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Biogeosciences
Discussions

Interactive comment on "Photosynthetic parameters in the Beaufort Sea in relation to the phytoplankton community structure" *by* Y. Huot et al.

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General Comments:

In this paper, Huot and co-authors present a dataset of phytoplankton photosynthetic parameters collected in the Arctic Ocean (Beaufort Sea), and explore predictive relationships which describe their observed variability. The case is well-stated that the Arctic Ocean is undergoing rapid climate change, and remote assessment and monitoring of these changes relies on proper characterization of in situ variability. To this end, Arctic ecosystems have been underrepresented in field datasets used to develop ocean color based primary production models. The data presented here and the resul-

tant functional relationships derived from them seek to bridge this need. The paper is well written and the authors nicely compare their dataset with historical data collected in the region and point out similarities and differences.

I have only two points of "general" concern. First, there has historically been some uncertainty as to what exactly the 14C method measures. Most recently, Halsey et al. (2010; 2011) have presented some intriguing results showing that what the 14C method measures is dependent upon incubation duration and growth rate of the phytoplankton. In particular, typical short-term incubations ($\sim$1-3 hours) show something far from net primary production and much closer to gross primary production. One conclusion from that work is that we should avoid short-term 14C incubations entirely. Barring that, we must consider that the dataset employed in the present paper will have some significant error (>40%) which cannot be resolved without knowledge of corresponding growth rate of the natural phytoplankton assemblage. Some recognition and discussion of this needs to be presented.

Second, and perhaps more philosophically, what is the purpose of predicting Pmax_chl at depth? Cells below the depth of the mixed layer are generally light limited, right? That is, Pmax_chl is likely only realized by the phytoplankton inhabiting the surface mixed layer, right? And when mixed layers are deep, even that statement may not be true, particularly at extremely high latitudes where incident irradiance is generally low. If true, some further justification is needed for the primary goal of the paper. Perhaps I'm wrong here, but this could be addressed by evaluating the in-water irradiances relative to the estimated Ek's.

Specific Comments:

The point for distinguishing whether a water sample is "dominate" by microplankton or not is 0.65 thorughout the paper. Why are values shown in Figure 6 truncated at 0.3?

There are no reference to Figures 8B&C anywhere in the text.

Technical Corrections:

The paper is generally well-written and I did not find any technical problems.

References:

Halsey, K.H., Milligan, A., Behrenfeld, M.J. Physiological optimization underlies growth rate-independent chlorophyll-specific gross and net primary production. Photosynth. Res. doi: 10.1007/s11120-009-9526-z, 2010.

Halsey, K.H., Milligan, A.J., Behrenfeld, M.J. Linking Time-dependent Carbon-fixation Efficiencies in Dunaliella Tertiolecta (Chlorophyceae) to Underlying Metabolic Pathways.J. Phycol.. 47, 66-76, 2011

Interactive comment on Biogeosciences Discuss., 10, 1551, 2013.