

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

I carefully read the responses to reviewers and the new version of the paper. Although I appreciated the effort of the authors to simplify the explanations of the methods (which was my main criticism to the paper), I have still some hesitations on the proposed approach. The RPA metric, in particular, appears unclear. (NB: In the follow, I marked in blue the formulas that are not explicitly present in the paper and that I tried to derive from text and plots).

The authors define RPA as

$$RPA = \Delta CHL / g \quad (1)$$

Where:

1. g is the maximum rate of increase of CHL_{clim} .

I guess then that:

$$g = \text{MAX}[\delta CHL_{clim} / \delta t]$$

2. ΔCHL is the CHL difference from climatology and from a specific year, both evaluated at the day RP_0 :

$$\Delta CHL = CHL_{year}(RP_0) - CHL_{clim}(RP_0)$$

Where RP_0 is the day of maximum increase in CHL_{clim} .

I guess then that

$$RP_0 = t \text{ [when } \delta CHL_{clim} / \delta t = g \text{]}$$

On the other hand, in figure 2, RPA seems to be the difference in time between g and the corresponding g , though calculated on the year series:

$$g_{year} = \text{MAX}[\delta CHL_{year} / \delta t]$$

$$RP_{year} = t \text{ [when } \delta CHL_{year} / \delta t = g_{year} \text{]}$$

And then

$$RPA = RP_0 - RP_{year} \quad (2)$$

The RPA definition given by the authors (line 257 of new version, equation 1 of this document), appears to me different to the definition that I obtained (equation 2) by interpreting text and figure. The definition of equation 2 seems also better match to the interpretation of the metric given by the author (line 259: "We thus estimated how delayed or advanced the bloom is in comparison with the climatological bloom"). I probably miss something, and I'm quite sure that the authors have probably a perfect explanation and they could probably clarify the discrepancies I highlighted. I'm however still disappointed that, despite of the explicit request of the three reviewers, several doubts on the definition and of the signification of the metric are persisting (at least for me). I'm certainly ignorant (or simply not so smart) but this doesn't justify the imprecisions of the proposed explications. Moreover, I have also suggested to the authors to dedicate more place to the explication of the methodology. I have also suggested the introduction of a specific figure to explain, using real data or on some schematic example, the proposed approach. I note that the size of the methods section was only slightly increased and no new figure is proposed.

For all the above, and although I'm still convinced that the paper is potentially interesting, I suggest a complete resubmission, as it is not acceptable in the present form.