what? Particles? Please be explicit.

Page 4319, line 7 – state what years were considered for this analysis, it not clear if this is just 1997-1999 or 1997 – 2007.

Table 1 – why do you report trends per century? Why not choose something more in line with the length of your data record?

Table 2 – state years of analysis in the first line of the caption rather than the last line. Check tense of all sentences.

Figure 4 – “number concentration” This is a confusing term. How about just number of

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particles per m<sup>3</sup> as stated at the end of the caption.

Figure 7 – check tense in caption

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**BGD**

7, C2121–C2126, 2010

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