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

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Response to reviewers on "Synoptic scale analysis of mechanisms driving surface chlorophyll dynamics in the North Atlantic" by A. S. A. Ferreira et al.

We would like to thank the three reviewers for the comments provided. We would also like to apologize for the repetitive tone of our replies, especially to the general comments. The three referees seem to have similar comments but we still decided to answer them one by one.

1 Anonymous Referee #1

1.1 General comments

We agree that the processes are not the same for the periods bloom initiation and growing phase. Our goal was to seek a metric that can be credibly related to the proposed processes involving surface mixing, mixed layer depth and heat flux, all of which relate to the preconditioning of the water column prior to bloom initiation. In that, any metric that used the bloom peak (such as the popular 5 % above annual median), or a seasonally integrated chlorophyll metric will suffer because it inherently takes into account not only what starts the bloom, but also what terminates it some weeks or months later. It was to try and get away from this problem that we decided to use the maximum rate of surface chlorophyll increase. We have therefore made sure we included this explanation in our manuscript.
We have also now tried to avoid the term "growing phase" as this can mean a number of things to different people. We talk instead of the rate of increase of surface chlorophyll – which we concede is not necessarily the same as cell division rate. It should be noted thought that these two aspects of the bloom are very much aligned at the start of the bloom when both dilution ad grazing are negligible.
In addition, the phenology metrics used in the literature (such as the 5 % above the median defined by Siegel et al 2002 and further used by Cole et al 2012, 2014, Racault et al 2012) usually fall within the "growing phase" and should maybe not be considered "initiation" metrics (see Ferreira et al 2014). We consider this to be a problem of semantics. It is indeed a metric of the greening of the oceans as observed from space.

1.2 Minor comments

Page 273, line 25 to Page 274, line 3: Our assumption is that the surface chlorophyll is representative of the vertically integrated chlorophyll, so that when there is an increase in chlorophyll concentration at the surface, and increase in the vertically integrated chlorophyll is also observed. We have rewritten this part to better reflect what we mean.
Page 274, line 4: We have changed it to three.
“”, line 7: We subcategorize dilution as a factor changing concentration, not necessarily a loss term.
“”, lines 17-18: Here, we meant that dilution does not influence surface chlorophyll during the growing phase, the phase where the rate of surface chlorophyll increase reaches its maximum.
Page 276, section 1.3: We have added subtitles to distinguish the mechanisms we do not examine.
“”, line 23: The increase of day length was used as an analogy to spring.
Page 277, lines 1-2: We have separated the subsections so it is clear.
“”, lines 7-10: We have changed the term “stratification” to stabilization of the water column.
“”, lines 12-24: We have added a more thorough explanation of the IT approach within its own subsection.
Page 278, line 5: Replaced.
“”, lines 10-11: We have added a sentence to clarify what we mean by “raw data”.
“”, lines 24-25: We do not use any threshold on light intensity. Instead, we use the integrated photosynthetic active radiation over the mixed layer, which changes with either the shoaling of the mixed layer depth or an increase in light intensity.
Page 279, line 8: The wind stress data we use are reanalysis, based on models and a wide palette of data. We have rephrased the text to reflect this. Please, see Kalnay et al (1996) for a description of the data source we used for wind stress and heat flux.

Page 280, lines 19-20: The MIX (|WS|ˆ3/2) is proportional to the mixing length scale calculations, more specifically the kinetic energy, in Brody and Lozier (2014). Because we want to try to minimize the level of pre-processing of original data, we decided to use a simpler form, which has been used before.
Page 281, lines 1-2: The term anomaly is used here because our metric is calculated with the climatological bloom as a reference, therefore providing an indication of how delayed or advanced the bloom is each year. We did this to avoid having to deal with maximum and minimum values of the chlorophyll cycle. See reply to general comments.
“”, lines 6-8: We have changed the wording of this sentence. For explanation see replies to general comments and to lines 1-2, page 281.
“”, lines 9-10: The way this GAM was applied is very similar to the “standard way”. It simply smooths the data for each year and then averages it to estimate an averaged cycle, the climatology. Using a GAM not only fills in gaps occurring systematically during the same days throughout the 13 years used in this study, e.g. during the winter; but also smooths the noisy daily data. Noise can contaminate the phenology metric being used and lead to a wrong estimation.
“”, lines 13-14: Yes, by “day of maximum increase”, we meant the maximum dCHl/dtime. The reason for using the RPA metric was presented, but maybe not thoroughly explained. We decided to develop this new metric because it avoids the use of the winter (which are often missing) and maximum values in the seasonal cycle. In contrast, it only depends on the growing phase, the phase where the rate of surface chlorophyll increase reaches its maximum, which we define as 30 days around the climatological maximum increase in surface chlorophyll.
“”, lines 15-19: Because we isolated the period of the growing phase, we do not deal with winter values. A 30-day window also covers the rapid bloom phenomenon, which could occur in the matter of days, as mentioned by the reviewer. We chose such a long window (30 days) to make sure we would cover a bloom that would occur within a few days far from the day of climatological maximum increase.

