Response to anonymous Referee #1.

We would like to thank the referee #1 for its review and the interest he shows to our manuscript. We are also very grateful for the careful review of the English. Regarding linguistic corrections, we changed the manuscript according to his suggestions. Changes appear in red in the new version of the manuscript. In the following we answer the comments he had about science.

(1) To my knowledge, a stationary gyre, such as the great gyres of the oceans, doesn't cause up- or down-welling (vertical velocity should be zero in a common stationary case). It's a quasi-geostrophic, i.e. time-evolving situation that does. I do understand, though, that nutrients are kept more (less) distant from the surface at the center of an anticyclonic (cyclonic) feature, e.g. because of its depressed (uplifted) pycnocline. Please correct or comment (and provide reference).

Authors response: We agree that in basin scale quasi-geostrophic gyre, vertical velocity is null so there are no nutrient supplies by this way. We wanted to refer to the impact of cyclonic and anticyclonic circulation on the position (depth) of the nutricline. To avoid confusion, we removed the mention to downwelling/upwelling line 16 (page 2) and changed the text accordingly:

"At basin scale, the trophic status of subtropical (subpolar) gyres is determined by anticyclonic (cyclonic) circulation, which controls the depth of the nutricline. " (page 2, lines 16-17)

(2) I have difficulty in attributing an anticyclonic circulation to the ADT pattern of Fig. 2d. Could the Authors better illustrate this circulation? By eye, it doesn't seem conceptually (sign-wise) different from the cyclonic pattens, though with less negative ADT values in the north. BTW I am OK with Fig. 2b's anticyclonic pattern, but Fig. 2d doesn't look like Fig. 2b. Line 20.

Authors response: We attributed an anticyclonic circulation pattern to the figure 2d because one can observe a tongue of higher ADT values (about –2 cm) expending from the Mid Ionian Jet to the north. A small yellow spot is visible around 38°N in the center of the North Ionian Jet. Although it is less clear than for a typical anticyclonic pattern (Figure 2C and not 2b), a similar structure is observed in Figure 2d. Please note that Figure 2b represents the cyclonic situation.

The following sentence has been added to the text (page 6, line 13)

"Indeed, similarly to Figure 2c, in Figure 2d a tongue with higher ADT values extents northward from the Mid Ionian Jet to the center of the NIG."

(3) Section title. I'm not sure about the title construction "Role of... compared to...", a little illogic. Maybe "NIG circulation patterns and MLD variability" or "Role of the NIG circulation in the variability of the MLD" or "The NIG circulation patterns compared to the ... MLD".

Authors response: Title of the section has been changed to: "Role of the NIG circulation patterns compared to the interannual variability in MLD (focus on the region S3)"

(4) Why don't Authors overplot buoyancy loss anomaly w/ respect to average in Fig. 6 and refer to Fig. 6 in sentences like this one? Once again, words are more cumbersome to digest without a figure. (Add another axis on the left with % difference buoyancy loss).

Authors response: We agree that figures are better than words. However, we can't overplot Buoyancy loss anomaly on Figure 6 because it is not a time-series but an annual value per year (it is winter integrated). In addition, Figure 6 is already relatively dense with many time-series in the same figure panel, that is why we estimate that to give winter buoyancy loss values in a table is the best option for clarity.

(5) What is an entrainment bloom? Line 20.

Authors response: An entrainment bloom as defined by Cullen (2002) is a bloom driven by the deepening of the MLD in case of nutrient limitation. To be clearer the following definition has been added line 15 page 10.

"A common explanation for the fall and spring bloom is the succession of an entrainment bloom (i.e. bloom driven by MLD deepening in nutrient-limited conditions) in fall followed by a spring bloom (light-limited, Cullen et al. 2002; Levy et al., 2005)."

(6) Figures:

Figure 1 add the position of the NIG

Authors response: Thank you for your suggestion, it has been added.

