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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 #1

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

This manuscript investigates physical processes associated with the start of rapid chlorophyll increase in spring in the North Atlantic. Physical parameters assessed include MLD, PAR, heat flux and wind stress, which are compared to satellite-derived chlorophyll time series. The manuscript presents some interesting results, however the methods are not sufficiently clearly explained and some of the interpretation of their results is, in my opinion, shaky. The authors seem to conflate the processes that control initiation of the bloom with the 'growing phase'. As the authors define it, (Figure 1 and 2) their 'growing phase' is clearly not the start of the bloom but the period of most rapid

increase. I don't think we should expect the relevant controlling processes to be the same for both. So the discussion where the authors attempt to compare their results to previous studies examining bloom initiation (true bloom initiation at the very start of the chl increase) is rather off-target. I suggest the manuscript be subject to major revisions.

Page 273, line 25 to Page 274, line 3: The assumption the authors are making by using surface chl to define a bloom is that phytoplankton are homogenously distributed within the mixed layer. This is not always the case (e.g. Townsend et al. 1994) and this needs to be made clear. However, I agree with the authors that satellite (surface) chl is the best option currently to look at large-scale phenology. I didn't understand the final sentence in this section – why is spring different from other times of year?

Page 274, line 4: I think the authors list 3 (not 4 as stated) processes here.

"", line 7: Isn't dilution a loss too?

"", line 17-18: I didn't understand the statement that surface chl can only vary due to growth or loss – surely that is the case for any stock (surface or otherwise?)

Page 276, section 1.3: The authors should make clear that they do not examine this group of hypotheses in this manuscript.

"", line 23: Behrenfeld does not (I think) consider the increase of day length in his hypothesis.

Page 277, line 1-2: again the authors should make clear that they cannot examine this hypothesis directly with this dataset.

"", lines 7-10: 'restratification', 'mixed layer shoaling' and 'weakening turbulent mixing' are different processes. (Stratification for example starts at the top of the ocean, not via a shoaling MLD). This was eloquently explained in the recent publication Franks (2014) doi: 10.1093/icesjms/fsu175

“”, lines 12-24: the opacity surrounding the methods starts here, where the IT approach is not explained clearly. Their description to me (and Table 1) seems to me to be a series of linear models constructed on the basis of existing hypotheses – this is no different (and no advance) on how research on this topic is typically conducted. Is this IT approach essentially multiple linear regression? Be clear.

Page 278, line 5: replace ‘apply’ with ‘can be applied to’

“”, line 10-11: I didn’t understand what ‘raw data (in their simpler form)’ is, or what that implies about the data previous studies have used?

“”, line 24-25: Light is always ‘available’ – whether it is enough for net positive phytoplankton growth is the key thing. Is a threshold value of minimum required light imposed?

Page 279, line 8: There is a ton of very high quality satellite-derived wind stress data available.

Page 280, line 19-20: Is the definition of MIX the appropriate one here? The authors might consider some of the mixing length scale calculations in Brody and Lozier (2014).

Page 281, lines 1-2: I did not follow the authors reasoning for using chl anomaly.

“”, lines 6-8: Again I did not follow the reasoning concerning the peak ‘contamination’.

“”, lines 9-10: Why not just calculate the climatology in the standard way rather than fit a GAM?

“”, lines 13-14: By ‘day of maximum increase’ do you mean you calculate dChl/dtime and find the maximum gradient? Importantly, the authors have not justified why they have .chosen this “RPA” metric.

“”, lines 15-19: A 30-day window is used: how do the authors deal with missing data in winter, when there can be a month or more of continuous missing data due to low sun angle? More fundamentally, how appropriate is it to use a 30-day moving window for

chl and physical forcing to investigate a phenomenon that occurs far more rapidly than that (a matter of days).

“”, lines 22-23: How low is ‘low seasonality’? The kriging is effectively inventing a bloom start date for a region where there is no bloom. This should clearly be avoided.

Page 282, lines 7-8: under what circumstances could thresholds not be estimated?

“”, lines 9-17: As far as I can understand the methods, the ‘IT method’ is a multiple linear regression with smoothed input fields. And the inputs are the start day and 30 day averaged forcing for each year, so that n = 13?

“”, line 20: Normally one would choose the model with the lowest AIC.

Page 283, lines 1-2: The same results were obtained with different bloom criteria – no systematic pattern as the authors say, but also critical depth dominant mechanism?

“”, line 17-18: are these single variable models just straightforward linear regression?

Page 284, lines 2-4: I don’t follow why the processes that control initiation should affect bloom amplitude? Also I don’t understand how this justifies use of ‘growing phase’ as the metric of ‘initiation’.

“”, lines 14-16: the CDM is calculated from measured PAR, not just modelled data. Also, observations of the MLD are available temporally and spatially resolved, so I didn’t understand the sentence on lines 16-17.

Page 285, lines7-9: Brody and Lozier tested the same set of theories but with a different approach. Also, the authors did not address mesoscale processes either (because they take a 1-D approach).

“”, lines 20-25: Again, 3D processes are not explored here, so the reasoning about subduction etc. is odd.

Page 286, lines 15-16: Has the seed stock been surviving at depth all winter? Or has convection kept them circulating (a la Backhaus, 1999).

Page 287, lines 1-6: Taboada and Anadon find that wind stress correlates with bloom timing across the majority of the subpolar North Atlantic. Your results find only very patchy correlation between wind + bloom timing. How then are your 'results in agreement'?

Page 287 + 288: As mentioned above, the authors are really investigating processes that correspond to changes in the timing of the growing phase, not really bloom initiation (as shown in Figure 1 and 2), so it is difficult to really compare these results to previous work.

Page 289, line 22: the authors do not show that the same mechanism does not hold in all years.

Table 1: The critical depth also depends on the compensation irradiance (as formulated by Sverdrup). What implications for the results does the simplification used here have? Are the alphas and betas the regression coefficients? What is 0 HF in the last line of the table? Is the model for critical depth in Table 1 any different from the single variable model used in Figure 3.

Figure 1: I'm not sure that this figure is actually useful for describing the different mechanisms. It's not clear how panels b and d are different from each other.

Figure 2: This diagram seems overly complicated to describe a very simple metric and is more likely to confuse than clarify the concept.

Figures 3 and 4: I find it hard to distinguish the blue and green colours.

Figure A1: Is this figure necessary? It doesn't seem to relate much to the discussion on page 274 where it's referenced.

Interactive comment on Biogeosciences Discuss., 12, 271, 2015.