**Note to All Reviewers:**

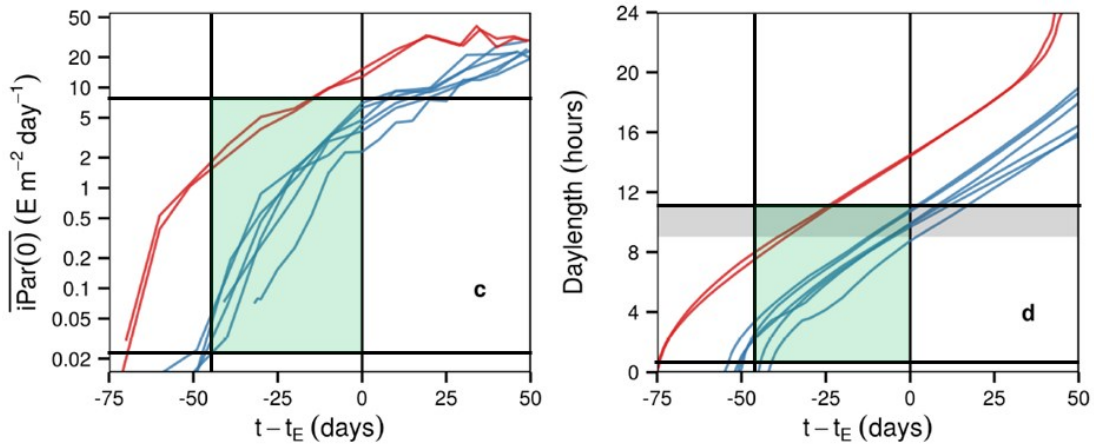
We wish to thank all the reviewers for offering many insightful comments and helping us clarify our results. Here we offer detailed responses to all questions. Reviewer's comments are in black, our replies are in blue, with direct quotes from the revised manuscript in *Times New Roman font*. Major changes include:
(1) Addition of three more floats in the database. We have removed as well the float that sampled during daytime, because the fluorescence data were potentially affected by nonphotochemical quenching.
(2) Simplification of the division rate model. We no longer estimate the division rate for three different classes. Instead we estimate the division rate for the bulk community using the formulation of Geider (1997) with the photo-physiological parameters from Antoine and Morel (1996).
(3) The structure of the manuscript has been modified to correctly portray our argument as suggested by reviewers #1, #3 and #4. The critical photoperiod hypothesis is now discussed before the critical depth hypothesis. This way we jump right into the new results, before discussing uncertainties and caveats of our interpretation.

**Responses to Reviewer #1 comments:**

Comment #1: The manuscript, "Spring bloom onset in the Nordic Seas" by Mignot et al attempts to evaluate growth conditions in the high latitude North Atlantic that are associated with bloom initiation. The study utilizes data from Bio-argo profiling floats that measure chlorophyll fluorescence and backscatter, along with other physical/chemical properties. I have multiple concerns with the analysis and manuscript, some of which compromise the validity of the conclusions and have no obvious solutions.
(1) One of the major problems with this analysis is that the data do not allow the fundamental question to be answered. This question is, 'What environmental conditions are associated with the onset of the spring bloom in the Nordic Seas?' According to the authors (first sentence in section 5.1), blooms begin (i.e., onset) when the average division rate of phytoplankton in the mixed layer exceeds the average loss rate (their terminology: 1/H<u-bar> is greater than or equal to m (= sum of all loss rates)). As acknowledged by the authors, the chlorophyll fluorometers used on the bio-argo floats have an insufficient sensitivity to detect fluorescence for a substantial period following the end of polar night. It is highly likely that chlorophyll concentrations are low but increasing during this period and, thus, that the bio-argo data misses the onset of the bloom. What the authors have done is identified a specific date at which the signal

measured by the fluorometers first exceeds the detection threshold of the instrument and associated this date with the latest possible start date for the bloom. They then argue correctly that the onset could not have occurred during polar night and, thus, the true onset occurs at sometime between the end of polar night and the first detection of fluorescence increase at, what they call, tE. This time range is denoted delta-t-onset and is shown as the gray shaded area in Figure 2c,d,e and equivalent panels in the supplemental figures. The duration of this potential range for the initiation date is approximately 1 1/2 months (~45 days) for each of the float data sets. With this information, we can now revise their figure 3c,d for the range of potential conditions associated with bloom onset. This revision is provided below and what it shows is that, given the limitations of the observations, bloom onset may occur at iPAR values ranging from ~0.02 to ~10 (E/m2/d) or day lengths ranging from ~1 h to 11 h, assuming we can ignore data corresponding to the red lines (also note the Einstein is not a SI unit, so that should be changed in the figure—*did you replace the Einstein with SI units?*). These are not useful constraints and certainly don't support a critical incident light threshold or day length threshold.

Bottom line is that the limitations of the fluorometer prevent any definitive statement about what controls the onset of the Nordic bloom, only what conditions exist when chlorophyll has risen above the detection threshold of the fluorometer (which is not a scientifically interesting question). In other words, the fluorescence data cannot be used to address the primary question, and I don't see any obvious way around this issue.




Response:

We believe that the fluorescence data offer useful information to study the development of blooms in the Nordic Seas. However, we agree that our presentation may have conveyed the opposite message. The paper began with a thorough discussion of the fact that the fluorescence concentrations were below the fluorometers noise threshold in winter. This would appear to prevent us from conclusively determining when chlorophyll (fluorescence) first began to grow at the end of winter. At face value we could only pinpoint when the chlorophyll (fluorescence) first exceeded the

fluorometer noise threshold. Unfortunately, we did not bring up two key aspects of the observations that appear to overcome these difficulties.

First, the fluorescence signal emerges from the noise threshold always at the same photoperiod of 10 ± 1 hours. We could find no reason to expect the noise threshold to be correlated to a particular photoperiod. It is much more likely that we are seeing a true biological signal.

Second, while the winter Chl-a concentrations are below instrument detection levels most of the time, there are plenty of profiles that have a few points with significant fluorescence values. This strongly suggests that the Chl-a concentrations are just below detection levels. If they were much smaller, we would not expect to have frequent spikes of significant Chl-a.

These two pieces of evidence strongly suggest that Chl-a concentrations are just below detection level in winter. At the end of winter, the Chl-a concentrations start growing rapidly and quickly emerge above the noise threshold of the instruments. Thus our first detection of fluorescence growth is likely quite close in time to the first growth of Chl-a in the water column.

We now move a detailed discussion of these two points upfront, straight after the presentations of the data. Then we have a section that discuss the implications of a correlation between bloom onset and a particular photoperiod. Finally, we retain a discussion of the possibility, however unlikely, that the correlation between the first detection of increase in fluorescence and a specific photoperiod in all the eight years is just a statistical fluke. We think that the new organization of the material makes a compelling case that there is a signal worth sharing with the rest of the scientific community. But we also fully disclose that additional measurements, with better fluorescence sensors, in the Nordic Seas are needed to put our results on a stronger footing. But this is the regular course of science. Every scientific finding is perfectible.

Comment #2:(page 1, lines 10-20) The critical depth hypothesis and the more recent hypothesis by Behrenfeld should be better represented in this text and elsewhere where they are discussed. The critical depth hypothesis (despite it's name) is actually a 'critical division rate hypothesis'. The idea is that division rate in the spring crosses a threshold rate where it first exceeds losses. This is clearly recognized by the authors later in the manuscript where they describe the CDH as: 1/H<u-bar> is greater than or equal to m. Thus, the testable hypothesis is whether division rate exhibits a threshold above which biomass increases and below which it decreases, irrespective of whether increases in division rate are caused by increasing incident light, shallower mixing, or both. The authors have already completed all the necessary calculations to test this fundamental prediction of the CDH and should show those results. They should also modify the manuscript so this concept is accurately portrayed. Based on the results

presented in the current manuscript, I see no evidence supporting the CDH.
The current manuscript also does not correctly represent the more recent interpretation of blooms suggested by Behrenfeld and colleagues. In their view, phytoplankton biomass can increase whenever division rate is increasing and will generally decrease when division rates are decreasing, with the exception for the latter condition being when the effects of population dilution have a greater impact on loss processes than division rates. In the current manuscript, the core of this bloom hypothesis has been ignored and only this latter exception of dilution effects is discussed. I see no evidence in the current data set suggesting that the Behrenfeld et al view is incorrect.
Again, the authors already have some data in hand to evaluate different bloom hypotheses. They should show the time series for each bio-argo data set of the relationship between calculated division rates and loss rate. The CDH predicts a threshold for bloom initiation while the Behrenfeld et al prediction is that division rates and loss rates covary and that biomass increases are associated with accelerations in division rate, not absolute values.

Response:

We agree with the reviewer. The more recent hypothesis by Behrenfeld and colleagues is now better represented in the revised manuscript. The corresponding paragraph in the introduction now reads:

"More recently, Behrenfeld and colleagues (2014; 2013) pointed out that blooms can develop because of any disturbance in the predator-prey balance which reduces the loss rates below the division rates. The disturbance may be initiated by winter mixing that dilutes both phytoplankton and herbivores reducing their encounter rate and hence the grazing rates (Behrenfeld, 2010). It may also be triggered by an improvement in growth conditions, such light exposure or nutrient availability that causes the division rates to accelerate and to outcompete the grazing rates. "

We have also revised the 3rd paragraph in section 4.1:

"To avoid any confusion down the road, it is worth emphasizing that the critical depth framework remains the key approach to study the development of blooms. There is however, an ongoing discussion as to what process drives Eq. (10) to be first satisfied at bloom onset. Sverdrup (1953) hypothesized that Eq. (10) is typically satisfied at the end of winter, when the ML depth H shoals resulting in an increase of $\frac{1}{H}\langle\mu\rangle$. Behrenfeld and colleagues (2013; 2014) argued that Eq. (10) is satisfied whenever a perturbation of the ecosystem drives $\frac{1}{H}\langle\mu\rangle$ to increase above m, including late

fall conditions when m decreases rapidly due to ML deepening and associated dilution of grazers (Behrenfeld, 2010). Finally, it is also possible for blooms to start in response to an increase in light, hence μ, with no changes in the other variables. In the following, we will use the critical depth framework to interpret the float data, with the goal of determining what process first trigger the bloom."