Figure 2 caption. "(S1 – S2, see Figure 1c)" ->"(S1 – S2, see Figure 1b)".

Authors response: Thank you noticing this mistake. It has been corrected in the new version of the manuscript.

Fig. 3 caption. Maybe add "black dots indicate in situ stations used for the maps".

Authors response: Caption of Figure 3 has been modified accordingly:

"Climatological maps of the estimation of the depth of the isopycnal 28.9 kg m-3 for cyclonic (panel a) and anticyclonic (panel b) regimes. Data were spatially interpolated with kriging, black dots indicate in situ data sued for kriging. Relationship between the depth of the nitracline and the depth of isopynal 28.9 kg m-3 in the North Ionian Sea (panel c) from in situ data collected during three oceanographic cruises (see colors). "

Figure 5. Even though you have units spec'd in the caption, I suggest you add the units on top of the palettes, i.e. mg m-3, month (this not strictly necessary) and %. Always for the ease of the reader. It can be done quickly, e.g. w/ Powerpoint.

Authors response: Thank you for your suggestion, units have been added on the top of palettes.

Fig 6. Characters are a bit small, in the Fig. Please enlarge (in view of drastic figure reduction by editorial process). Also, please add units on axes.

Authors response: Thank you for your suggestion, we enlarged all characters in the new version of the manuscript.

Figure 7. Again, characters are small and isopycnal line almost invisible. Please enlarge chars, andthicken and change color to line

Authors response: Thank you for your suggestion. It has been done in the new version of the manuscript.

We would like to thank the referee #2 for its review and the interest he shows to our manuscript. In the following we answer the comments one by one.

(1) In the abstract: A suggestion could be to highlight the obtained results better.

Author response: Thank you for your suggestion, the following sentences have been added to the abstract

"Two trophic regimes were then identified in the NIG and they could be explained with the relative position of the MLD and nitracline. The first one is characterised by an early winter bloom onset and the absence of chlorophyll peak in March. It was observed when circulation was anticyclonic or when winter MLD was relatively shallow. A dominant regenerated production all year round and an absence of significant nutrient supplies to surface waters are proposed to explain this trophic regime. Conversely, the second trophic regime is marked by a bloom onset in late winter (i.e. February) and a peak of chlorophyll in March. The chlorophyll increase was interpreted as the direct response to nutrient enrichment of surface waters. This winter/spring bloom was observed when circulation was cyclonic and when winter mixing was relatively strong. "

(2) Introduction: In Page 3 - Line 15-16, authors should rewrite these sentences. Several authors of Mayot et al (2016) study coincide with the current work. The authors indicate that Mayot et al don't analyze the inter-annual variability, when the title itself is "Interannual variability of the ...". The authors should rewrite these sentences and specify what improvement or advance present this study with respect to the study of Mayot et al. (2016).

Athors response: We fully agree that Mayot et al. (2016) study interannual variability in phenology and that is what we implied in sentence line 15 : "More recently, Mayot et al. (2016) showed that phytoplankton phenology is extremely dynamic in the North Ionian Sea with significant changes in the annual cycle of surface chlorophyll-a concentration from year to year."

The improvement of the paper compared to the one of Mayot et al. (2016) is that it focuses on the causes of change in phytoplankton phenology which is out of the scope of the Mayot et al. (2016) paper. In particular, the impact of the decadal changes in BiOS circulation (switching from cyclonic to anticyclonic and vice-versa) on phytoplankton phenology is investigated in our paper.

To avoid confusion lines 15 and 16 have been modified to:

"More recently, Mayot et al. (2016) studied interannual variability in phytoplankton phenology in the Mediterranean Sea. Authors showed that phytoplankton phenology is extremely dynamic in the North Ionian Sea with significant changes in the annual cycle of surface chlorophyll-a concentration from year to year. However, they did not investigate the potential causes of this interannual variability. "

(3) Page 3, Line 13: Define CZCS and SeaWIFS.

