Review of Mignot et al. 2015 Biogeosciences

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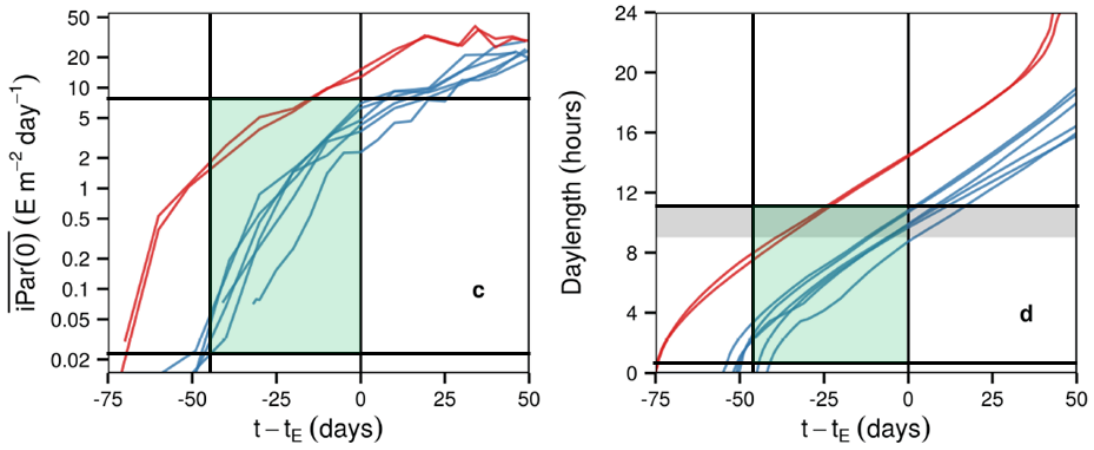
The manuscript, “Spring bloom onset in the Nordic Seas” by Mignot et al attempts to evaluate growth conditions in the high latitude North Atlantic that are associated with bloom initiation. The study utilizes data from Bio-argo profiling floats that measure chlorophyll fluorescence and backscatter, along 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 ½ 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). 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.

(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.

(3) (Section 2.3) 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…?

(4) (section 2.4) 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.

(5) (Section 2.5) The calculation of daily PAR from iPAR should be included in this section, since daily PAR data are used in later sections.

(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."

(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….

(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'.

(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.

(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)).

(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-t-onset **<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."

(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'

(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.

(14) (supplemental 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.