Comment #3: How did the authors deal with nonphotochemical quenching in fluorescence profiles collected during daylight hours?

The authors report that for fluorometer-MODIS matchups indicated that the manufacturer correction factor was consistently higher than the satellite based correction for 6 of the 8 floats. They therefore used the MODIS based corrections for these floats, but defaulted to the manufacturer's correction for the other two floats. Why? If the 6 floats that had adequate MODIS matchups all showed the manufacturer's value to be too high, then isn't it reasonable to assume that the manufacturer's value is also too high for the other two floats? Might I suggest using the average MODIS-based correction from the other 6 floats to do the conversion of data from the 2 floats with <10 MODIS matchups…?

Response:

To avoid any potential issue with nonphotochemical quenching, the only float that collected fluorescence profiles during daytime (wmo number 6900547), was removed from the database. The 8 other floats collected fluorescence profiles during nighttime.

In response to the second question, we now use a composite in 1°×1° boxes centered (instead of 0.2°×0.2°) for matchup data. The MODIS matchups for all floats were used to estimate the regression slopes [IMR1: 0.0051 mg $m^{-3}$ $count^{-1,}$,82 match-ups, R=0.61; IMR2: 0.0034 mg $m^{-3}$ $count^{-1,}$,69 match-ups, R=0.69; IMR3: 0.0049 mg $m^{-3}$ $count^{-1,}$,62 match-ups, R=0.63; IMR4: 0.0026 mg $m^{-3}$ $count^{-1,}$,36 match-ups, R=0.44; IMR5: 0.0020 mg $m^{-3}$ $count^{-1,}$,42 match-ups, R=0.81; IMR6: 0.0013 mg $m^{-3}$ $count^{-1,}$,42 match-ups, R=0.63; IMR7: 0.0018 mg $m^{-3}$ $count^{-1,}$,32 match-ups, R=0.49; IMR8: 0.0024 mg $m^{-3}$ $count^{-1,}$,46 match-ups, R=0.53]. These values are significantly smaller than those provided by the manufacturer's for our fluorometers (IMR1: 0.0072 mg $m^{-3}$ $count^{-1}$, IMR2: 0.0072 mg $m^{-3}$ $count^{-1,}$ IMR3: 0.0074 mg $m^{-3}$ $count^{-1}$, IMR4: 0.0073 mg $m^{-3}$ $count^{-1}$, IMR5: 0.0072 mg $m^{-3}$ $count^{-1}$, IMR6: 0.0072 mg $m^{-3}$ $count^{-1}$, IMR7: 0.0073 mg $m^{-3}$ $count^{-1}$, IMR8: 0.0072 mg $m^{-3}$ $count^{-1}$).

We attribute the variations in regression slopes for the various slopes to uncertainties in the matchups: we are regressing 1°×1° satellite data to pointwise measurements. Consistently, we compute the MODIS-based correction by averaging

over all eight floats and apply a 0.0029 +/- 0.0014 mg $m^{-3}$ $count^{-1}$ slope to compute chlorophyll from the nine fluorometers. Should have we applied a different regression slope for each float, the chlorophyll values for each float would have been very different, inconsistent with the fact that the floats sampled the same general region.

Comment #4: There has been a long history of debate about how to characterize the euphotic depth. The most common approach has been to use the 1% light level (or perhaps the 0.1% light level), but others have recognized that the percentage of light is not relevant to phytoplankton, only the absolute light level. Accordingly some authors have promoted used of a specific isolume. The current study is an extreme example of why the percentage light level should not be used to define euphotic depth. As a simple example, we see in figure 2a,b that the euphotic depth is estimated at approximately 170 meters during polar night. Of course, this is impossible. During polar night the euphotic depth = 0 m. I would STRONGLY suggest that the authors recalculate all euphotic depth values based on a chosen isolume (for example, simply follow the suggested value in Boss and Behrenfeld 2010 GRL). It is important to use a representative annual cycle in euphotic depths for comparison with mixed layer depths.

Response:

This is a good point. Following the strong suggestion of the reviewer, we now estimate the euphotic layer depth as the depth below which the light level is too low to support photosynthesis. This is now explained in the revised Section 2.4:

" We defined the instantaneous euphotic layer depth $iH_{eu}(t)$ as the depth below which the light level is too low to support photosynthesis. The threshold light level was set to 1 µmol quanta $m^{-2}$ $s^{-1}$, corresponding to the lowest light levels at which the temperate diatom *Phaeodactylum-tricornutum* has been observed to grow (Geider et al., 1986). Following the Beer-Lambert law, the incoming solar radiation was assumed to decay exponentially with depth. The decay rate, equal to the inverse of the diffuse coefficient attenuation of light K ($m^{-1}$), was set to a constant value throughout the day and from the surface to the depth at which the light intensity is 1% of its surface value, namely $H_{1\%}$. Therefore, K was given by:

$$K = \log (0.01)/H_{1\%}. \qquad (3)$$

The depth at which the light intensity is 1% of its surface value was calculated from $[Chl]_{ml}$ using the empirical relationship derived by (Morel et al., 2007) from a global datasets of ship-based measurements of $H_{1\%}$ and surface [Chl a] :

$\log_{10} H_{1\%} =$

$$=1.524 -0.436 \times \log_{10} [Chl]_{ml} – 0.0145\times (\log_{10} [Chl]_{ml})^2 + 0.0186\times (\log_{10}[Chl]_{ml})^3 \quad (4)$$

Whenever K was estimated to be lower than the diffuse coefficient attenuation of light by pure water $K_w = 0.027\ m^{-1}$ (Smith and Baker, 1981), K was set to $K_w$. Then, $iH_{eu}(t)$ was estimated as the depth where the irradiance is 1 µmol quanta $m^{-2}\ s^{-1}$:

$$iH_{eu}(t) = 1/K*\log [iPAR(0,t)/1], \quad (5)$$

where iPAR(0,t ) is the clear sky instantaneous PAR. Finally, we found that iHeu(t) transitions vary rapidly from zero at night to a constant value during the day, so that it can be described by a rectangle function. The height of the function is the daily-averaged euphotic layer, $H_{eu}$ (m), obtained by averaging Eq. (5) over the length of the day, the full-width is the length of daytime and the center is the local noon.

Comment #5: The calculation of daily PAR from iPAR should be included in this section, since daily PAR data are used in later sections.

Response:

Thanks, we have changed the corresponding paragraph accordingly:

" The daily sea surface PAR in mol quanta $m^{-2}$ $day^{-1}$, PAR(0), was obtained by integrating Eq. (2) over the length of the day."

Comment #6: (Section 3) The beginning of this section provides an honest account of the issue with the fluorimeter detection limit, clearly stating that it is not possible to define the true onset time of blooming, on the time when chlorophyll levels are sufficient for detection by the instrument. However, subsequent text treats this latter detection date as the bloom onset. This is simply inappropriate. Once it is stated that the data cannot identify the onset, an analysis of conditions for the onset of blooming needs to be abandoned.

To make sure I'm clear on the importance of the above issue, I've pasted below 3 paragraphs from Section 3 and added comments in bold **<text>**. This is only an example, similar issues occur throughout the text:

“Figure 3c shows that the solar radiation reaching the surface increased monotonically by close to two orders of magnitude during the weeks preceding t =tE, suggesting that increase in PAR played an important role in all bloom onsets. **<you don’t know when the onset occurred so this statement is not informative. obviously you need light for phytoplankton to divide, but figure 3c does not demonstrate the role of PAR on bloom onset>.** Figure 3d further shows that at t =tE, the time when the accumulation of phytoplankton biomass was first detected by 5 the fluorometer **<this is a proper statement given limitations of the instrument>**, the daylength was between 9 and 11 h for the six float years that clearly did not bloom in response to changes in heat fluxes and ML depth **<but this is not a proper statement because you cannot say when the bloom started>**.

Two possible bloom onset scenarios emerge from this simple preliminary analysis of the float data **<not true. you don’t know when onset occurred, so you cannot from figure 3 identify which “scenarios emerge”>.** One interpretation is that all bloom onsets are consistent with the critical depth hypothesis **<again, onset is not known>**. In six cases, the bloom started **<don’t know this>** because phytoplankton division rate increased rapidly as the surface insolation increased, and became larger than the phytoplankton loss rates **<this is not a logical argument, in that division rate does not have to increase rapidly until it becomes larger than loss rates, only that it IS larger than loss rates, independent of the absolute value>** (notice that these events are not quite consistent with Sverdrup’s assumption that it is changes in the ML depth rather than changes in surface insolation that are key) **<this statement in parentheses is not consistent with the CDH definition in the current manuscript>**. In the remaining two cases, it appears that the ML was so deep that the increase in surface insolation was not sufficient to drive phytoplankton division rates larger than the loss rates until the ML shoaled.**<don’t know this, because you don’t know when onset occurred>** However, it is also possible that the bloom started before the ML shoaling, but the biomass accumulation was so weak as to go undetected by the fluorometers **<this is a true statement and suggests that an assessment of bloom onset is not possible with these data>**.

A second interpretation is that blooms started at t =tE, when the accumulation of phytoplankton biomass was first detected by the fluorometer, and the photoperiod (the duration of a phytoplankton cell daily exposure to light) reached a critical value of 10±1 h **<there is no indication in figure 3 that daylength is a better predictor of when the instrument detects chlorophyll than iPAR (i.e., the six events correspond to a equally small range in iPAR). Neither of these interpretations are mechanistically defensible>**.