Authors response: This is done in the new version of the manuscript. Thank you for the suggestion.

(4) Material and Methods: One of the main concerns I detect in the study is that they use chlorophyll concentration obtained from OC-CCI database that has been built using standard algorithms. It is known by all that in the Mediterranean, these algorithms do not work correctly and there are several studies, including some made by the same authors of this work, which indicate the use of specific chl-a algorithms for the Mediterranean Sea. Authors should use these algorithms (MedOC4, Volpe et al. 2007 for Case 1 waters and the AD4 algorithm for Case 2 waters type D'Alimonte and Zibordi,2003) or demonstrate, through a statistical analysis, that there are no differences in the results of this study using standard instead of regional algorithms.

D'Alimonte D. and Zibordi G. (2003). Phytoplankton Determination in an Optically Complex Coastal Region Using a Multilayer Perceptron Neural Network. IEEE Trans. Geosci. Remote Sensing, vol. 41, pp. 286.

Volpe, G., Santoleri, R., Vellucci, V., Ribera d Acalà, M., Marullo, S., and D Ortenzio, F. (2007). The colour of the Mediterranean Sea: Global versus regional bio-optical algorithms evaluation and implication for satellite chlorophyll estimates. Remote Sens.

On the other hand, I suggest that the authors can work with the Marine Copernicus database (OCEANCOLOUR_MED_CHL_L3_REP_OBSERVATIONS_009_073) that uses regional chlorophyll algorithms as well as a better spatial resolution (1km x 1km).

In fact, the authors use this type of data for temperature and salinity (Page 4, line 25).

Authors response: we agree that global Chl-a products are not the most adapted to retrieve accurate estimations of Chl-a in the Mediteranean Sea as they tend to overestimate Chl-a concentration in oligotrophic conditions (Chl-a < 0.5 mg m-3). Nevertheless, Mediterranean algorithms developed for Chl-a are based on the blue-green reflectance band ratio and universal atmospheric correction algorithms, similarly to the Chl-a CCI-CI V1 product (OC4v6 algorithm was used). As shown in the figure annex 1, with Volpe et al., (2007) and OC4v6 algorithms, Chl-a increases when blue-green band ratio decreases resulting in similar patterns in terms of Chl-a variations (see annex 2). As our analysis focus on the relative changes in Chl-a within the Ionian Sea, main results are unchanged with the utilisation of regional algorithms. To allow comparison with reference papers analysing satellite Chl-a phenology in the Mediterranean Sea (D'Ortenzio and Ribera d'Alcalà, 2009) which are also based on global Chl-a products, we decided to keep global OC-CCI products.

Nevertheless, we agree that we should warn the reader about this point and stress the limits of the data used. The following paragraph has been added to the manuscript page 4 line 10:

"OC_CCI [Chl-a] products are processed with the algorithm OC4v6 which has been fitted on a global dataset (O'Reilly et al., 2000). It has been demonstrated that this algorithm overestimates by a factor close to 2 [Chl-a] in oligotrophic conditions ([Chl-a] < 0.4 mg.m-3) and alternative algorithms have been developed for the Mediterranean Sea (Bricaud et al., 2002; Volpe et al., 2007). However, as well as the OC4 global algorithm, Mediterranean algorithms are based on the blue-green reflectance band-ratio model: [Chl-a] increases with a decrease of the blue-green band-ratio. Hence, similar patterns in [Chl-a] variations are obtained whith each class of algorithms. As the present study focus only on the intercomparison of [Chl-a] patterns in the Ionian Sea, the utilisation of global algorithm is valid. In addition, it allows the comparison with reference papers in Mediterranean phytoplankton phenology (D'Ortenzio et al., 2009; Mayot et al., 2016) which are also based on global [Chl-a] products."

