

Interactive comment on “Interannual variability of the Mediterranean trophic regimes from ocean color satellites” by N. Mayot et al.

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First of all, we would like to thank Dr. Kostas Tsiraras for his true evaluation of our paper and his encouraging comments. We have modified the manuscript according to his comments. We think that the new manuscript has been accordingly improved.

Specific comments:

1) K. Tsiraras: “In Page 14946 (Line 6) you mention that the Chl-a time-series were normalized in order to minimize the impact of the satellite algorithm artifacts. Unless I’m missing something, It seems that since in your (clustering/time series) analysis you are interested in chl-a differences between different areas, using the absolute Chl-a would probably give the same results. The Chl-a normalization is very useful however

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in order to plot different areas on the same scale and probably also to remove any difference (in terms of bias) of the two satellite datasets. If this is the case, I suggest you rephrase your reasoning for normalizing Chl-a.”

Author’s response:

> We agree with the referee. We normalized the annual time series of [Chl]surf to minimise the potential errors (i.e. bias from the satellite algorithms) in the [Chl]surf estimates, but also to efficiently apply the clustering technique, which cannot be used on time series of absolute values of [Chl]surf, because the values’ range of variability is too high to provide a relevant clustering. As the referee correctly pointed out, we aimed to analyse the different areas on the same scale. To clarify our reasoning:

Page 14946, line 6 – the text was substituted with “Consequently, as in DR09, to minimize the impact of the [Chl]surf algorithms artifacts and in order to focus on the seasonal variations of the [Chl]surf (regardless the existing difference between the Mediterranean Sea areas in the values of [Chl]surf), each annual time series was normalized by its maximal value.”

2) K. Tsiraras: “Please provide some reference for the “Chebyshev distance” (P14946, L22).”

Author’s response:

>The Chebyshev distance between two time series $X=(x_1,x_2...x_n)$ and $Y=(y_1,y_2...y_n)$ is defined as,

[see equation (1) in the supplement to this comment]

with $n = 46$. In the manuscript:

Page 14946, line 21 – the text was substituted with “2. The similarity between the “annual” time series and each of DR09 trophic regimes is evaluated using the Chebyshev distance (e.g. Han et al., 2011), with only the 8-day averages of nChl as variables

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(i.e. 46 variables). Between two time series $X=(x_1,x_2,\dots,x_n)$ and $Y=(y_1,y_2,\dots,y_n)$ the Chebyshev distance (d_{XY}) is defined as,

[see equation (1) in the supplement to this comment]

with $n = 46$.”

The reference added is:

- Han, J., Kamber, M. and Pei, J.: Data Mining: Concepts and Techniques, third Edn., The Morgan Kaufmann Series in Data Management Systems, Morgan Kaufmann, Boston, 2011.

3) K. Tsiras: “It is not totally clear (also in DR09) how you do the clustering from the annual time series. From the dataset tables in Fig.1 it seems that you use the different 8-day Chl-a averages (w1-w46) as different “variables” in the clustering. If this is case or some other method (e.g taking some properties of the time series as “variables”) is used, please describe this explicitly in the methods section.”

Author’s response:

> The referee is right. As in DR09, we only used the 8-day averages of [Chl]surf as variables (i.e. 46 variables). To clarify:

Page 14946, line 21 – the text was substituted with “The similarity between the “annual” time series and each of DR09 trophic regimes is evaluated using the Chebyshev distance (e.g. Han et al., 2011), with only the 8-day averages of nChl as variables (i.e. 46 variables).”

5) K. Tsiras: “You mention (P14948, L5) that Fig.3 represents “16 annual maps of the spatial distribution of the 11 trophic regimes”. How are these annual maps generated? Do you follow the same procedure (as in step4, section 2.2), comparing each pixel annual time-series with the time-series of the clusters (DR09+anomalous)? Please explain in the text. Perhaps it would be also useful, in terms of methodology, to discuss

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how these maps would be different with the maps based on performing clustering on each year separately.”

Author’s response:

> The referee is right. The origin of the maps in Fig. 3 was not clear and the description of the method was misleading. In fact, each annual map is generated as follows:

Firstly, we identified, for each “annual” time series, the DR09 trophic regime with the most similar time series. When the “annual” time series is too different (i.e. an important Chebyshev distance) from the time series of this DR09 trophic regime, the “annual” time series is considered as “non-assigned” (steps 1 to 4 in the description of the method page 14946-14947). These first four steps are thus carried out on an annual basis. The result of these first four steps are 16 annual maps (not shown in the manuscript) illustrating the spatial distribution of the DR09 trophic regimes and also the spatial distribution of the pixels with a “non-assigned” time series.

