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Discussions

Interactive comment on "Synoptic scale analysis of mechanisms driving surface chlorophyll dynamics in the North Atlantic" *by* A. S. A. Ferreira et al.

Anonymous Referee #3

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Review of Ferreira et al 2015: Synoptic scale analysis of mechanisms driving surface chlorophyll dynamics in the North Atlantic

This paper investigates the relationship between physical forcing and bloom onset using a new method in conjugation with satellite-derived chlorophyll data. This method derives a new bloom phenology indicator to quantify how advanced or delayed the bloom is in comparison to the climatological year. The authors develop simple models to represent 4 hypotheses describing the physical conditions needed to initiation a bloom through bottom up processes. These models are tested on a pixel-by-pixel basis over the North Atlantic to find the "most likely" model for each time series. The authors find that the model representing the critical depth hypothesis is the most likely

over large areas of the North Atlantic.

This paper contributes to the ongoing discussion on mechanisms for bloom onset and I think the comparison of the 4 hypotheses brings an interesting and potentially useful discussion. However, I feel the paper needs work before it is ready to be published. In particular the methods section and description of the new method needs to be much more clear and explicit. I found it hard to assess the results and interpretations made as I do not fully understand the methods used and feel I cannot make a proper judgement on the integrity of the work. Therefore, I recommend major revisions before publication.

Specific comments

Justifying the choice of metric for bloom timing: The authors need to justify why they chose an indicator method that identifies the growing phase versus the start of seasonal increase in chl. Why is this an interesting part of the seasonal cycle to identify with the bloom indicator? There are many ways to define a phytoplankton bloom (as used in other studies). Determining bloom initiation can be based on threshold values, relative thresholds, rate of change in chlorophyll/biomass, fitted model parameters etc. But each of these indicators can be "tuned" to consistently identify the same part of the seasonal cycle – for example, identifying the beginning of the seasonal rise in chlorophyll would require a lower threshold value than identifying the middle of the growing phase. In this way different indicators can be used for different questions. For example, Behrenfeld's dilution-recoupling hypothesis indicates that the bloom starts in mid-winter when mixed layers are deepest and net growth becomes positive. However, at this time phytoplankton stock levels are still very low. Has a bloom started when growth became positive or when a certain level of biomass was reached? I think this depends really on why bloom initiation is of interest to the authors in the first place which I feel is not explained fully in the text.

Chlorophyll: The time series used for chlorophyll runs from 1998 to 2010 using the GlobColour dataset. However, though coverage is increased by merging the three sen-

sors, only SeaWiFS was available prior to 2002. Does the GAM fitting method deal with this change in coverage (i.e. increased gaps in the time series)? Phenology metric: Page 281: The explanation of how the metric is calculated and especially how it is used needs to be clearer. As it is a phenology metric I would assume the RPA has units of days but I'm not sure if this is the case so this needs to be clearer. Furthermore, there are a few statistical jargon phrases in this section that need to be explained (for readers who are not statisticians). Also, as an anomaly in chlorophyll from the climatology is used in calculating RPA, how does the method deal with interannual variability in bloom amplitude? Is delta chl a true measure of how advanced or delayed the bloom is or is a reflection of a difference in chlorophyll concentration between years? Additionally, is missing data in the satellite time series dealt with?

Analysis: Section 2.3: The explanation of the IT approach needs to be clearer. How does the method use the time series of physical variables and the hypotheses models (from Table 1)? Are there any major advantages or drawbacks to using this approach? It is difficult to assess the results from this method as the details of how the method works are not explained.

Discussion: In the discussion more care should be taken to interpret results. The critical hypothesis "wins" but the authors state two methodological reasons for this result. This is then tested by increasing model terms to 2 from 1 so all models are penalised by the same amount and the result (dominance of critical depth) disappears. When combining this result with the NHF and wind mixing variables dominating the results in Figure 4 I think it maybe questionable whether the critical depth result is real or not. This needs further investigation to check the validity of this result. Furthermore, the comparison of these results to previous studies seems to find that they are all consistent (e.g. line 5-6 page 287 – agreement with wind stress result but critical depth hypothesis and HF were reported as the "winning" variable/hypothesis). I think these comparisons just need a little more explanation.

Line 19-21 page 274: Delete "during the growing phase" from this sentence. I think

mentioning growing phase here is a little confusing, given the multiple ways other studies have define bloom initiation, and is probably best left explained in the methods.

Line 6-9 page 280: The HYCOM model assimilates observational data –how often? What is the coverage like in the North Atlantic? Is it more observational or more model?

Line 6-7 page 281: I do not understand what the peak contamination is. This could this be explained better.

Line 12-15 page 281: How does this day of climatological maximum rate of increase (g) compare to that of calculating the climatological time series from the satellite data first and then fitting the GAM. What was the justification for forming the climatology from the GAM fitting?

Line 24 page 282 to line 2 page 283: How were the results similar? Just no systematic pattern or did was the "winning" hypothesis result also seen?

Line 19 page 283: is the HF referred to here 0HF or 30d HF? Be consistent.

Line 2-4 page 284: I would say that the dominant processes controlling the bloom amplitude (nutrient limitation, grazing etc) are different from the processes that start the bloom (alleviation of light limitation) at least for this study region. If a time series shows a significant seasonal cycle then a bloom occurred and at some point a limiting factor was overcome, initiating the bloom. Defining bloom initiation is still problematic and may be due to different processes in different basins and at different spatial scales but maybe not for the reasons stated here. But maybe the authors have a different view?

Line 6 page 284: typo – change loosing to losing

Line 15 – 16 page 286: the seed stock does not really survive at depth but survives low light levels caused by very turbulent conditions which periodically move the cells from the surface to greater depth (according to the studies cited). The form of this sentence suggests that phytoplankton are kept at depth all winter which is not what the authors

are trying to describe here.

Table 1: What are B1, B2 and B3?

Figure 1: It is hard to see if the circles are open or not. Also, I am unsure of what they mean – what does each one represent (i.e. different years)?

Figure 3: I think the criterion of winning models having a weight higher than 30% should to be stated in the methods section as well.

Figure 4: Be consistent with the labelling of the frequency bar plot and the names of the variables in the caption and in the text.