

Interactive
Comment

Interactive comment on “Chl *a* trends in European seas estimated using ocean-colour products” by G. Coppini et al.

Anonymous Referee #1

Received and published: 21 May 2012

General comment

The work by Coppini et al. presents a trend analysis conducted on European seas using satellite-derived chlorophyll-*a* concentration. The work is supported by in-situ measurements collected mostly in coastal regions. The context of the work is well introduced and the general framework of the analysis is clear. There are however some important parts of the analysis that are incomplete or unclear, and insufficiently discussed. More importantly the use of the considered satellite products for trend analysis is questionable. Particularly the suitability of the Globcolour GSM-based merged product for trend analysis is to be demonstrated, and the use of a regional algorithm developed for Mediterranean open waters to conduct a trend analysis on the Black Sea is not supported. The work uses a European-scale Chl*a* product and a Mediter-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



anean regional product, which is potentially interesting, but the analysis is conducted independently for the 2 products.

As a consequence of these issues, the interpretation of the trends illustrated in the study is hard to make and could even be misleading. These issues and associated recommendations are further developed in the specific and technical comments below.

For this study to be considered for final publication, the authors must thoroughly address these points, which likely entails a large amount of revisions (including in terms of data sets used) and/or additional analyzes.

Specific comments

The analysis of trends in ocean color products has to consider the way these products have been created (see more details below on Section 2). For a series based on one sensor, this means making sure that the series has been created with one processing chain, including a consistent calibration table and calibration history. This has to be acknowledged in the paper and supported by appropriate references (e.g., Eplee et al. SPIE 7452, 74520X, 2009, Meister et al. IEEE, 50, 310-319, 2012, and ref. therein).

Using a merged product adds the issue of inter-mission biases. If there is a systematic bias between the products derived from different missions, and if the merging is performed without specifically accounting for this bias, then spurious trends might be generated. Such biases do exist and can vary between regions; they can be seen on the NASA OBPG web site but in that case they are given only for very large regions. The implication is that, without a dedicated analysis on the regions of interest, the suitability of this type of products for trend analysis is unclear. To my knowledge, the Globcolour GSM-based merged product does not comply with this requirement for trend analysis. This means that the trends documented in the paper cannot be unambiguously/fully associated with natural variability, which questions the value of the work. Unless the authors can demonstrate that inter-mission biases do not create spurious trends in the products, or that they can correct for them, then the study has to consider

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



another product (like a SeaWiFS-only series).

Another weakness of the analysis is to use products derived from algorithms developed for open ocean waters for coastal waters and marginal seas, including the Baltic and Black Seas that are notoriously challenging for ocean color and bio-optical algorithms. This is acknowledged in the manuscript but the implication for what concerns the derived trends is not discussed. The scatter plot given for the GSM product on Fig.4 is fairly encouraging, even though validation statistics are varying between basins. There is also a validation analysis performed on the Med regional products for Mediterranean waters. But the use of a regional algorithm developed for Mediterranean off-shore waters to conduct a trend analysis on the Black Sea (including its shelf) is not supported. The analysis of Fig.3a should be removed unless additional elements can demonstrate the appropriateness of this algorithm for Black Sea waters.

The work uses a European-scale Chla product and a Mediterranean regional product. This could allow for a comparison between trend results associated with each data set. But Fig.2 cannot be compared with a Mediterranean equivalent based on the regional product. Fig.3 is only for the regional product, and again Fig.11 has no equivalent for the regional Chla. This would be an interesting point of discussion and could lend confidence to the results (if the trend results are consistent).

My fourth general comment is that there are too many technical points which are unclear. This is mostly true for Section 4 (but not only).

The Section 2 “Data and Methods” should be completed. A part should deal with the in situ data (see below in Technical Comments).

The processors associated with the satellite data sets should be mentioned: which SeaDAS (for SeaWiFS and MODIS) and MEGS (for MERIS) versions? Are the data sets created with a consistent version of the processor (including calibration) through the entire time series?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

Satellite products are provided as daily maps for the GSM one, and daily, then monthly, for the Med regional one (by the way, considering that CNR staff are co-authors, it is surprising that daily maps are not available for the full data set). How is the transition to summer values actually performed? With monthly means as intermediate values? If the daily values are used directly to compute summer means, there might be biases created from one year to another if these daily values are unevenly distributed in time. Is there a check on the distribution in time of the valid Chla values? In any case, is there a threshold on the minimum number of days required to create a valid summer value (I'd argue there should be one)? In general the 'data content' of the analysis should be thoroughly documented. These post-processing aspects are important to avoid creating spurious trends.