“”, lines 22-23: We do acknowledge this, but low seasonality issues are more of a problem South of our model domain (Taboada and Anadón 2014, Demarcq et al 2011). Only 22 +/- 11 % of the pixels could not be determined by the RPA method each year. The reason why the RPA did not work on some of the pixels was due to missing data around the date of interest. We also applied a threshold of at least 3 values within the 30-day window. We have now added text explaining this.
Page 282, lines 7-8: Check previous comment.
“”, lines 9-17: Yes, n = 13 (13 years). The IT approach compares competing models of any type, not necessarily linear regressions. Each of our models is a linear regression and we use the IT approach to weight the evidence in support of each model/hypothesis. We have added a separate section on the IT approach.
“”, line 20: The lowest AICc provides the highest AICc weight (or Akaike weight). AICc is the AIC corrected for small samples, which is the case in our study (13 years).
Page 283, lines 1-2: We compared the four models to other bloom metrics and found no systematic pattern. However, our goal was not to focus on these metrics, therefore we decided not to show these results.
“”, lines 17-18: Yes, they are linear regressions. We have referred to this in the text, so it is clearer now.
Page 284, lines 2-4: Because there is no connection between the processes that control initiation and the ones that control what finishes it (and thus determines the bloom amplitude), we felt the need to develop a new metric that was independent of the processes that finish a bloom. This is why a metric that only relies on the growing phase, as opposed to depending on the full seasonal cycle, is advantageous. We have added text explaining this. Moreover, most of the “initiation” metrics should in fact be considered “growing phase” metrics, simply because they fall too far from the initiation phase. However, we have removed the term “growing phase” from the manuscript.
“”, lines 14-16: It is true that the Argo-floats are starting to provide high-resolution MLD data. However, the time frame was not ideal for our study and their less recent data did not provide MLD data of high-resolution.

Pages 285, lines 7-9: We agree with the reviewer and we think our phrasing reflects that.
“”, lines 20-25: It is true that we do not include 3D processes in our study, but that does not necessarily mean they are not important. These lines are speculative, where we try to assess the implications on 3D processes.
Page 286, lines 15-16: The seed stock is kept all winter due to convection. We have now made sure we say this.
Page 287, lines 1-6: The agreement with Taboada and Anadón (2014) is in regards to this “confirm that spring blooms are triggered by different physical properties in different mesoscale regions”.
Pages 287 + 288: We believe this has been clarified in the general comments.
Page 289, line 22: We do not show that the same mechanism does not hold in all years, but, as we speculate earlier in the Discussion, the interannual variability of the driver mechanism is probably as likely to occur as the spatial variability.
Table 1: This table shows the four models used in our study. We include the integrated PAR over the MLD. The alphas and betas represent the regression coefficients. 0HF, as stated in the Methods section, is the day when net heat flux becomes positive. Yes, the models used in Figure 3 are the same as in this table.
Figure 1: The difference in panels b and d is in the heat flux: 30dHF (b) and 0HF (d).
Figure 2: After careful consideration, we have decided to keep this figure.
Figures 3 and 4: We have changed the colour scheme and we hope it is now clearer to distinguish the colours.
Figure A1: We have removed this figure.

2 Anonymous Referee #2

2.1 General comments

We agree with the reviewer that the use of statistical jargon is a little excessive. We have therefore included a more thorough explanation of the statistical methods used here. We have covered the comments made, either by providing an explanation for why we use the methods presented, or by following the reviewer's advice.

