Biogeosciences Discuss., 12, C56–C59, 2015
www.biogeosciences-discuss.net/12/C56/2015/

Biogeosciences
Discussions
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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.

Anonymous Referee #2

Received and published: 3 February 2015

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

The paper describes a new method to fit satellite surface chlorophyll time series on the North Atlantic, in order to identify the primary physical forcing factor for the phytoplankton growth. The authors limited their analysis to four theories based on bottom-up processes, developing then a simplified model for each theory. Each model is then evaluated, pixel by pixel, with satellite time series, in order to identify the "best" model for each time series. For this, a new phenological metric is introduced. The spatial distribution of the "best" model is discussed in the framework of existing hypothesis for the onset of the North Atlantic bloom. My review was quite finished when other reviewer

published its comments. I agree with most of the remarks of my colleague. However, in the next, I will describe my opinion independently.

General comments

Although I'm convinced that the paper is potentially interesting, I'm quite disappointed about the very scarce effort that the authors dedicated to explain the method. The whole approach is described in less than a page, using often a statistical jargon, which could be incomprehensible for a not expert reader (i.e. what is a "penalized cyclic cubic regression spline"? what is a "AIC with greater penalty for finite samples"?). I generally expect that statistical methods not widely used (as the ones proposed in the paper, as also stated by the authors) should be, at least, introduced with exhaustive explications, which is not the case in the present form of the paper. Moreover, it is sometime hard to understand why some, complex, statistical analysis are used instead of simpler methods (i.e. why using a Generalized Additive Method to generate a climatology? What is the added value compared to a "simple" average??). The RPA metric is also relatively obscure for me, and more critically, the justification of the introduction of this new metric is totally missing. I suppose also (on the basis of my incomplete comprehension of the method) that the RPA metric, based on the maximum gradient, differs noticeably to the existing metrics based on threshold values. In my opinion, then, the authors compared "apples" and "oranges". All the discussion is, then, not really pertinent. In conclusion, I suggest to strongly revisiting the paper (i.e. major revision).

Minor comments

Pag. 274, line 2. "In addition, during spring, it is possible to detect a bloom from the fluctuations both in surface chlorophyll and in vertically integrated biomass." This sentence is not clear and nebulous. The detection of a bloom is one of the most debated questions, and, if I'm not wrong, still not really resolved.

Pag 274. Line 9. The figure A1, introduced here, is really simplistic to explain the process behind the bloom initiation. The authors should propose more exhaustive illustrations (see, for comparison, the figure 2 in Behrenfeld and Boss, “Resurrecting the Ecological Underpinnings of Ocean Plankton Blooms”, Annu. Rev. Mar. Sci. 2014. 6:16.1–16.28), or simply refer to existing papers.

Pag 276. Section 1.4 “Grazing”. I’m quite confused about this section. It describes the impact of grazing process on the bloom onset, although the authors do not consider this mechanism in their further analysis. Moreover, not a clear separation exists between the grazing and a more general discussion on the paper topics (the difference is, I guess, at line 3, page 277). I suggest revisiting the whole section.

Pag. 279. Line 8. Why using model wind estimations when satellite alternatives are available?? Please explain.

Pag 279. Section “Data sets”. In my opinion, the description of the data processing, of the metrics and of the statistical analysis should not be mixed to the description of the data sets. More details are necessary to exhaustively explain the method and a dedicated section should be introduced.

Pag 281. Line 22. The use of the kriging is not explained and not justified. In my opinion, it could introduce important bias in the analysis. Why reconstruct where the method cannot be applied? How many pixels have “low seasonality”? Please, be more exhaustive.

Pag. 282. Line 3. Again, could the authors justify the use of a moving average? Why they use a 30day window (why not 15 or 60 days?).

Pag. 282. Line 8. Could the authors explain to which “threshold” the interpolation is applied?

Pag 282. Line 10. The AIC method, which seems central in the authors approach, is not clearly explained. I strongly suggest dedicating a figure, showing real or imaginary examples, on the method, which, if I well understood, provide an evaluation of the fitting.

Pag. 283. “Discussion” and “Conclusions”. As already stated, I’m not able to evaluate the “discussion” and “conclusions” sections because of I’m not still convinced about the data analysis. Although I have some doubts (see “general comments”), I suspend my judgement on these sections, waiting for a more exhaustive and clear description of the methods.

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