Bricaud, A., Bosc, E., & Antoine, D. (2002). Algal biomass and sea surface temperature in the Mediterranean Basin: Intercomparison of data from various satellite sensors, and implications for primary production estimates. Remote Sensing of Environment, 81(2-3), 163-178.

O'Reilly, J.E., Maritorena, S., Siegel, D.A., O'Brien, M.C., Toole, D., Mitchell, B.G., Kahru, M., Chavez, F.P., Strutton, P., Cota, G.F. and Hooker, S.B., 2000. Ocean color chlorophyll a algorithms for SeaWiFS, OC2, and OC4: Version 4. SeaWiFS postlaunch calibration and validation analyses, Part, 3, pp.9-23.

D'Ortenzio, F. and Ribera d'Alcalà, M., 2009. On the trophic regimes of the Mediterranean Sea: a satellite analysis. Biogeosciences, 6(2), pp.139-148.

(5) Page 5, Lines 8-9. Why the authors use the value of 1uM to establish the nitracline? In addition, the authors should explain why they only work with nitrate and not with phosphate, when it is known by the scientific community to be a very important nutrient in the Mediterranean Sea.

Author response: Isoline 1μM is commonly used to estimate of the nitracline depth which separates surface nitrate depleted waters to deep nitrate repleted waters, in the global ocean (Cermeno et al., 2008) and in the Mediterranean Sea (Lavigne et al., 2013; Pasqueron de Fommervault et al. 2015). In practice, we found also this measure more robust than depth of maximum gradient to estimate nitracline.

This is true that Phosphate is considered the most limitant element regarding phytoplankton growth in the eastern Mediterranean Sea as N/P ratio is higher than the Redfield ratio. However, experiments have shown that phytoplankton growth is co-limited by nitrate and phosphate in the Mediterranean Sea (Psarra et al., 2005; Thingstad et al., 2005). As the quality of phosphate data in databases are poorer than the quality of nitrate data (because of the very small phosphate concentrations) we preferred to work with nitrate data. This choice is not unusual as previous Mediterranean Sea works were also based on nitrate (D'Ortenzio et al., 2014; Lavigne et al., 2013).

The following sentences have been added page 5, lines 13:

"Although it was demonstrated that phytoplankton growth is co-limited by nitrate and phosphate in the Eastern Mediterranean Sea (Psarra et al., 2005; Thingstal et al., 2005), only nitrate dynamic is considered here as the both elements co-varies and control phytoplankton growth (Ribera d’Alcalà et al. 2003) and as the quality of phosphate data in database is not good enough for such analysis. "

Cermeño, P., Dutkiewicz, S., Harris, R.P., Follows, M., Schofield, O. and Falkowski, P.G: The role of nutricline depth in regulating the ocean carbon cycle. Proceedings of the National Academy of Sciences, 105(51), pp.20344-20349, 2008.

Psarra, S., Zohary, T., Krom, M.D., Mantoura, R.F.C., Polychronaki, T., Stambler, N., Tanaka, T., Tselepides, A. and Thingstad, T.F.: Phytoplankton response to a Lagrangian phosphate addition in the Levantine Sea (Eastern Mediterranean). Deep Sea Research Part II: Topical Studies in Oceanography, 52(22-23), pp.2944-2960, 2005.

Thingstad, T.F., Krom, M.D., Mantoura, R.F.C., Flaten, G.F., Groom, S., Herut, B., Kress, N., Law, C.S., Pasternak, A., Pitta, P. and Psarra, S.: Nature of phosphorus limitation in the ultraoligotrophic eastern Mediterranean. Science, 309(5737), pp.1068-1071, 2005.

Lavigne, H., D’Ortenzio, F., Migon, C., Claustre, H., Testor, P., d’Alcalà, M. R.,Lavezza, R., Houpert, L. and Prieur, L.: Enhancing the comprehension of mixed layer depth control on the Mediterranean phytoplankton phenology, Journal of Geophysical Research: Oceans, 118, 3416–3430, doi: 10.1002/jgrc.20251, 2013.

