

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

Anonymous Referee #2

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Perrette et al. present the first pan-arctic study on the extent of ice-edge blooms, and their contribution to primary production. They conduct their study using ocean color remote sensing data. In the current context of major modifications Arctic marine ecosystems due to climate change, including the shrinking of multi-year ice, this study is timely and important. There are currently a lot of uncertainties in the use of ocean color data in the Arctic Ocean, but still, the timeliness of this study may make it worth being published in BG. It may provide a significant progress in our knowledge, and call for more such studies with improving methodologies. However, better arguments are needed to convince the reader that ice-edge blooms as detected from space are true. See the detailed comments below. Detailed comments: - The concept of “durably” or “consistently” below 10% of ice cover is vague when setting the start time of the MIZ. This must be clarified. - Although the assumption of consistent bias in time and space and therefore meaningful relative changes is somewhat reasonable, this needs

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some demonstration. I am not sure that it applies so well to adjacency effect, sub-pixel ice contamination and CDOM. This point needs to be further developed. - The last paragraph of Section 2 in which the potential biases are reviewed and one by one discarded as a significant problem is weak. Bélanger et al.’s study shows that, for ice floes, sub-pixel ice contamination always leads to over-estimation. The authors should provide a more detailed account of Bélanger et al.’s results to convince us that it is a negligible problem. - End of first paragraph in Section 4: “Low chlorophyll values are visible between ...”. That may well result from the adjacency effect as it generally lead to an under-estimation of chlorophyll concentration. - It would be useful to show detailed color images of ice concentration, for instance in Figs. 2 and 3, to get a sense of the possible impact of ice for concentration below 10%. I doubt that CDOM is always responsible for high chlorophyll values, for instance along the coasts in the Bering Sea. - Last paragraph of Section 5: Provide more details about the data used to run the VGPM (other than ocean color and ice). Also, the authors should discuss the strengths and weaknesses of that model in the Arctic Ocean. - The last sentence of Section 5 is unclear. - Legend of Fig. 3d: provide more detailed explanations about that panel.

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