For the six events with shallow MLs, the photoperiod was equal to the daylength (see Fig. 4) **<note below, that the determination of photoperiod may be incorrect. It is also worth noting that, according to the supplemental figures, chlorophyll was detected above background for 4 of the 8 floats while the MLD was still deeper than the 'euphotic depth'>**. In the two cases with deep MLs, the phytoplankton did not experience 10±1 h of light until the mixing subsided and allowed cells to linger at the surface. In the next section, we develop the theoretical framework to test these two possible scenarios."

Response:

We think that the revised manuscript better explains why we think the fluorometer can be used to study the development of blooms in the Nordic Seas, as we explained in response to question #1. However, we revised the text in section 3 and we now carefully distinguish the time when the signal emerges above instrument noise from the bloom onset time. Here is the revised text:

"From fall to spring, in each of the nine events sampled by the floats north of the Arctic Circle (two years each from IMR1, IMR2, IMR3 and one year from IMR4, IMR5 and IMR6), we observed the same pattern in Chl a concentration. Figure 2 shows the potential density anomaly ($\sigma_\theta$), and [Chl a], acquired by the float IMR2 from September 2011 to June 2012. (Equivalent figures for the other eight years are displayed in the supplementary material Figs. S1-S9.) The ML and euphotic depths are marked as continuous and dashed black lines respectively. Figure 2a shows that in fall, from September to December, the [Chl a] decreased and the ML deepened. The fluorescence signal dropped to its minimum value from late December-early January during the polar night and the values were essentially uniform from the ML down to 1000 m (not shown) for the following several weeks.

To test whether the polar night ML [Chl a] was too low to be detected by the fluorometer, we compared the fluorescence measurements collected in the ML, where one expects to find some low [Chl a], with those comprised between 900 m and 1000 m, where no [Chl a] is expected and the fluorescence values can be used as an estimate of the dark signal, i.e. fluorescence values measured in the absence of [Chl a]. For each profile collected from December to April, we checked whether the distribution of fluorescence values in the ML was significantly different from the distribution of values between 900 and 1000 m using a two-sample Mann-Whitney-Wilcoxon test.

The Mann-Whitney-Wilcoxon test confirmed that during winter, the ML fluorescence values were not different from the deep values at the 95% confidence interval (marked with an asterisk in the figures). In other words, the winter [Chl a] in the ML was on average too low to be detected by the fluorometer. However, the fluorometers detected numerous spikes of higher than average [Chl a] in the winter mixed layer, but not at depth. This suggests that the winter [Chl a] was just below detection levels in winter and occasionally the signal emerged above the noise.

The winter [Chl a] profiles from floats IMR7 and IMR8, which profiled south of Arctic Circle in winter, were very different from those north of the Arctic Circle as shown in Fig. 3 and S.11. These profiles were characterized by significantly higher fluorescence values in the ML than at deeper depths, most likely because the [Chl a] remained high enough to be detected by the fluorometer. This last point is important, because it suggests that a period of complete darkness depletes the phytoplankton biomass so dramatically that traditional fluorometers cannot detect its concentration.

The ML fluorescence values north of the Arctic Circle emerged from the fluorometer noise level after the end of the polar night. The time of "emergence from noise" $t_E$, was defined as the time (second white vertical line in Fig. 2), when the ML fluorescence values became significantly greater than the deep fluorescence values as per the Mann-Whitney-Wilcoxon test in three consecutive profiles (~ 1 month). The positions of the floats at $t_E$ for all floats deployed north of the Arctic Circle are shown as a black dots in Fig. 1.

The net accumulation of chlorophyll starting at $t_E$ was detected both in surface $[Chl]_{ml}$ and vertically integrated <Chl>, and lasted until June-July. However, we cannot say whether accumulation started at $t_E$ or earlier, when the fluorescence values were too low to be detected by the fluorometer. Given that photoautotrophic growth is not possible without light, we can however conclude that the bloom must have started sometime between the end of the polar night and $t_E$. We will refer to this time interval as $\Delta t_{onset}$ (shown as a gray shading area in Fig. 2).

Figure 4 shows the surface heat fluxes, the ML depth, the daily PAR and the length of daytime with time shifted so that the origin is at $t = t_E$ for each of the nine float years. Interestingly, the time of "emergence from noise" of seven out of nine events (blue lines) occurred when the daylength was between 9 and 11 hours (Fig. 4d) and PAR (0) was between 4 and 7 mol quanta $m^{-}$

$^{2}$ day$^{-1}$ (Fig. 4c) . The surface heat flux $Q_0$ was moderately negative, between 100 and 200 W m$^{-2}$ (Fig. 4a), and the ML was as likely to be shoaling or deepening (Fig. 4b). Table 2 shows as well that the time of "emergence from noise" for these seven events always occurred between the day of the year 59 and 72. Moreover, the time of emergence from noise for the events located further south occurred earlier than for the events located further north. For the two other events (red lines), Figs 4a and 4b show that the time of "emergence from noise" coincided with the shutdown of convection and the sudden shoaling of the ML, when the daylength was between 14 hours, PAR (0) was ~15 mol quanta m$^{-2}$ day$^{-1}$ and the day of the year was 95 and 96 (Table 2).

Two possible bloom onset scenarios emerge from this simple preliminary analysis of the float data. Because it is hard to believe that the co-occurrence of a particular day of the year and the first increase in chlorophyll detected by the fluorometers is mere coincidence, one interpretation is that blooms started at $t = t_E$, when the accumulation of phytoplankton biomass was first detected by the fluorometer, and the photoperiod (the duration of a phytoplankton cell daily exposure to light) reached a critical value of 10 ± 1 hours. For the six events with shallow MLs, the photoperiod was equal to the daylength (see Fig. 5). In the two cases with deep MLs, the phytoplankton did not experience 10 ± 1 hours of light until the mixing subsided and allowed cells to linger at the surface. This interpretation is also consistent with the evidence that the winter $[Chl]_{ml}$ was just below detection levels and thus the emergence from noise was likely close to the actual increase in phytoplankton. (We only discuss photoperiod. Attempts to correlate the bloom onset with daily integrated light or maximum iPAR do not collapse the data as well, because cloud coverage varies strongly from year to year.)

A second interpretation is that all bloom onsets are consistent with the critical depth hypothesis. Blooms started when phytoplankton division rate became larger than the phytoplankton loss rates. However, the biomass accumulation was so weak during $\Delta t_{onset}$ that went undetected by the fluorometers. In this interpretation the coincidence of the emergence of the fluorescence signal from noise and the photoperiod must be considered a statistical fluke. We develop the theoretical framework to test these two possible scenarios in the next section."

**Table 2.** Time of "emergence from noise" **($t_E$)** in year day**,** and latitude at $t_E$. Events have been sorted in increasing day of the year form top to bottom.

| *Bloom* | $t_E$ | *Latitude* |
|---|---|---|

| | | |
|---|---|---|
| IMR1 2011-2012 | 59 | 66.7 |
| IMR1 2010-2011 | 62 | 68.5 |
| IMR5 2014-2015 | 66 | 72.5 |
| IMR3 2011-2012 | 69 | 69.9 |
| IMR4 2014-2015 | 69 | 70.4 |
| IMR2 2011-2012 | 70 | 70.5 |
| IMR6 2014-2015 | 72 | 69.9 |
| IMR3 2010-2011 | 95 | 69.4 |
| IMR2 2010-2011 | 96 | 69.0 |