Pasqueron de Fommervault, O., D'Ortenzio, F., Mangin, A., Serra, R., Migon, C., Claustre,H.,Lavigne, H., Ribera d’Alcalà, M., Prieur, L., Taillandier, V., Schmechtig, C., Poteau, A., Leymarie, E., BessonF., and Obolensky, G.: Seasonal variability of nutrient concentrations in the Mediterranean Sea: contribution of Bio-Argo floats. Journal of Geophysical Research, 120, doi:10.1002/2015JC011103, 2015.

D’Ortenzio, F., et al.: Observing mixed layer depth, nitrate and chlorophyll concentrations in the Northwestern Mediterranean: A combined satellite and no3 profiling floats experiment, Geophysical Research Letters, 2014.

(6) Page 7, Lines 1-8, The authors found a linear relationship between the depth of the nitracline and the 28.9 kg/m3 isopycnal. The authors should clarify if this relationship is for the whole year or only for certain dates, since it can be modified clearly depending on the time (mixed or stratification period). This issue is very important since then you use this relationship to build figure 7.

Author response: The linear relationship has been built with data collected over different seasons and years but always avoiding the winter mixing period (i.e. October 1991; April-May 1992 and April-May 1999). We agree that the relationship is not anymore valid if winter mixing reaches the nitracline. This is illustrated on figure 7 when nitracline is observed at the surface after a deep mixing event. In the new version of the manuscript we better warn the reader about the limits of this relationship: page 7 line 8 the following sentences has been added: "Data used here were collected at different seasons (i.e. in October 1991 and April/May 1992 and 1999) but always during the long period during which water column is stratified. As soon as vertical mixing reaches nitracline depth, this relationship is not anymore valid. Nevertheless, such events are rare and short in the North Ionian Sea."

(7) Page 9, Line 25-30. The authors should provide empirical data to support these hypotheses.

Authors response: We attempted to support these hypotheses with model data presented in Figure 7. In situ data presented in Figure 3 were not sufficient to analyse interannual variability.

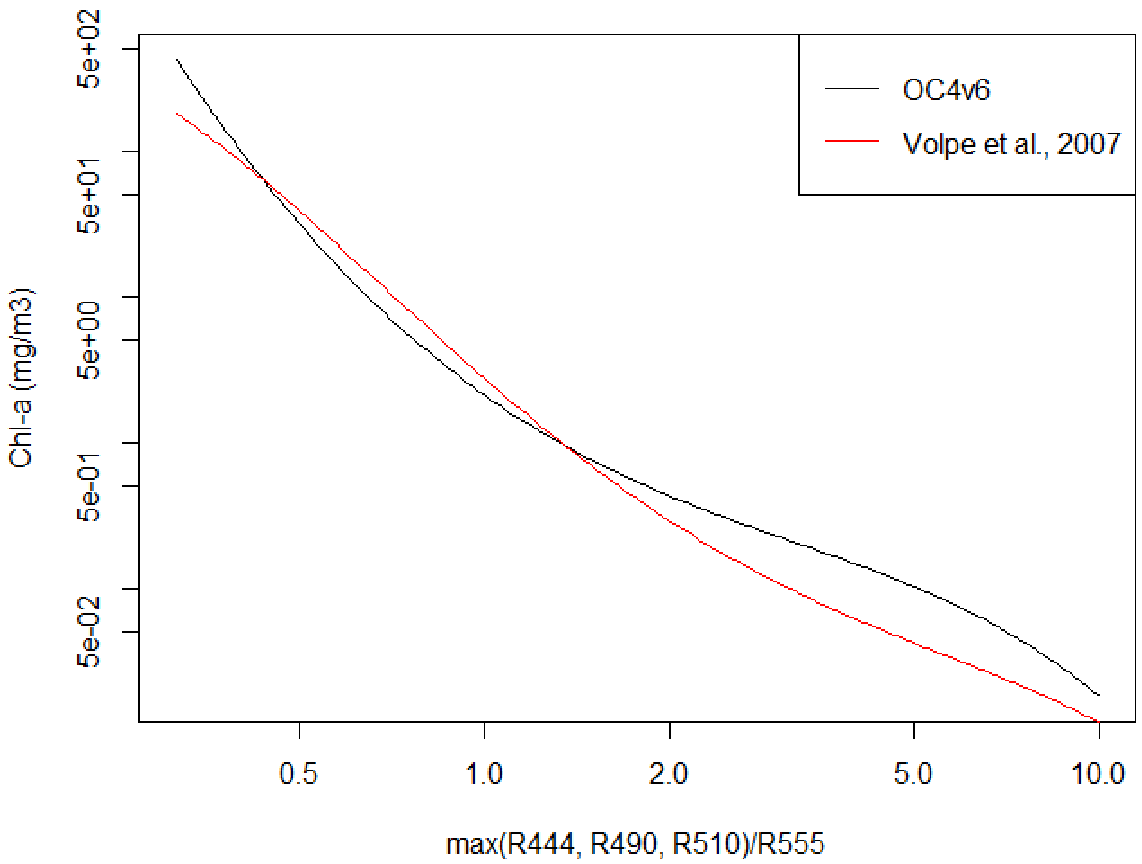
(8) Figure 4. Please, include in Figure 1 the line that has been performed the Hovmöeller diagram.

