

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

**Anonymous Referee #2**

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What starts the spring phytoplankton bloom is a question that is important to our understanding of marine ecology and biogeochemistry. It is currently enjoying a significant amount of debate as scientists have revisited the classic theories by Sverdrup et al. The topic tackled by this paper is therefore of considerable broad interest, particularly as it introduces another new factor into the discussion - the photoperiod experienced by phytoplankton. While the new hypothesis put forward by the authors (in addition to testing a variation on a classic one) is exciting I think that the authors need to address the issues that I raise below before I can recommend the paper for publication.

My main concern is with the calculation of the photoperiod. The authors use the euphotic depth for their calculation. However, the euphotic depth is a relative measure. It is where irradiance is 1% of the surface value. It does not represent the amount of radiation available to phytoplankton. As phytoplankton will have a minimum requirement to sustain growth (e.g. Geider et al., *Journal of Phycology*, 21, 609–619), it is not clear

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to me that the euphotic depth is the best measure to use, particularly when the light level is near threshold values, as at the end of winter in the Arctic. e.g. if surface PAR is already at the minimum requirements then there will be no growth at depths with even 10% of this value, let alone 1%. Hence, I think that a more accurate approach would be to use the isolume of a minimum light flux rather than euphotic depth.

I also wonder whether a different means of estimating photoperiod might be more appropriate. The authors' approach estimates RMS speeds and assumes orbits within the mixed layer but doesn't seem to take into account the phase between the orbit and the daily cycle of light. Even if a cell spends 8 of 24 hours at the surface it will not see any light if it is there at night. Related to this it is not clear that the estimate of euphotic depth based on Chl makes sense at night. A simpler option would be to consider the population rather than individuals using the assumption that phytoplankton are homogenous in the mixed layer. (As an aside, from this perspective it is not clear how the rate of mixing can effect photoperiod as it does not affect the fraction of a homogenous population above a given depth.) During the night time the whole population is in the dark. In the day time (of duration D days) only the fraction of the population above the depth of the critical isolume (Z) is in the light at any given time i.e. Z/H of the population are in the light where H is mixed layer depth. Assuming, for simplicity, that the isolume changes depth linearly with time either side of noon (when  $Z=Z_{max}$ ) to zero at dawn/dusk, then the population average for the fraction of the day spent in the light is  $(D \cdot Z_{max}/2)/(T \cdot H)$  where T is 1 day. i.e. photoperiod  $= (1/2) \cdot D \cdot (Z_{max}/H)$  days Assuming a square profile of Z vs time instead removes the (1/2). The precise value is likely to be between the two estimates. Even taking Z-max to be  $Z_{eu}$ , the above equation is rather different to A6 in the manuscript, particularly in terms of dependence on  $Z_{eu}$  and H. For example, A6 would seem to predict that photoperiod increases with mixed layer depth for constant  $Z_{eu}$  (though the lack of brackets makes equations through the manuscript ambiguous). The above equation, however, predicts a decrease which makes more conceptual sense to me.

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Additional comments/questions:

- The authors should comment on how the frequency of their data (samples every 5 days or longer) affects their ability to test hypotheses related to bloom timing.
- How does Eq 1 perform against the PAR data from the float with a PAR sensor?
- Are there any profiles for which Chl is not homogeneous within the diagnosed mixed layer? If so, how many of the profiles? Would Chl be a better (i.e. more consistent) tracer to use for diagnosing mixed layer depth?
- It would be of interest to have a table of values for  $t_E$
- It's beyond the scope of this paper but it would be nice to see something in the Discussion on how resting spores can get back into the surface waters in waters that are >1000m deep

Minor comments:

- equations in Appendix would benefit from some brackets to make clear what is denominator and what is numerator.
- p13634, lines 17 and 19: one of the IMR4 should be IMR5?
- should use either 'critical photoperiod' or 'critical daylength', not both, for consistency
- equation 1 is repeated in Appendix as A1

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