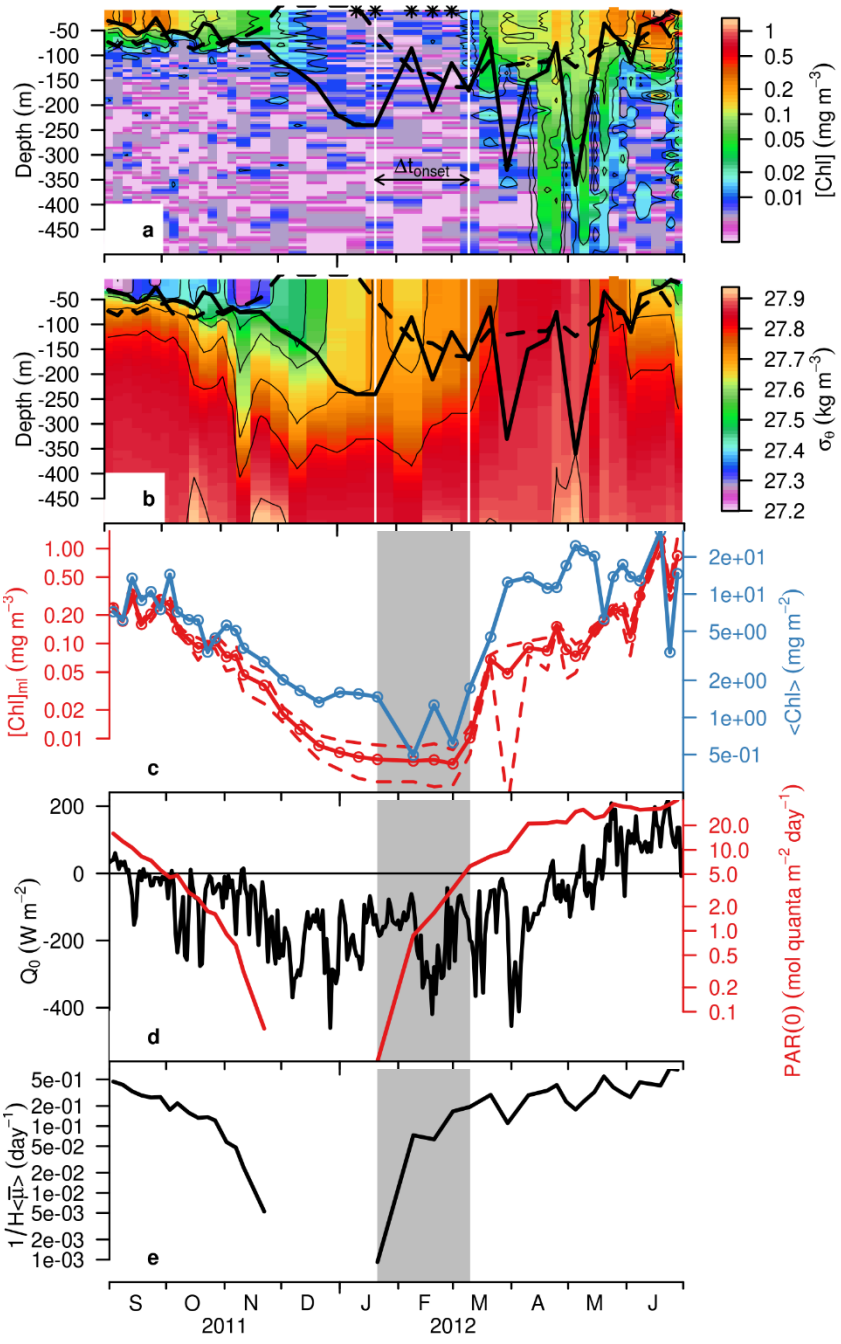



**Fig. 2.** Bloom observed by float IMR2 during the fall-spring 2011-2012. (**a**) Time evolution of the vertical distribution of [Chl *a*].The asterisks symbolize the vertical profiles where the ML fluorescence values are not significantly different from the deep fluorescence values. (**b**) Time evolution of the vertical distribution of potential density $\sigma_\theta$. The continuous and dashed black lines are the mixed layer, H, and euphotic layer depths, $H_{eu}$, respectively. (**c**) Time series of the vertical integral and the average concentration of [Chl *a*] in the ML (<Chl>, blue lines and

$[Chl]_{ml}$, red line).The dashed lines are the standard deviations around the average cycle of $[Chl]_{ml}$ .(**d**) Time series of the daily surface heat flux $Q_0$ (black line) and the daily surface PAR corrected for cloud cover, PAR(0),(red line). (**e**) Time series of the division rate. The two white vertical lines and the gray shading indicate $\Delta t_{onset}$; period of time during which bloom onset is possible. The second white vertical line indicates the sampling profile during which the ML fluorescence become significantly different from the deep fluorescence values (i.e., emergence from signal to noise, $t_E$).

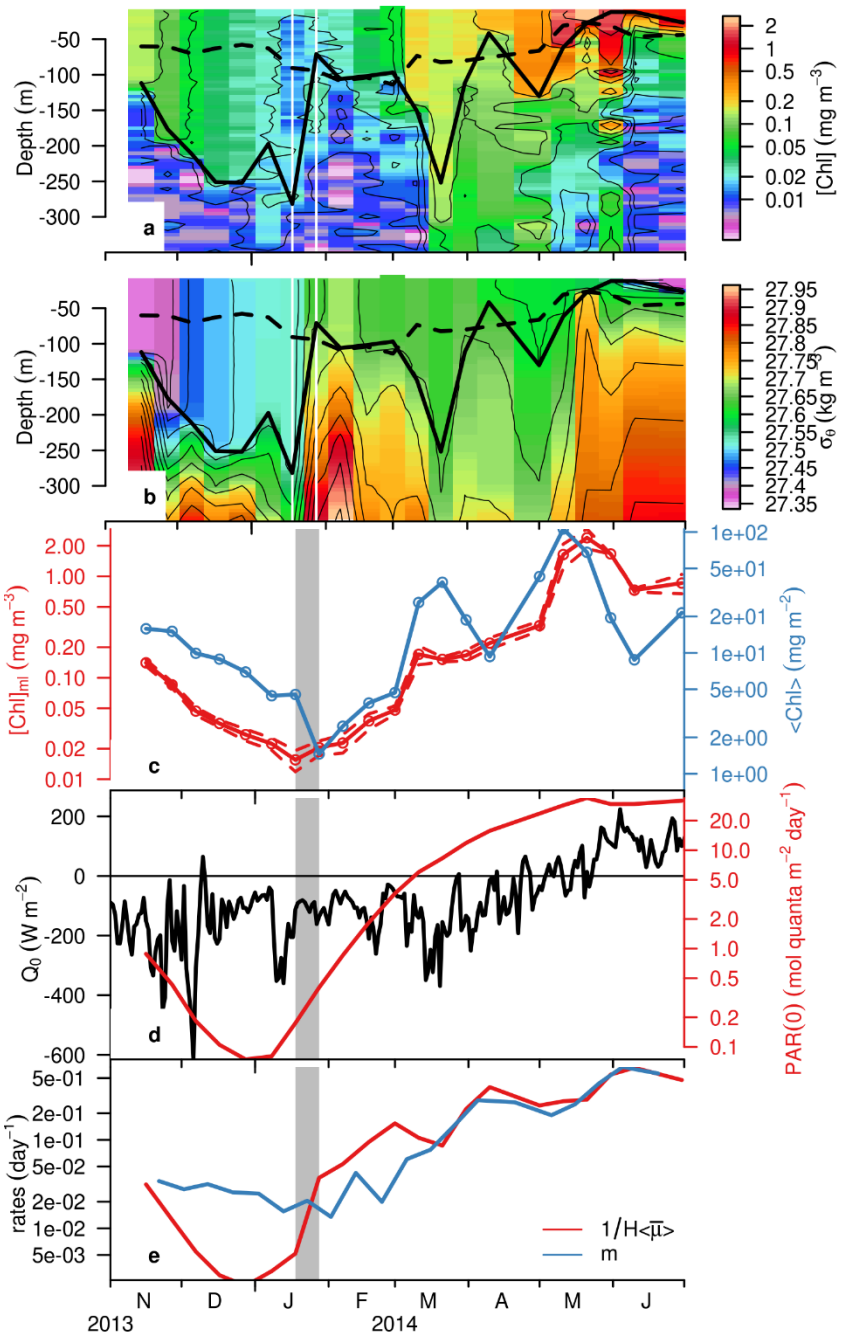



**Fig. 3.** Same as Fig. 2, but for the float IMR7 during the winter-spring 203-2014. The two white vertical lines and the gray shading indicate the onset of the bloom. In panel (**e**), the continuous red and blue lines are the daily mixed layer averaged division rate (1/H< $\bar{\mu}$>) and the phytoplankton loss rates (m), respectively.

Comment #7: (Section 4.1.1) This section begins with the following statement:

> "The division rate μ in Eq. (4) represents the division rate of the overall phytoplankton population. Thus, its quantification would require detailed information of the species present in the water column. Unfortunately, species information is very hard to collect."

This may be a true statement, but it is not clear exactly what information about species is needed and why this is critical to assess the division rate of the overall population. Please be specific on this requirement. In subsequent text, division rates are quantified in terms of class specific rates. How is it that grouping phytoplankton into classes alleviates the problem of the needed "detailed information" stated above? How well do the authors know appropriate 'class based' values for alpha, mu-max, and theta? Are the uncertainties so large that the reported differences between classes are statistically insignificant? If there are differences in photosynthetic performance between classes, wouldn't this be expressed, at least in part, by differences in Chl:C? Stated another way, is it reasonable to assume differences in photosynthetic performance and then simultaneously assume a constant value of Chl:C for all classes? Just asking….

Response:

We agree with the reviewer that the uncertainties in the class specific rates and the overall population composition are very large and our attempt to account for this variability was futile. We no longer estimate the division rate for three class-specific classes. Instead we estimate the division rate for the bulk community using the formulation of Geider (1997) with the photo-physiological parameters from Antoine and Morel (1996). New section 4.1.1 reads:

"The division rate μ in Eq. (6) represents the division rate of the overall phytoplankton population. We proceeded to quantify the division rate using the physiological model of Geider et al. (1997) together with the photo-physiological parameters from Antoine and Morel (1996).

Geider et al. (1997) proposed that the nutrient-saturated division rates are well described by the equation:

$$\mu\,(z,\,t) = \mu_{max}\left(1 - e^{-\frac{\alpha chl \times \theta c \times iPAR(z,t)}{\mu max}}\right), \qquad (11)$$

where $\mu_{max}$ is the maximum value of the division rate under light-saturated conditions ($s^{-1}$), $\alpha_{chl}$ is the Chl a-specific initial slope of the photosynthesis-irradiance curve, namely $6.4*10^{-6}$ gC $gChla^{-1}$ $(\mu mol\ quanta)^{-1}$ $m^2$ and $\theta_c$ is the chlorophyll to carbon ratio. The maximum value of the

division rate under light-saturated conditions is represented as a function of temperature, following the Arrhenius equation (Dutkiewicz et al., 2015) and similar to Eppley's (1972) :

$$\mu_{max} = 0.8 \times \mu_{ref}\, e^{\left(A_E\left(\frac{1}{T+273.15}-\frac{1}{T_0}\right)\right)}, \quad (12)$$

where $\mu_{ref}$ is the value for $\mu_{max}$ at 20°C, namely 0.0013* $\theta_c$ ($s^{-1}$), $A_E$ and $T_0$ regulate the form of the temperature modification function (-4000 K and 293.15 K, respectively) and T is the average temperature in the ML. The chlorophyll to carbon ratio is estimated using a float-derived empirical relationship between $\theta_c$ and the daily surface PAR, PAR(0), weighted for daylength (mol quanta $m^{-2}$ $hr^{-1}$) in the subpolar North Atlantic (~62°N) (Xing et al., 2014):

$$\theta c = 0.016 + (0.033 - 0.016) \times \exp(-3 \times \frac{PAR(0)}{dl} \times \exp(-0.5 \times K \times H)). \quad (13)$$

We are interested in sustained growth rates for at least a day, not transient growth rates lasting only a few hours. Consistently we average Eq. (11) over a full day (indicating by an overbar) in addition to integrating over the full ML depth:

$$\langle \bar{\mu} \rangle = \frac{1}{1\ \text{day}} \int_0^{1\ \text{day}} \int_0^{H} \mu_{max} \left(1 - e^{-\frac{\alpha chl \times \theta c \times iPAR(z,t)}{\mu_{max}}}\right) dz\, dt. \quad (14)$$

Finally, the vertical profile of iPAR (µmol photons $m^{-2}$ $s^{-1}$) is modeled through:

$$iPAR(z, t) = iPAR(0, t)\, e^{K \times z} \quad (15)$$

Comment #8: (Section 4.1.2) This section begins with the statement:

> "Phytoplankton loss rates are given by the sum of phytoplankton respiration rate, grazing, viral lysis and parasitism."

