Biogeosciences Discuss., 7, C3906–C3909, 2010
www.biogeosciences-discuss.net/7/C3906/2010/

Biogeosciences Discussions

BGD

7, C3906–C3909, 2010

Interactive Comment

Interactive comment on "Near-ubiquity of ice-edge blooms in the Arctic" *by* M. Perrette et al.

Anonymous Referee #1

Received and published: 19 November 2010

General Comments:

This work convincingly demonstrates that ice-edge bloom is nearly ubiquitous in the Arctic, a phenomenon well documented in Antarctica, but not so well in the Arctic if we exclude the Barents and Bering Seas. In fact, it has never been documented in many arctic regions (e.g. most of the canadian archipelago). It is based on a careful, and original, analysis of satellite ocean color and sea ice data collected over the Arctic Ocean. The authors are also aware of some inherent limitations of ocean color data (but not all) and discussed some of them. Although the PP model used is certainly not appropriate for the Arctic Ocean (Behrendfeld & Falkowski 1997), the results are not over-interpreted. They found that ice-edge bloom might be accounting for more than a half of the total annual primary productivity (PP) of the Arctic Ocean calculated using satellite Ocean Color data. This might be overestimate because of the sub-surface production that follows the bloom and the production prior the ice melting. Nevertheless the paper indicates (to some extent) that time resolution may be important to consider when PP may be computed from Ocean Color data. The structure of the manuscript is straightforward, concise and well written. The figures are pertinent and of very good quality.

Specific comments and recommendations to improve the overall manuscript and some interpretations:

1. In the introduction, the authors mentioned “However, few observations of ice–edge phytoplankton blooms from satellites have been published to date (e.g. Arrigo and van Dijken, 2004), and thus a primary aim of this study is to fill this gap and investigate their existence at the large scale.” It is partly true. I would like to stress that a few studies have been done ∼20 years ago demonstrating the great potential of Ocean Color data (i.e. CZCS) to study Ice-Edge bloom in the Arctic. They provided insightful discussions about ice-edge bloom dynamics. One of them actually combine in situ observations acquired near-simultaneously with satellite data (Mitchell et al 1991). They discussed some inherent limitations of ocean color data (e.g. the difficulty to detect the post-bloom subsurface productivity). These studies couldn't perform large-scale studies due to lack of CZCS data in this region and limitation of the sensors. It is actually surprising that no one has addressed this issue since the SeaWiFS launch in 1997. So the present study is more than wellcome. I recommend adding these references as a recognition of their pioneer works:

a. Kögeler, J. and F. Rey (1999). "Ocean colour and the spatial and seasonal distribution of phytoplankton in the Barents Sea." International Journal of Remote Sensing 20(7): 1303-1318. b. Mitchell, B. G., E. A. Brody, et al. (1991). Meridional zonation of the Barent Sea ecosystem inferred from satellite remote sensing and in situ bio-optical observations. Pro Mare Symposium on Polar Marine Ecology, Trondheim, Polar Research. c. Maynard, N. G. and D. K. Clark (1987). "Satellite Color observations of spring blooming in the Bearing Sea shelf waters during the ice edge retreat in 1980." Journal of Geophysical Research-Oceans 92(C7): 7127-7139. d. Maynard,

BGD

7, C3906–C3909, 2010

Interactive Comment

N. G. (1986). "Coastal Zone Color Scanner imagery in the margical ice zone " Marine Technology Society 20(2): 14-27.

2. In section 4, the statement “Low chlorophyll values are visible between the main patch of the bloom and the sea–ice. The bloom is clearly propagating in a 20–100 km belt behind the retreating ice edge.” should be revised. The issue of sea-ice contamination must be stressed here as a potential explanation for the low chlorophyll values observed between the main patch and the sea-ice. Bélanger et al 2007 showed that adjacency effect (i.e. photon reflected by ice and then scattered above the water toward the sensor) might results in severe underestimation of chlorophyll concentration within the first 15 km of the ice edge (their Fig. 5). The underestimation is more important when the actual concentration is high, which may be the case in Fig 3. In addition, it was recently found that ice-edge blooms may be initiated below sea ice, when upon melting more light transmitted through sea ice (Mundy et al., GRL 2008). So it is actually possible that high chlorophyll values are present right at the ice edge.

3. Section 5. The lack of bloom in the central Arctic must be discussed more deeply. Several reasons may explain this observation: a. The late melting of sea ice. This reduced the chance to observe the bloom, if any, because Ocean Color data become scarce after mid-august (of are of poor quality) b. Central Arctic is strongly stratify and poor in nutrients and is likely that blooms cannot develop even if light is available. Nutrients may have been consumed during the summer because the pack ice is partly broken and let light penetrate the ocean.

4. Section 5. We could read: “For ∼30% of points with recorded blooms the first observation is the highest, indicating that the bloom probably peaked before the ice concentration durably reduced to below 10%, while a further 52% show the chlorophyll peaking in the MIZ period.” Here you could refer to the work of Mundy et al GRL 2008 (see above).

5. Section 5. The authors recognized that the VGPM have considerable uncertainty.

It is probably the worst model for the Arctic since it uses an empirical relationship for Pbopt that is certainly very far from the Arctic reality. Another problem with the PP modeling is the use of OC4v4 algorithm for the chlorophyll. This algorithm is inappropriate for the Arctic waters, which are CDOM rich relative to the global ocean (Amon et al JGR 2003; Siegel et al JGR 2002; 2005; Bélanger et al JGR 2008). So the magnitude of PP must be taken with a lot of care. In addition, the ratio TPP (6b) is most likely overestimated because of the well-known sub-surface bloom development after the spring bloom (see Mitchell et al 1991). The productivity of those sub-surface chlorophyll maxima are still to be investigate but may be relatively more important than in other ocean (see also Martin et al MEPS 2010). I recommend a deeper discussion about the validity of their results presented here (in particular Fig 6b).

6. Conclusion section. While most of their analysis is not so much affected by the absolute values of CHL, the authors must recognize again the large uncertainties of Ocean Color products for this peculiar ocean. The Arctic is affected by sea ice (see Bélanger et al 2007 and also Wang and Shi 2009), receives large river inputs and sub-surface production may contribute more than expected. Theses limitations make current quantitative assessment of PP from satellite very uncertain, including the work of Arrigo (e.g. OC4v4 in not appropiate at all but is still widely used).

Interactive comment on Biogeosciences Discuss., 7, 8123, 2010.