Authors response: Thank you for your suggestion, the line has been added in the new version of the figure 1.

(9) Write properly "Hovmöeller" throughout the text

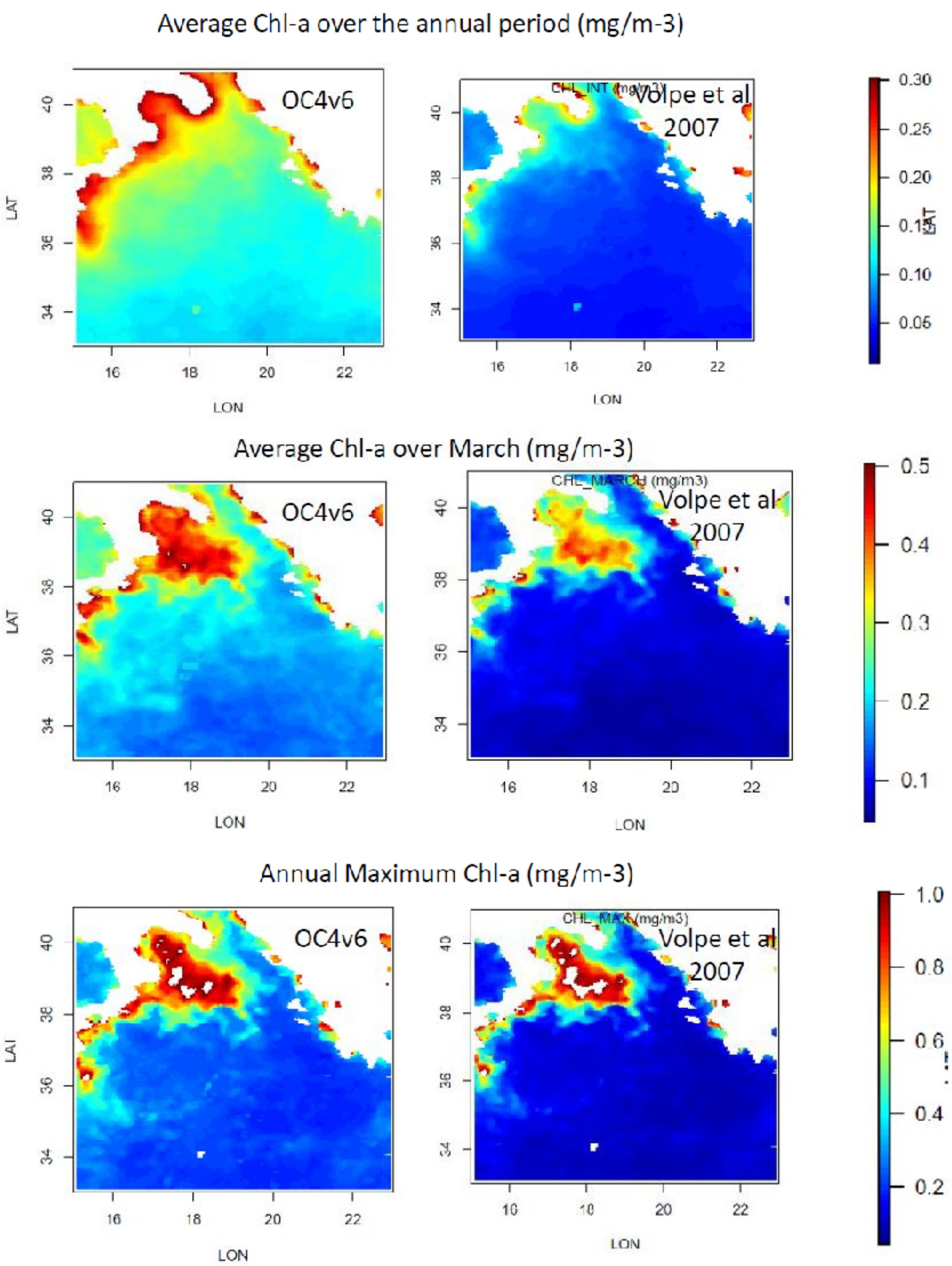
Authors response: Corrections have been done. Thank you.