However, in the previous section, I believe that the model of productivity was parameterized using properties associated with net production (e.g., mu-max and alpha). Is so, then 'phytoplankton respiration' is not one of the processes included in the 'phytoplankton loss rate'.

Response:

Good point. We have corrected the manuscript.

Comment #9: (Section 4.2). Early in this section it is stated:

> "As one moves of the Arctic Circle, there are progressively longer periods of complete winter darkness, the polar nights. It is not clear that the critical depth framework is appropriate to study blooms under these conditions. The very concept of critical depth assumes that growth is always possible at the ocean surface, while this is not the case during polar nights."

Obviously, the beginning of the first sentences needs to be fixed. More importantly, the overall logic of these sentences is incorrect. The existence of polar night does not make the CDH inappropriate. The CDH states that there is a threshold division rate above which biomass accumulates and below which it decreases. Polar night just means that biomass should be decreasing.

Response:

We agree with the reviewer. The corresponding paragraph now reads:

" In the Nordic Seas, the insolation drops dramatically in winter. As one moves north of the Arctic Circle, there are progressively longer periods of complete winter darkness, the polar nights. Phytoplankton growth is simply impossible for days to weeks, depending on the latitude. Under these conditions, the focus must shift on understanding how phytoplankton cells survive the winter darkness to give rise to a bloom in spring. With no energy to photosynthesize, cells will likely strive to reduce losses due to metabolic respiration, grazing pressure, parasitism, and viral infections. There is literature, reviewed below, suggesting that the cells enter in a dormant state during polar nights and wake up when the daylength crosses some threshold."

Comment #10: (Section 4.2). Second paragraph in this section it is stated:

> "The 'critical daylength hypothesis' differs fundamentally from 'the critical depth hypothesis' in that the bloom onset is not associated with either mixing layer depth or biological losses."

These statements are incorrect. First, it is earlier stated in the manuscript that for 6 of the datasets the photoperiod = daylength, while for the other two photoperiod is not equal to daylength because of the mixing depth relative to the photic depth. Thus, the critical daylength hypothesis IS dependent on mixing depth. The critical depth hypothesis is not, based on the current manuscript's definition, fundamentally dependent on mixing depth, in the sense that one could hold mixing depth constant and only change incident light and still observe the threshold division rate associated with bloom onset (assuming such a thing exists). Furthermore, the critical daylength hypothesis is dependent on biological losses in exactly the same manner as the CDH. The assumption that phytoplankton start dividing only after a specified daylength is achieved does not mean that biomass will accumulate (i.e., bloom). The division rate after this 'critical daylength' still has to exceed loss rates.

More generally, I found the idea of a 'critical daylength' difficult to understand. First, if it did exist, all species within the population would have to have a critical daylength for initiating cell division. There is no evidence for this. If only a fraction of the population has a 'critical daylength', what prevents the other species from blooming earlier? Second, and as discussed above for (1), the data do not demonstrate that bloom onset is associated with a limited range of daylengths (also see technical concerns raised in comment (14)).

Response:

In response to the first and second question, we follow the critical daylength hypothesis as formulated and tested by Eilersten in the Nordic Seas coastal waters. It differs from the critical photoperiod hypothesis which is developed in this paper for the open ocean. The critical daylength hypothesis does not depend on ML mixing, whereas the critical photoperiod hypothesis depends on it. We agree with the comment that the division rates after the 'critical daylength' must exceed loss rates for a bloom to begin. We have removed the offending sentence since it was indeed inaccurate.

The third question raises an excellent point and we agree that there is no evidence that all species within the population ought to start dividing at a critical daylength. But absence of evidence is not evidence of absence. To our knowledge, such behavior has never been explored. However, we now point this caveat in the manuscript discussion.

*Comment #11: (Section 5.1) I'm afraid I found this section very confusing. I've pasted below the 3 paragraph of this section and added comments in bold* ***<text>*** *where I was unclear:*

> *"First, we test whether the start of the Nordic Seas blooms is consistent with the critical depth hypothesis, i.e. the blooms begin when 1/H<u> > m. To do so, <u>* ***<do you instead mean 1/H<u> here?>*** *is estimated according Eq. (10) using Antoine and Morel's (1996) model of PAR and the [Chl a]- based estimate of K. The phytoplankton loss rates are then computed as a residual between division and accumulation rates as described in the previous section* ***<according to the equations earlier in the manuscript, this calculation only makes sense if you are determining the loss rates from 1/H<u> and not simply <u> …>*** *. m was in the range of 0.0–0.4 day−1 with a median value of 0.06 day −1* ***<…so, what caused the loss rate to vary? is loss rate proportional to division rate? why not plot the loss rates along with division rates in the lower panel of figure 2 and the supplemental figures?>*** *. Loss rates could not be estimated prior to tE, because measurements of [Chl a] are dominated by noise*

*during delta-tonset* ***<see my multiple comments regarding this issue above>*** *. The median value across all eight years is used as representative of an upper bound on the winter phytoplankton loss rates* ***<why? why is the median value of any interest? what is of interest is the value of division and loss at all time points>*** *; respiration and grazing are likely to progressively increase through delta-t-onset as the Nordic Seas emerge out of the polar night* ***<what do you mean here? is it that the value of 'm' is increasing, or is 'm' constant while the product of 'm time P' increases?>*** *. A loss rate of within a range of 0.05–0.1 day−1 is typically used to parametrize phytoplankton non-grazing mortality rate (eg., Behrenfeld et al., 2013; Dutkiewicz et al., 2015; Evans and Parslow, 1985; Moore et al., 2002), thus our estimate support the hypothesis that grazing was very weak in winter* ***<maybe the model is wrong? do you have any stronger evidence? >*** *. In order to test if the bloom onset was consistent with the critical depth hypothesis we next test whether 1/H<u> exceeded 0.06 day−1 during delta-t-onset* ***<this is fundamentally incorrect. the CDH states that there is a threshold in mu above which biomass increases and below which it decreases. It does not say anything about the division rate relative to an average loss rate>*** *. Figure 5a shows the time series of delta-t-onset* ***<again, you don't know onset. same comment for all the other statements below regarding onset>*** *with time axis shifted so that for each of the eight years the origin is at tE. In all years, delta-t-onset exceeded 0.06 day−1 within the month prior to t =tE* ***<irrelevant. see above>*** *. Moreover, as anticipated in the preliminary data analysis, delta-t-onset primarily tracks the increase in insolation* ***<what does this mean?>*** *. Fig. 5b shows that the dramatic increase in delta-t-onset disappears if the seasonal increase in surface insolation is ignored – iPAR(0,t) was replaced with a periodic repetition of the daily cycle of incoming surface insolation on 1 March at 70. Surprisingly, even the deep MLs sampled by floats IMR2 and IMR3 had little impact in delaying the increase in division rates driven by the surface insolation. Indeed, it would be argued that the only reason for the delay in tE for these two years is because the MLs were very deep and the [Chl a] remained too diluted to be detected by the fluorometer.*

*In conclusion, our data are consistent with the hypothesis that the Nordic Seas blooms start according to the critical depth hypothesis* ***<I do not see how this statement is supported. I do not see evidience of a critical division rate threshold>*** *. But the analysis falls short of proving that the deepening of critical depth at the end of winter is the trigger of the bloom* ***<the CDH is fundamentally a concept based on a critical division rate, not depth>*** *. Such a proof would require accurate estimates of winter division and loss rates, which are simply impossible to obtain with present technology* ***<what about your backscatter***

***data?>*** *. Moreover, fluorometers with lower noise threshold are needed to document the first accumulation of chlorophyll in the Nordic Seas winter, when concentrations are extremely low."*

Response:

Based on your comments, we substantially revised section 5.1. We now test the critical depth hypothesis with the two events that did not experiment a polar night. For these two events: (1) the division rates and loss rates covary together during fall and spring. However, it in winter the loss rates cannot go below 0.02-0.05 $day^{-1}$. (2) Blooms started because $\frac{1}{H}\langle\bar{\mu}\rangle$ increased rapidly as the surface insolation increased, and became larger than the steady winter phytoplankton loss rates (0.02-0.05 $day^{-1}$). These two events are consistent with the critical depth hypothesis but not with Sverdrup's assumption that it is changes in the ML depth rather than changes in surface insolation that are key. Then, assuming that the winter dynamics of *m* is similar on both sides of the Arctic Circle, we test if the Nordic Seas blooms start according to the critical depth hypothesis.

The backscatter data show a peculiar dynamics. During winter, the values in the ML were always lower than the values below. Particles below and in the ML are likely to be different in composition and with possibly different dynamics associated. This of course, makes it hard to compute C-phyto from $b_{bp}$. Consequently, we have decided to not incorporate the $b_{bp}$ data in the manuscript.