- Using GAMs simply smooths the data for each year and then averages it to estimate an averaged cycle, the climatology. The advantages of using a GAM have to do with smoothness and gap filling. Using a GAM not only fills in gaps occurring systematically during the same days throughout the 13 years used in this study, e.g. during the winter; but also smooths the noisy daily data. Noise can contaminate the phenology metric being used and lead to a wrong estimation (see Ferreira et al 2014).
- The reason for using the RPA metric was presented, but maybe not thoroughly explained. We decided to develop this new metric because it avoids the use of the winter (which are often missing) and maximum values. In contrast, it only depends on the growing phase, which we define as 30 days around the climatological maximum increase in surface chlorophyll. More bellow.

In general, we agree that the metric used in our study is different from the metrics used in the literature. As outlined above, there is a good reason for this in that bloom initiation – and all the physical reasoning that goes into describing its dynamics – should be independent of the bloom peak. Metrics that use the bloom peak will suffer because they inherently take into account not only what starts the bloom, but also what terminates it some weeks or months later. This is now stressed in our manuscript.

We have also now tried to avoid the term "growing phase" as this can mean a number of things to different people. We talk instead of the rate of increase of surface chlorophyll – which we concede is not necessarily the same as cell division rate. It should be noted thought that these two aspects of the bloom are very much aligned

at the start of the bloom when both dilution ad grazing are negligible. In addition, the phenology metrics used in the literature (such as the 5 % above the median defined by Siegel et al 2002 and further used by Cole et al 2012, 2014, Racault et al 2012) usually fall within the "growing phase" and should maybe not be considered "initiation" metrics (see Ferreira et al 2014). We consider this to be a problem of semantics. It is indeed a metric of the greening of the oceans as observed from space.

2.2 Minor comments

Page 274, line 2: We agree with the reviewer in that successfully detecting an accurate and precise date for bloom ignition is indeed not fully resolved. However, in this sentence we were referring to the detection of the bloom from space, just by observing the highly-resolved images from remote sensing. In particular, it is indeed possible to detect the bloom in regions where it is easily distinguishable, such as regions of high seasonality. In these regions, a bloom can be easily detectable either from space, or from the vertically integrated chlorophyll concentrations. We have, however, decided to remove this sentence.

Page 274, line 9: Because of such high-quality figures such as the one in Behrenfeld and Boss (2013) or Lindemann and St. John (2014), we believe that there is little to be gained in adding one more figure to the literature. In short, we think it is not necessary and it would probably just confuse, as it would perhaps give the idea that we defend a different mechanism than these authors do. Instead, we decided to develop a different metric that best described what we wanted to test. Our figure A1 was simply showing what we mean by the "growing phase", therefore it was added as an appendix, as it is not necessary to fully comprehend our study. We have, therefore, decided to remove the figure and refer to others available in the literature. In addition, we have removed the term "growing phase" from our manuscript.

Page 276, Section 1.4 "Grazing": We agree with the reviewer and have reorganized

these sections to reflect what was included in our study and what was not.
Page 279, line 8: The wind stress data we use are reanalysis, based on models and a wide palette of data. We have rephrased the text to reflect this. Please, see Kalnay et al (1996) for a description of the data source we used for wind stress and heat flux.
“”, Section 2.2 “Data Sets”: We have added reorganized the methods section.
Page 281, line 22: We do acknowledge this, and have added an explanation for the using of kriging. Low seasonality issues are more of a problem South of our model domain (Taboada and Anadón 2014, Demarcq et al 2011). Only 22 +/- 11 % of the pixels could not be determined by the RPA method each year. The reason why the RPA did not work on some of the pixels was due to missing data around the date of interest. We also applied a threshold of at least 3 values within the 30-day window.
Page 282, line 3: The moving average of 30 days around the day of climatological maximum increase in chlorophyll is simply a smoothing function that takes care of noise in the data. We believe that 15 days would be too short and 60 days too long.
“”, line 8: The thresholds referred here are the thresholds used in each model: average conditions 30 days prior to climatological maximum increase for all and the date of positive net heat flux.
“”, line 10: We have included a section thoroughly explaining the IT approach, that uses the Akaike Information Criteria (AIC).
Page 283, “Discussion” and “Conclusions”: Hopefully the new changes clarify this.