Secondly, all the “non-assigned” time series, irrespectively of the year, are classified with a clustering analysis (i.e. a K-means clustering) to generate the “anomalous” trophic regimes (point 5). This last clustering provided a way to classify all the pixels whose time series after the step 4 was “non-assigned” to a DR09 trophic regime.

So, we did not perform two different analyses (one with only the DR09 trophic regimes and one with the DR09 + the anomalous), rather, we first assigned the pixels on the basis of the DR09 trophic regimes, then, for the remaining “non-assigned” pixels, we performed a cluster analysis to generate the “anomalous” trophic regimes. See also next comments.

4) K. Tsiras: “Step 5 (section 2.2 and Fig.1) also is not totally clear. You mention “from all 16 years combined”. How does this works? You put all the years of an “anomalous” pixel one below the other, as implied by the table in Fig.1 (e.g having 2000 below 1999 etc). This is slightly different from the clustering in DR09. Does this affects the

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procedure since there is the case that in one year a pixel is “anomalous” and in another is based on DR09? Please expand your description in methods to make this clearer for a reader not (necessarily) familiar with clustering techniques.”

Author’s response:

> As explained in the previous comment, the “anomalous” trophic regimes are obtained by clustering all the time series that were “non assigned” after the first four steps of our method. This is not inconsistent with the possibility, for one pixel, to show year-to-year variations in its associated trophic regime. However, our text was incomplete and misleading and we agree with the referee that the description should be strongly improved. For this reason, and because of the previous points, we modified the Methods section (Sect. 2.2):

Page 14946, line 11 – the text was substituted with “The method proposed here initially uses the trophic regimes identified by DR09 to classify pixels on an annual basis. The method consists in identifying, for each “annual” time series of each pixel, the DR09 trophic regime with the most similar time series. After this first classification, some time series remain unclassified (i.e. “non-assigned”). These “non-assigned” time series are then clustered to identify new trophic regimes, which were somehow hidden in the DR09 approach.”.

Page 14947, line 11 – this text was added “At this stage, 16 annual maps (not shown) were obtained, indicating either the membership of the pixels among one of the DR09 trophic regimes, or if they were still “non-assigned”.

Page 14947, line 12 – the text was substituted with “5. All of the “non-assigned” time series (from all the 16 years combined) were classified using the K-means clustering (Hartigan and Wong, 1979) (Fig. 1, step 5).”

Page 14947, line 24 – this text was added “The pixels whose times series were “non-assigned” at the step 4 are thus now classified as one of the “Anomalous” trophic

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regimes.”

6) K. Tsiras: “It would be useful to provide in Table 1 also the absolute Chl-a values (e.g in parenthesis after the normalized values) to permit a rough comparison between different clusters in terms of productivity. For example, is No_Bloom1 that is permanently observed in the Levantine the most “oligotrophic”?”

Author’s response:

> We agree. In Table 1, we added the absolute [Chl]surf values for the other indices of the time series (i.e. mean summer value and the annual maximum), in order to clarify the trophic status of each trophic regimes.

7) K. Tsiras: “By “minimum rate of change” (e.g P14948, L25) I guess you mean negative values, describing a stronger decrease. You can add a note in the text to make this more apparent.” Author’s response:

> The referee is right. We changed: Page 14948, line 25 – “. . .whereas the dates of the minimum rate of change (i.e. the date of the lowest first derivative of the nChl time series). . .” with “. . .whereas the dates of the minimum rate of change (i.e. the date of the lowest first derivative of the nChl time series, the most negative value). . .”.

8) K. Tsiras: “P14949, L2 “The maximum value of the “Coastal #6” time series is lower (0.72 nChl)”. Is this correct? It appears lower in the figure while 0.72 is higher than 0.66 of Bloom#5.”

Author’s response:

> The maximum value of the “Bloom #5” is 0.82 nChl, whereas its amplitude is 0.66 nChl (i.e. the difference between the mean summer values and the annual maximum values of nChl). Thus the sentence reported (Page 14949, line 2) is correct.

9) K. Tsiras: “P14950, L12 “but a higher amplitude of [Chl]surf (0.48 mg m-3 for the “Anomalous #4” and 0.25 for the “No Bloom #3”)”. Not sure what you mean here.

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Please check.”