New Section 5.1 reads:

" Next, we test whether the start of the Nordic Seas blooms is consistent with the critical depth hypothesis, i.e. the blooms begin when $\frac{1}{H}\langle\bar{\mu}\rangle \geq m$ before they are detected by the fluorometers. In this interpretation, the coincidence of the emergence of the fluorescence signal from noise and the photoperiod must be considered a statistical fluke. Since we cannot determine the loss rates during part of the winter north of the Arctic Circle, we first conduct the analysis on the two events that did not experiment the polar night. Then, assuming that the winter dynamics of *m* is similar on both sides of the Arctic Circle, we test if the Nordic Seas blooms start according to the critical depth hypothesis.

For the two blooms sampled south of the Arctic Circle, $\langle\bar{\mu}\rangle$ is estimated according Eq. (14) using Antoine and Morel's (1996) model of PAR and the [Chl *a*]-based estimate of K. The phytoplankton loss rates are then computed as a residual between division and accumulation rates as described in the previous section. Fig. 3e shows the time series of $\frac{1}{H}\langle\bar{\mu}\rangle$ and *m* for the float IMR7 from November 2014 to June 2015 (an equivalent figure for the float IMR8 is displayed in the supplementary material Fig. S11.). The figures reveal that $\frac{1}{H}\langle\bar{\mu}\rangle$ primarily tracks the increase in insolation; they both augmented monotonically by close to two orders of magnitude from January to April. The division rates and loss rates covary together during fall and spring. However, it seems that in winter the loss rates cannot go below 0.02-0.05 $day^{-1}$, hereinafter denoted as $m_{winter}$, whereas $\frac{1}{H}\langle\bar{\mu}\rangle$ can reach extremely low values, i.e., ~$5*10^{-3}$ $day^{-1}$. A loss rate of within a range of 0.05 - 0.1 $day^{-1}$ is typically used to parametrize phytoplankton non-grazing mortality rate (Behrenfeld et al., 2013; Dutkiewicz et al., 2015; Evans and Parslow, 1985; Moore et al., 2002), thus our estimate support the hypothesis that grazing was very weak in winter. The bloom onset was defined as the time when $[Chl]_{ML}$ or <Chl> first increased. For the float-year IMR7 2014-2015, the bloom onset was associated with a strong shoaling of the ML, resulting in an increase of $[Chl]_{ML}$ and a decrease in <Chl> due to detrainment. Inversely, for the float-year IMR8 2013-2014, the bloom onset was associated with a strong deepening of the ML, resulting in an increase of <Chl> and a decrease in $[Chl]_{ML}$ due to dilution with fluid with no phytoplankton from below. However, the Figs. 3e and S11e show that at such low light levels the change in ML depths had little impact on the dynamics of $\frac{1}{H}\langle\bar{\mu}\rangle$. Therefore, blooms started because $\frac{1}{H}\langle\bar{\mu}\rangle$ increased rapidly as the surface insolation increased, and became larger than the steady winter phytoplankton loss rates. These two events are consistent with the critical depth hypothesis but not with Sverdrup's assumption that it is changes in the ML depth rather than changes in surface insolation that are key.

The same analysis is repeated for the nine blooms sampled north of the Arctic Circle. Loss rates could not be estimated prior to $t_{E,}$ because measurements of [Chl *a*] are dominated by noise during $\Delta t_{onset}$. The average loss rates at $t_E$ across all nine blooms was 0.05 ± 0.04 $day^{-1}$. This value is consistent with the winter phytoplankton loss rates range estimated south of the Arctic Circle. We are therefore confident to assume that the winter dynamics of *m* was likely similar on both

sides of the Arctic Circle. Thus, in order to test if the bloom onset was consistent with the critical depth hypothesis, we next test whether $\frac{1}{H}\langle\bar{\mu}\rangle$ exceeded the upper bound of $m_{winter}$ , i.e. 0.05 day$^{-1}$ ,during $\Delta t_{onset,}$.

Fig. 6a shows the time series of $\frac{1}{H}\langle\bar{\mu}\rangle$ with time axis shifted so that for each of the nine years the origin is at $t_E$. In all years, $\frac{1}{H}\langle\bar{\mu}\rangle$ exceeded 0.05 day$^{-1}$ within the month prior to t = $t_E$. Moreover, as shown for the events sampled south of the Arctic Circle, $\frac{1}{H}\langle\bar{\mu}\rangle$ primarily tracks the increase in insolation. Fig. 6b shows that the dramatic increase in $\frac{1}{H}\langle\bar{\mu}\rangle$ disappears if the seasonal increase in surface insolation is ignored − iPAR (0, t) was replaced with a periodic repetition of the daily cycle of incoming surface insolation on March 1$^{st}$ at 70 °. Surprisingly, even the deep MLs sampled by floats IMR2 and IMR3 had little impact in delaying the increase in division rates driven by the surface insolation. Indeed, it would be argued that the only reason for the delay in $t_E$ for these two years is because the MLs were very deep and the [Chl *a*] remained too diluted to be detected by the fluorometer.

Our data are consistent with the hypothesis that the Nordic Seas blooms start according to the critical depth hypothesis. But the analysis falls short of proving that the deepening of critical depth at the end of winter is the trigger of the bloom. Such a proof would require accurate estimates of winter division and loss rates, which are simply impossible to obtain with present technology. Moreover, fluorometers with lower noise threshold are needed to document the first accumulation of chlorophyll in the Nordic Seas winter, when concentrations are extremely low.

In conclusion, the bloom onset is consistent with the photoperiod hypothesis if the chlorophyll started to accumulate when it was first detected by the fluorometer. However, it is also possible that the bloom started earlier according to the critical depth hypothesis, if some weak accumulation started earlier in the season at levels too low to be detected by fluorometers. Our opinion is that the photoperiod hypothesis is more likely to be correct, because it is hard to believe that the co-occurrence of a critical photoperiod of 10 ± 1 hours and the increase in chlorophyll detected by the fluorometers is mere coincidence."

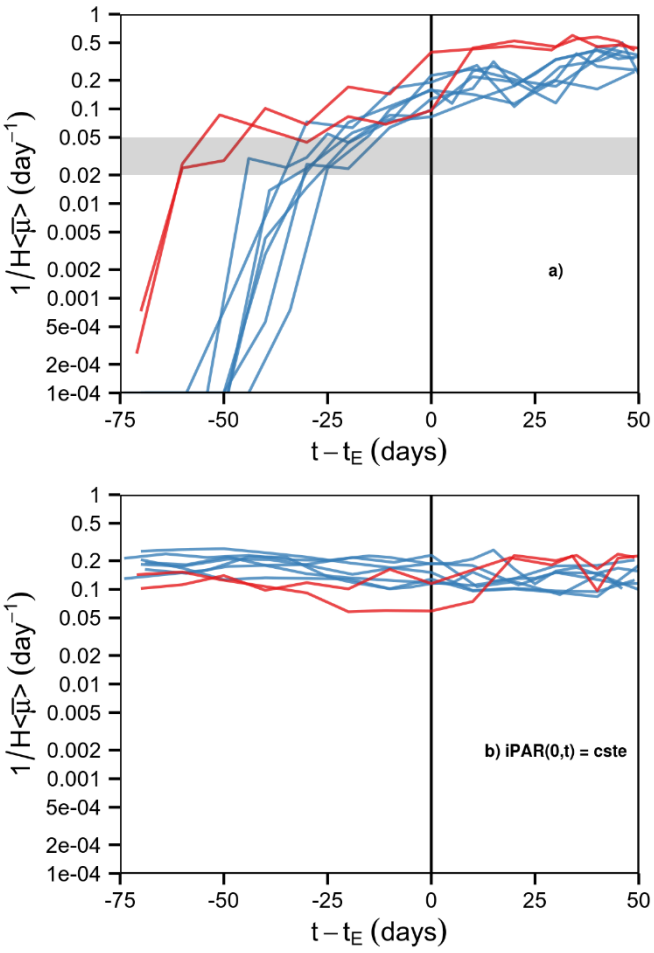



**Fig. 6.** Time series of the daily mixed layer-averaged phytoplankton division rate ($1/H<\bar{\mu}>$) based on Eq. (9) relative to $t = t_E$ for the eight events observed by the floats. The blue lines represent an accumulation of biomass associated with negative sea surface heat fluxes. The red lines represent accumulation of biomass associated with the shutdown of the wintertime cooling. The horizontal gray shading represents the winter phytoplankton loss rates range, namely 0.02-0.05 $day^{-1}$. **(a)** Estimates based a clear sky model of incoming irradiance for the days and latitudes sampled by each float (Gregg and Carder, 1990). (**b)** Estimates based on the periodic repetition of the incoming irradiance on March 1st at 70°N.