3 Anonymous Referee #3

3.1 General comments

We thank the reviewer and note that much of these points were also raised by the other reviewers. As outlined above, we agree that the metric used in our study is

different from the metrics used in the literature. In this, we have concentrated on the appearance of surface chlorophyll, the “greening” of the sea surface if you will, as an indicator of bloom initiation. This is after all the fundamental signal seen from satellites. Secondly, the physical reasoning behind the “bottom-up” bloom mechanisms all relate to its initiation – some combination of light, water column stability and mixing. None of these are strongly linked to what terminates the bloom some weeks or months later. It was to try and get away from this problem that we decided to use the maximum rate of surface chlorophyll increase. We have therefore made sure we included this explanation in our manuscript.
We have also now tried to avoid the term “growing phase” as this can mean a number of things to different people. We talk instead of the rate of increase of surface chlorophyll – which we concede is not necessarily the same as cell division rate. It should be noted thought that these two aspects of the bloom are very much aligned at the start of the bloom when both dilution ad grazing are negligible. In addition, the phenology metrics used in the literature (such as the 5 % above the median defined by Siegel et al 2002 and further used by Cole et al 2012, 2014, Racault et al 2012) usually fall within the “growing phase” and should maybe not be considered “initiation” metrics (see Ferreira et al 2014). We consider this to be a problem of semantics. It is indeed a metric of the greening of the oceans as observed from space.
Chlorophyll: The merging of the three sensors was not performed by us. It was performed by the GlobColour project, from which the products are available online.
Phenology metric, page 281: Yes, the phenology metric RPA has units of days and we have made sure this was included in the text. We have also provided a more thorough explanation of the RPA metric, and have removed statistical jargon. Our method does not require the maximum values of chlorophyll, therefore bloom amplitude is not an issue. Our RPA metric is not dCHL, but dtime, which is in days, thus providing an indicator of how advanced or delayed the bloom is each year. Missing data within the 30 days considered for the growing phase (the phase where the rate of surface chlorophyll increase reaches its maximum) are filled in by the GAM. We believe that

our late explanation was indeed confusing and that the new one may be able to better explain all the steps we performed and answer the questions the reviewer makes in this point.
Analysis, Section 2.3: We have included a more thorough explanation of the IT approach.
Discussion: There is a clear winner, which is the critical depth hypothesis, here described by PARmld (light integrated over the mixed layer depth). We have explored the strength of this model when comparing the model including PAR and MLD separately. The results are indeed puzzling. We tried to use the simplest level of data to be fed to the models, so that the interpretation would be as simple as possible. At other levels (like the ones presented by Brody and Lozier 2014), it becomes less clear which one is important.

3.2 Minor comments

Lines 19-21, page 274: We have considered this comment.
Lines 6-9, page 280: The HYCOM model assimilates weekly sea surface temperature, altimetry, ice concentration, ice drift, and available in situ measurements with the ensemble Kalman Filter (Evensen, 2003). The coverage of the model can be seen in Sakov et al (2012). In data assimilation one attempt to provide an optimal model estimate based on the optimal guess of the model and their accuracy estimated by the ensemble spread the observation and their uncertainty. So in this sense one can see as a synthesis of model and observation.
Lines 6-7, page 281: We have added a better explanation of this.
Lines 12-15, page 281: We believe the referee is confusing the climatological calculations due to our previous description of the method. We have now tried to fix the problem. The GAM was used to smooth the data and fill in the gaps. It is, however, just a averaging function.

Line 24, page 282 to line 2, page 283: The critical depth was still the winner, and there was no systematic pattern amongst the other.
Line 19, page 283: Here is referred to 30dHF. We have included this in the text.
Lines 2-4, page 284: We agree with the referee and have acknowledge the important of limiting factors.
Line 6, page 284: We have changed the word.
Lines 15-16, page 286: We agree and have changed the sentence accordingly.
Table 1: Beta1, beta2, and beta3 are regression coefficients of the models used in our study.
Figure 1: The circles are representing the 30 days isolated from each year. It is nor important if they are open or closed in this case as it is just a schematic figure.
Figure 3: This criterion was just used to map the data, otherwise it would be confusing to see.
Figure 4: We have followed the referee's advice.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C406/2015/bgd-12-C406-2015-supplement.pdf

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