Author’s response:

> We would like to indicate that the nChl time series of the “Anomalous #4” is flatter than the one of the “No Bloom #3” because the timing of the maximal value is more variable for the “Anomalous #4”. It is not due to a lower maximal value for the “Anomalous #4”, which has an amplitude in [Chl]surf more important. The explanation is more explicit with the maximum values, and thus we changed: Page 14950, line 12 – “the “Anomalous #4” trophic regime presents a lower maximal value of nChl (0.60 nChl) than the “No Bloom #3” trophic regime (0.86 nChl), indicating a variability in the timing of the peak between individual time-series, but a higher amplitude of [Chl]surf (0.48mg m-3 for the “Anomalous #4” and 0.25 for the “No Bloom #3”).”, with:

“the “Anomalous #4” trophic regime presents a higher maximum value of [Chl]surf (0.68 mg m-3) than the “No Bloom #3” trophic regime (0.35 mg m-3), but a lower maximum of nChl (0.60 nChl for the “Anomalous #4” and 0.86 nChl for the “No Bloom #3”), indicating a variability in the timing of the peak between individual time-series.”

10) K. Tsiras: “P14957, L1: “The bimodal pattern” Not sure what you mean here with bimodal.”

Author’s response:

> We agree, the expression “bimodal” was changed with “unimodal”.

11) K. Tsiras: “P14958, L13: With regard to the influence of the Black Sea Water, You could also refer to Petihakis et al. (2015).”

Author’s response:

> Done.

12) K. Tsiras: “P14962, L15 “the new approach had permitted to demonstrate that when the 16 years are considered separately, the patterns in the seasonality of the

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phytoplankton described by DR09 (except the “Coastal #7” trophic regimes) were always recovered.” Not sure what you mean by “considered separately” in this context.”

Author’s response:

> We used the expression “considered separately” to accentuate the fact that it was not a climatological study but an interannual analysis. To clarify the conclusion, we changed: Page 14962, line 14 – “In fact, the new approach had permitted to demonstrate that when the 16 years are considered separately, the patterns in the seasonality of the phytoplankton described by DR09 (except the “Coastal #7” trophic regimes) were always recovered.”, with:

“In fact, the new interannual approach allowed to demonstrate that the patterns in the seasonality of the phytoplankton described by DR09 (except the “Coastal #7” trophic regimes) were recovered for every year.”.

13) K. Tsiras: “P14960, L2 “..more than the deep convection events, the permanent cyclonic circulation in this region was the primary factor inducing favorable conditions for phytoplankton bloom, by bringing the nitracline depths close to surface. Relatively shallow mixed layers.. ” Usually deep convection sites are found in areas with cyclonic circulation due to the dome shape of the density that favours deep mixing and I think the phytoplankton bloom mechanism is mostly related to the vertical mixing. Therefore, the “relatively shallow mixed layers” might be misleading. I suggest you rephrase this.”

Author’s response:

> We agree. We removed the misleading sentences:

Page 14949, line 27 – “This uplift of the nitracline by the cyclonic circulation should allow an efficient replenishment of nitrate at the surface.”

Technical corrections:

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- K. Tsiaras: "-Page 14943, Line 3 & Line 8: Replace "dynamic" with "dynamics".
- Page 14943, Line 5 : Replace "that kind" with "those kind".
 - Page 14943, Line 6 : Replace "impact on the" with "impact the".
 - Page 14943, Line 21 : Replace "factors affecting ecosystem function" with "factors affecting the ecosystem functioning".
 - Page 14943, Line 22 : Rephrase "has been relatively under considered" with e.g "has received less consideration".
 - Page 14944, Line 17 : Replace "has been already used" with "has already been used"
 - Replace "and of nitrate" with "and the nitrate".
 - Page 14945, Line 20 : Replace "respectively 8 days and 9Km" with "9 Km and 8 days respectively".
 - Page 14947, Line 12 : Replace "from of all" with "from all".
 - Page 14950, Line 20 : Replace "We will discuss on this later" with "We will discuss this later".
 - Page 14955, Line 17 : "Similitude" You mean similarity?
 - Fig1: Replace "all years combined" with "all years combined".
 - Page 14960, Line 23 : Replace "is confirmed as be strongly impacted" with ""is confirmed to be strongly impacted".
 - Page 14962, Line 8 : Replace "have been hide" with "have been hidden" or "have been masked".
 - Page 14962, Line 8 : Replace "artifactual regime produce" with "artifactual regime produced"."

Author's response:

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> We agree with all technical corrections made by K. Tsiaras and modified the manuscript and all the figures by considering all these corrections. The manuscript was also proofread by an English native speaker.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C9297/2016/bgd-12-C9297-2016-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 12, 14941, 2015.

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