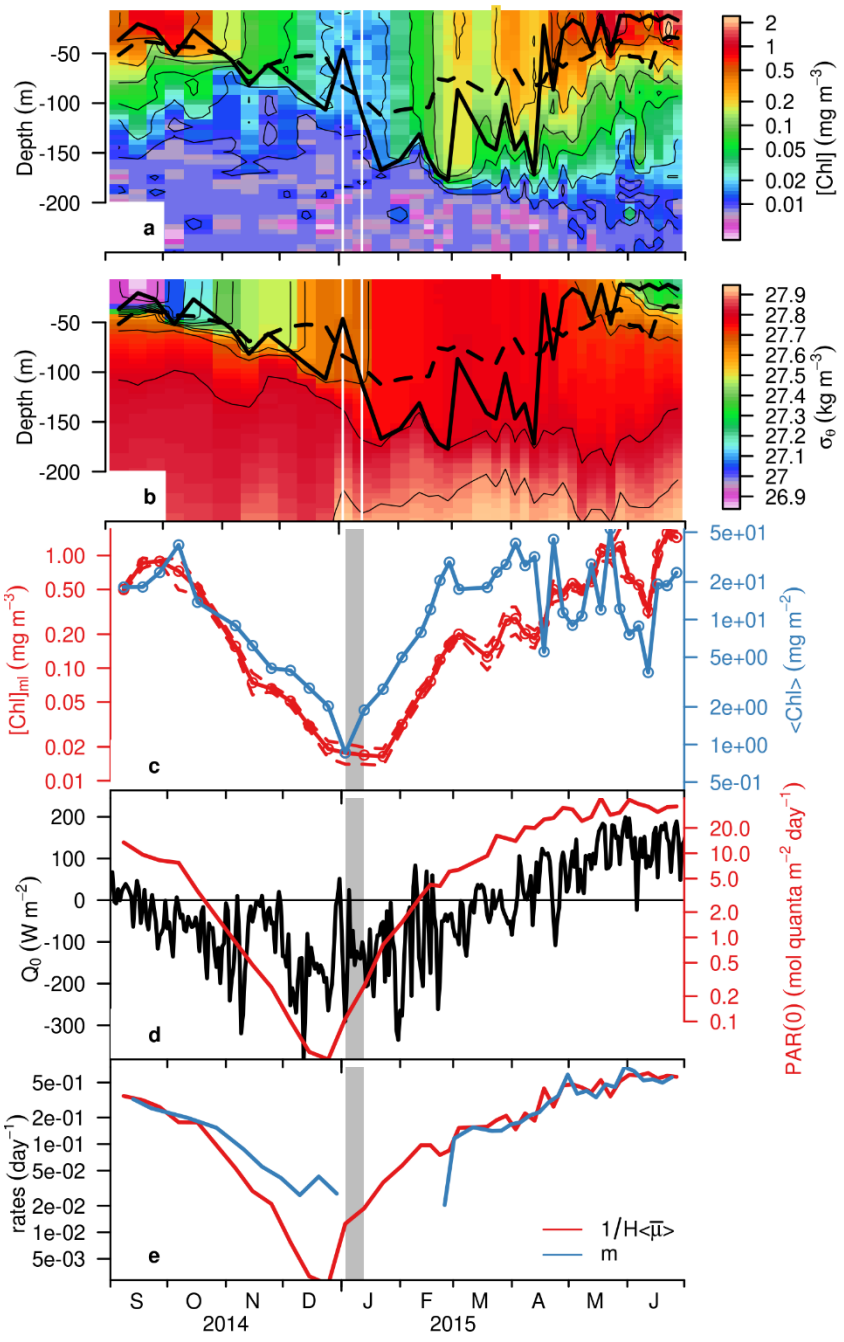



**Fig. S11.** Same as Fig. 2, but for the float IMR8 during the winter-spring 2014-2015. Note that two loss rates lower than 0, estimated in January, have been set to undefined value.

*Comment #12: (Appendix) What is the mechanistic justification for assessing photoperiod as the time spent in the euphotic layer within a day? In plant systems, length of day is often monitored using special light-sensing pigments, such as phytochrome. My understanding is that daylength is actually determined by sensing the duration of darkness. This measure of darkness registers the length of sustained darkness, not the daily integration of periodic exposures to darkness. Thus, in a cell mixing between a light and dark environment, it is not clear why the "time spent in the euphotic layer within a day" is an approriate assessment of what a phytoplankton measures as 'photoperiod'*

Response:

Excellent comment. We have thought a lot about this issue of "day-length" versus "darkness-length" and we concluded that in the ocean environment photoperiod based on "darkness-length" is not a viable strategy. At the end of winter, when mixing weakens, cells close to the surface can experience the full length of day. However many cells will be left behind in darkness, below the euphotic layer. It makes sense that the cells that experience some extended period of light will germinate, not the ones left in total darkness. More importantly, strong mixing events occur every time a storm passes by, suddenly deepening the mixed layer and increasing the number of hours without light experienced by phytoplankton. It would be suicidal for cells to germinate at this stage, when light conditions are clearly insufficient to sustain photosynthesis. Photoperiod in the ocean must be based on the length of "light-hours", if it is to be a viable strategy. We revised the manuscript to make this point. And we fully acknowledge that it remains an open question whether this form of photoperiod, based on number of light hours, is biologically plausible.

*Comment #13: (Figure 5) In the caption, I believe you mean 'horizontal' black line, not 'vertical'. Also, a reader is likely not going to understand what you mean by "periodic repetition of the incoming irradiance". Perhaps rewrite this.*

Response:

Yes, we meant horizontal black line, thanks. We have replaced "periodic repetition of the incoming irradiance by "periodic repetition of the daily cycle of incoming surface insolation on March 1$^{st}$ at 70"

*Comment #14:* (s*upplemental figures) In the top panel, please use the SAME scale for chlorophyll for all figures. In the current version, figure S5 and S9 use different scales than the rest. Related to this, it is not clear exactly how the date of tE was determined.*

*For example, in a number of these figures, tE corresponds to chlorophyll values of around 0.015, but in S5 it corresponds to something closer to 0.1 (i.e., about an order of magnitude higher). Why the difference? In figure S7, the value of tE should clearly be set at mid-February, where the mixed layer chlorophyll is easily above the background values. Of course, setting this value to mid-February would mean that this 'first detection' occurs at a daylength much shorter than the proposed critical photoperiod of 10 h. Similarly, in figure S1, it could also be argued that the assignment of tE is rather arbitrary and thus the 'photoperiod of first detection' is equally arbitrary.*

Response:

We did not determine $t_E$ based on an absolute values of chlorophyll. The criterion chosen to pick $t_E$ is now clearly spelled out in the manuscript:

"The time of "emergence from noise" $t_E$, was defined as the first time (second white vertical line in Fig. 2), when the ML fluorescence values became significantly greater than the deep fluorescence values, as per the Mann-Whitney-Wilcoxon test, in three consecutive profiles (~ 1 month)."

The reviewer is correct that in one event (IMR2-2010-2011; Figure S7) the mixed layer chlorophyll exceeds background values already at a photoperiod of 9 hours (still well within our estimate of the photoperiod of 10 ± 1 hours).

However the spike in chlorophyll was not seen in three consecutive profiles and thus did not meet our criterion for the definition of $t_E$, which aims to detect sustained growth not just the short lived bursts of high chlorophyll which can be easily due to lateral advection of different water masses.

The same scale for chlorophyll is now used for all figures.

**Responses to Reviewer #2 comments:**

Comment #01: 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 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.

Response:

Thanks for the positive comment. We agree with the reviewer. We have changed our definition of the euphotic layer. It is now defined as the depth below which the light level is too low to support photosynthesis. Please see our response to Reviewer #1 comment #4.

Comment #02: 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*Z_max/2)/(T*H) where T is 1 day. i.e. photoperiod =(1/2)*D*(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 de-pendence 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.

Response:

Excellent comment. We have thought a lot about the proper way to estimate the photoperiod in the ocean environment. Our approach was to evaluate if it was possible for one lucky individual cell to sense the whole length of daytime. On the contrary, the reviewer proposes to consider the population, rather than individuals. This method would give an estimate of the daily integration of periodic exposures to light for the whole population. Whereas we are interested in the length of sustained light exposure for one individual.

Moreover, thanks to the reviewer comment we have looked in detail at the diurnal cycle of the euphotic layer. We have found that the euphotic layer transitions very rapidly from zero at night to a constant value during the day, so that it can be described by a rectangle function. The height of the function being the daily-averaged euphotic layer. Please see our response to Reviewer 1 Comment #4.

Comment #03: 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.

Response:

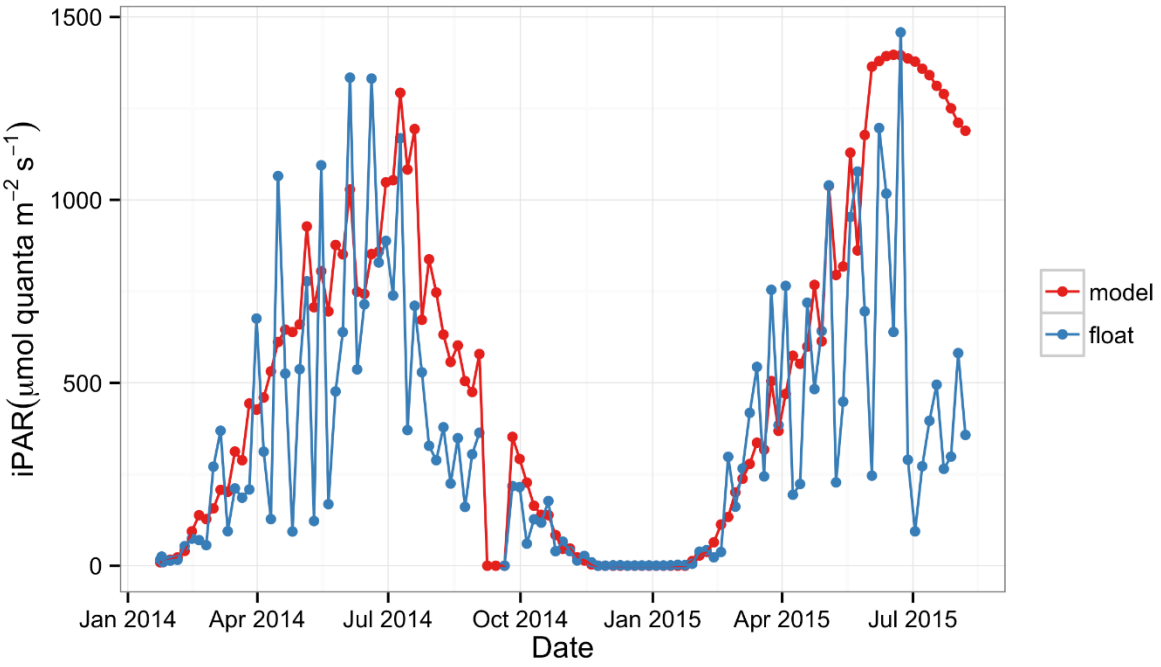
We commented in section 5.2 how the 10-day sampling frequency affected the determination of the photoperiod:

"The daylength increases by one hour every 10 days along the Arctic circle, so the photoperiod cannot be determined to better than one hour with the 10-day float sampling frequency."