Annex 1: Relationship between Chl-a concentration and the blue/green reflectance band ratio in OC4v6 and Volpe et al. (2007) algorithm.

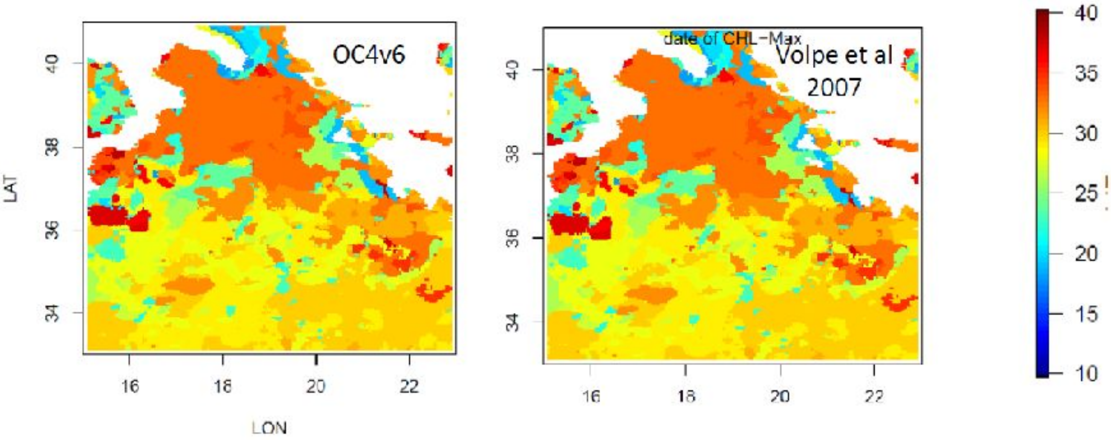


Annex 2:

Comparison of the phenological metrics obtained year 2004-2003 with (1) OC-CCI product (0C4v6 algorithm for Chl-a) and (2) Volpe et al., (2007) algorithm.



Date of Chl-a Max (number sof 8-day period since 2004/07/01)



Date of maximum growth rate (number sof 8-day period since 2004/07/01)