Biogeosciences Discuss., 12, C7408–C7410, 2015
www.biogeosciences-discuss.net/12/C7408/2015/

Biogeosciences
Discussions

Interactive comment on "Spring bloom onset in the Nordic Seas" *by* A. Mignot et al.

Anonymous Referee #3

Received and published: 6 November 2015

General comments

In this paper, Mignot et al. address the mechanisms triggering spring phytoplankton bloom at high-latitudes. The authors use floats data to propose a new mechanism able to onset blooms north of the Arctic Circle. This new mechanism, presented as the "critical photoperiod hypothesis" (hereinafter CPH), is contrasted with the classical "critical depth hypothesis" (hereinafter CDH) formulated by Sverdrup nearly 70 years ago. The paper addresses a specific kind of blooms that were until now weakly studied: blooms occurring in open waters within the Arctic Polar Circle. The exploitation of data and the theoretical computation of the photoperiod is original and mostly coherent. The results and conclusion provide new insights to the recently invigorated high-latitude bloom debate. However, in my opinion the paper could be presented in a different way with the aim to stress the relevance of the proposed hypothesis. After some changes to justify several assumptions and to ease the paper and figures readiness I think the

paper can be published in this journal.

My main concern reading this paper is why authors test CDH in a framework that is not adapted to Sverdrup's hypothesis. As fully described in the paper, open waters north of the Arctic Circle are characterized by polar nights. How phytoplankton organisms survive to winter darkness is a question that authors cannot address with the current data (even if they speculate on possible mechanisms) but it is clear that phytoplankton populations in these latitudes present specific features that differentiate them from populations south of the Arctic Circle. In my opinion, these particularities invalidate the application of Sverdrup's hypothesis north of the Arctic Circle. In fact, a similar statement is already present in the text (p13633, l.21 to end of section or p.13647 l. 11-12). Furthermore I do not agree that "the data suggest" CDH as mechanism able to onset the bloom. If I am not wrong, this statement is mainly based in Fig.5a that only shows that Sverdrup's blooming condition are satisfied at tE but it says nothing about how actually started the bloom. For all that, I suggest that the paper focus only on the proposed CPH as a theory able to explain bloom dynamics in these extreme latitudes.

The second general comment concerns the general definition of the euphotic layer depth which is extremely delicate in this region. Eq 1 is based on global datasets that probably do not represent the very specific low light features of phytoplankton species adapted to polar latitudes. Furthermore, a value of Zeu=165m is used on the calculation of the photoperiod without any reference or argument that sustains this choice. Authors should provide some justification for using Eq.1 and this value of Zeu in the study.

Specific comments - It would be nice to know some more about the limits on the calculation of the photoperiod. How may changes on cloud coverage and "quick" (i.e.: less than 10 days) restratification events may influence CPH? In other words, how many days in a row of a 9-11h photoperiod may be necessary to trigger the bloom?

- p13640; l14-20. The word "years" is misleading to refer to the eight seasonal cycles sampled by the floats. I would suggest "seasonal cycles" or, simply, "cycles".

- It is very hard for the reader to relate the different seasonal cycles presented in Table1 and C2 with figures 3, 5 and B1. To ease the readiness and clarify the whole paper I suggest to identify each seasonal cycle with a different name; for example, IMR1a, IMR1b, IMR2a, IMR2b,..., IMR4, etc. As presented in the paper, the sentence in l26 has no sense because IMR2 and IMR3 sampled two seasonal cycles each.

- p13648; l5. Guessing that the "critical daylength hypothesis" is equivalent to CPH (please use only one of the two names for your hypothesis), I disagree with the sentence: CPH does have a link with mixing depth. Mixing partially controls the time that cells spend inside the Zeu (as authors detail later in the text).

- In Table C2; why the photoperiod of IMR2 2010-2011 is 14? If I am not wrong, in the text is stated that at tE, the photoperiod for this specific seasonal cycle bloom is close to 10h but arrives later than for the rest due to very deep mixing.

Technical corrections - p13643; l.20-23. The referred equation is Eq.(8) not Eq.(7).

- p13646; l16. Is there any reason why standing stock cannot be represented by <P> ?

- p13647; l5. "Eqs. (14a) or..." must be changed to "Eqs. (14b) or..."

- A right parenthesis is lacking in Figure 3 caption: (iPAR(0))

- In figure 5, the horizontal black line marking 0.06 is misleading with the vertical 0 line.

- I wonder if a colourscale could eventually be applied in figures 3 and 5 to help identified each seasonal cycle. The two cycles in red can be differentiated with dashed lines.

- Colours in Figure B1 are quite hard to discern.

Interactive comment on Biogeosciences Discuss., 12, 13631, 2015.