Comment #04: How does Eq 1 perform against the PAR data from the float with a PAR sensor?

Response:

Good question. Below is a figure that shows the time series of iPAR measured just below the sea surface by the float (blue points) and iPAR estimated with Eq. (1). The correlation coefficient between the two estimates is 0.7. Overall, the model performs quite well in reproducing the sea surface PAR. We note however that the model reproduces a lower variability due to cloud coverage than the real data.




Comment #05: 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?

Response:

Overall, we found that the Chl vertical profiles become not homogeneous at the shutdown of the convective cooling. The Chl would definitively be a better tracer to use for diagnosing the ML depth. But having no fluorescence data during most of the winter, we cannot use this information to estimate the depth of the ML.

Comment #06: It would be of interest to have a table of values for t_E

Response:

We have incorporated values in yearday for $t_E$ in table 2 (see our response to Reviewer #1 comment #6) and in table C2 (2$^{nd}$ column).

**Table C2.** Time of "emergence from noise" **($t_E$),** daylength (hours), mixing layer depth (H), surface heat flux $Q_0$ (W m$^{-2}$), the euphotic layer residency time ($T_{eu}$) and photoperiod at $t_E$. The daylength is the estimated value at $t_E$. The estimates for $Q_0$, H, and their uncertainty are

discussed in the text. The estimates of $T_{eu}$, based on Eq. A6, are only indicated if $H > H_{eu}=165$ m.

| Bloom | $t_E$ | Daylength (hrs) | H (m) | $Q_0$ (W $m^{-2}$) | $T_{eu}$ (hrs) | Photoperiod (hrs) |
|---|---|---|---|---|---|---|
| IMR1 2010-2011 | 2011-03-03 | 10 | 240 ± 50 | -65 ± 70 | 12 | 10 |
| IMR1 2011-2012 | 2012-02-28 | 10 | 160 ± 20 | -130 ± 60 | | 10 |
| IMR2 2010-2011 | 2011-04-06 | 14 | 200 ± 170 | -100 ± 100 | 11 | 11 |
| IMR2 2011-2012 | 2012-03-10 | 11 | 140 ± 30 | -130 ± 20 | | 11 |
| IMR3 2010-2011 | 2011-04-05 | 14 | 360 ± 310 | -130 ± 120 | 10 | 10 |
| IMR3 2011-2012 | 2012-03-09 | 11 | 100 ± 10 | -120 ± 20 | | 11 |
| IMR4 2014-2015 | 2015-03-10 | 11 | 320 ± 10 | -150 ± 40 | 10 | 10 |
| IMR5 2014-2015 | 2015-03-07 | 10 | 120 ± 10 | -100 ± 30 | | 10 |
| IMR6 2014-2015 | 2015-03-13 | 11 | 330 ± 40 | -170 ± 50 | 9 | 9 |

Comment #07: 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.

Response:

Indeed, it is a very interesting topic that has never been addressed before. However, we believe that this issue would be better tackle in a separate paper.

Comment #08: Equations in Appendix would benefit from some brackets to make clear what is de-nominator and what is numerator.

Response:

We have added brackets to the equations in Appendix.

Comment #09: p13634, lines 17 and 19: one of the IMR4 should be IMR5?

Response:

No, it is IMR4. The float IMR5 is detailed in the Section "float deployed south of the Arctic Circle"

Comment #10: should use either ’critical photoperiod’ or ’critical daylength’, not both, for consistency

Response:

The critical daylength is the hypothesis as formulated and tested by Eilersten in the Nordic Seas coastal waters. It differs from the critical photoperiod hypothesis which is developed in this paper for the open ocean. Note that the sentence is now removed from the paper (please see our response to Reviewer #1 comment #10).

Comment #11: equation 1 is repeated in Appendix as A1

Response:

Thanks, we have changed it in the manuscript.

**Responses to Reviewer #3 comments:**

Comment #01: 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 phytoplank-ton 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.

Response:

We agree with the reviewer that the phytoplankton populations in these latitude present specific features that differentiate from population south of the Arctic Circle. Certainly there should be a strategy that enhance their survival when there is no light to support photosynthesize. However, because the Chlorophyll concentration was below the detection limit of the fluorometer, we cannot prove or disprove this possibility. Therefore, the critical depth hypothesis remains a plausible framework to

explain bloom dynamics in the Nordic Seas (please see our response to Reviewer #1 Comment#6)

As suggested by the reviewer, we have also changed the structure of the manuscript to correctly portray our argument. The critical photoperiod hypothesis is now discussed before the critical depth hypothesis. This way we jump right into the new results, before discussing uncertainties.

Comment #02: 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.

Response:

We agree with the reviewer. We have changed our definition of the euphotic layer. It is now defined as the depth below which the light level is too low to support photosynthesis. Please see our response to Reviewer #1 comment #4.

Comment #03: 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?

Response:

Indeed, it is a very interesting topic. However, we believe that it would be better address in a separate paper, should the critical photoperiod be proved true.

Comment #04: 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".

Response:

Ok we have replaced years by events.

Comment #05: 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.

Response:
Ok, we have changed it through the manuscript.

Comment #06: 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).

Response:
The critical daylength hypothesis refers to the hypothesis as formulated and tested by Eilersten in the Nordic Seas coastal waters. It differs from the critical photoperiod hypothesis which is developed in this paper for the open ocean. The critical daylength hypothesis does not depend on ML mixing, whereas the critical photoperiod hypothesis depends on it.

Comment #07: - 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.

Response:
You are correct. We have changed it in the manuscript.

Comment #08: Technical corrections - p13643; l.20-23. The referred equation is Eq.(8) not Eq.(7)

Response:
You are correct. We have changed it in the manuscript.

Comment #09: p13646; l16. Is there any reason why standing stock cannot be

represented by <P> ?

Response:
Good point. The standing stock is now represented by <P>

Comment #10: p13647; l5. "Eqs. (14a) or..." must be changed to "Eqs. (14b) or..."

Response:
You are correct. We have changed it in the manuscript.

Comment #11: A right parenthesis is lacking in Figure 3 caption: (iPAR(0))

Response:
Ok. We have added the lacking parenthesis

Comment #12: In figure 5, the horizontal black line marking 0.06 is misleading with the vertical 0 line.

Response:
The phytoplankton loss rates are now represented by a gray shading.

Comment #13: I wonder if a colourscale could eventually be applied in figures 3 and 5 to help iden-tified each seasonal cycle. The two cycles in red can be differentiated with dashed lines.

Response:
Thanks for the suggestion. However, we prefer to keep it as it is. The two colors, blue and red, help to contrast the 7 events associated with negative sea surface heat fluxes from the 2 events associated with the shutdown of the wintertime cooling.

Comment #14: Colours in Figure B1 are quite hard to discern.

Response:
We have improved the figure B1 to make the colours easier to discern.

**Responses to Reviewer #4 comments:**

Comment #01: Forming of sinking spores seems irrelevant in the area investigated, and the discussion on this process should be considerably shortened.

Response:

Ok. We have reduced the discussion on this process.

Comment #02: Introduction, page 2, line 23: “North of the Arctic Circle, no light is received at the ocean surface during the polar nights. Phytoplankton growth is simply impossible for days to weeks”. The authors have not measured the light during the polar night, and I have never seen complete darkness there. However, I know of unpublished work that actually measured photosynthesis at 81◦N in mid winter. Again, the authors should try to restrict their discussion to what can be extracted from their data.

Response:

Excellent remark. One float that is no longer used in the study carried a PAR sensor. The data shows that the average PAR just below the sea surface during the polar night was $0.8 \pm 0.3$ *µmol quanta* $m^{-2}$ $s^{-1}$. This value confirms the reviewer’s point, that there is no complete darkness during the polar nights. However, this value remains lower than the smallest light levels at which diatoms have been observed to grow (1 *µmol quanta* $m^{-2}$ $s^{-1}$, Geider et al., 1986). Moreover, phytoplankton cells, being mixed at greater depth, would be exposed to very brief instant of light over a day. Therefore, it is very likely that phytoplankton growth is so low during the polar nights that it can be considered as impossible. This point is now fully acknowledge in the manuscript.

Comment #03: It is refreshing to see that the authors go all the way to extract both growth rate and mortality from their fluorescence data. However, we know very little about mortality (and division rates) during winter. I agree that grazing probably is very low, and that division rate also is low. However, the argument that the length of day triggers the onset hinges on the definition of the onset of the bloom. But the authors are aware that this is not necessarily unambiguous: 6 Conclusions line 25: “We cannot definitively conclude that this increase marked the bloom onset, because low Chl a accumulation could have started earlier in the season at levels below the fluorometer’s detection levels”. The treatment of the data is elegant, and the role of surface heat flux was quite enlightening. I look forward to see a revised version in print.

Response:

Thanks for the positive comments

Comment #07: Specific comments 1 Intro page 2 line 5. “Phytoplankton division rates increase with abundance of nutrients and light.” Delete, next sentence is sufficient.

Response:

The sentence “At high latitudes, nutrients are plentiful in winter, because the strong upper ocean mixing generated by winds and cooling brings deep nutrients to the surface” is complementary rather than repetitive.

Comment #08: Figure 2. I kept looking for white vertical lines in panel e, until I realized they were only present in a and b.

Response:

In panel e, the gray shading indicates $\Delta t_{onset}$; period of time during which bloom onset is possible, whereas it is indicated by two white vertical lines in panel a and b. We have made it clear in the legend.

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