The Chla areas should be better documented, typically by a map or from a reference where the reader can visualize them. This would be useful to fully benefit from Section 3.2.2.

For Section 4, the validation strategy should be clearly introduced, and the different analyzes well documented and explained: table with validation statistics, explanation of how averages are computed for the trend analysis, etc. . . . These points are given in detail in the Technical Comments below.

Technical comments:

They are provided per page and with line numbers.

In general, the abstract should be improved, following a logical sequence. For now, 'this work aims to develop a new indicator' (1st paragraph) and then (2nd paragraph), 'this work proposes a methodology', with in between, considerations about validation, the potential of remote sensing or the advantages of a regional algorithm. The last part, about 'Chla areas', is not clear if the reader has not read the paper.

p.1482

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

I.14: 'transformed into Chla'

I.18: 'but': it is not clear why there is opposition between the 2 parts of the sentence.

p.1483

I.24: 'variables to be monitored'

p.1484

I.22: please check the URL.

I.23: there is no end to the sentence.

I.27: 'fluorometer'

p.1485

The description of the in-situ data should be done in Section 2, with a minimum amount of information: sources, number of points, coverage in time & space (referring to Fig.1), discussion of their uncertainties. . . .

I.6: 'fluorometers'

I.22: the authors forget that there were other ocean color sensors between CZCS and SeaWiFS (MOS, OCTS).

I.26: 'Siegel'

The last paragraph would be better located in the description of the satellite products.

p.1486

I.17: 'colored dissolved organic matter'

I.22: 'these types'

I.23: 'analyses . . . are ' or 'analysis . . . is' I.25: what do the authors mean by 'consistent'?

p.1487

I.1: 'present'

I.7: 'The purpose of the paper is to:'

p.1488

I.14: I'd suggest: 'The product was obtained from ...'

p.1489

I.18: Gilbert 1987 and El-Shaarawi are not found in the references list.

In note 2: 'the Sea of Azov has ...'

p.1490

I.15: 'statistically'

I.21: 'summer values'

p.1493

I.4: 'negative west-to-east' ?

I.12: '... the values by the satellite products.'

The 2 points introduced at the beginning of Section 3.2 were already given before (Section 2.2, and introduction of Section 3). Please remove the part that is not necessary.

p.1494

In Section 3.2.1, it is worth underlining that the standard deviation of Chla (in mg.m-3) is naturally high in regions of high Chla. Looking at relative changes could give a modified picture.

I.24: what is the basis for the selection of some areas? A map of the Mediterranean Sea would be useful to compare with the results of Fig.2.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

I.25: I'd suggest : 'for presentation in Fig.3'.

I.26: '80% of Chla areas do not show significant trends.' : which means for none of the associated grid points? Or for too few?

p.1495

I.1: I'd suggest 'Increasing Chla trends (i.e., found for more than 10%....' if this is what is meant.

I.5: the 2nd 'Strait' to be removed?

In this part, the acronyms should be defined (from a map or a table). Again, the paper should allow for a visualization of the regions.

p.1496

The validation of satellite products (direct comparisons with in situ data) could be better placed after the description of the data, and before the description of trends. Moreover, comparing monthly satellite products and in-situ values in coastal waters is really questionable.

I.11: The use (and usefulness) of Table 2 is not so clear.

p.1497

I.1: 'averaged': in space. In time as well?

I.7: 'products differ'

I.10-11: 'without rounded at the minutes value': what does this mean precisely? ('without rounded' is incorrect grammatically).

I.15-16: match-ups exhibiting large differences are 'eliminated'. This filtering excludes 16% to 21% of the match-ups, which goes beyond the exclusion of a few outliers. This issue should be better supported. Providing validation statistics for the filtered and unfiltered match-up sets would be appropriate.

Last paragraph: At that point, it is not clear for the reader what the ‘tests’ are. I’d suggest that the validation strategy for the analysis be clearly and completely introduced earlier. Then the description of results would appear clearer.

p.1498

1st paragraph: Fig.5 to 8 could be removed, or at least reduced to 2 by using different colors for the match-ups from different basins. More importantly, validation statistics should be provided with 2 digits for R^2 and slope/intercept of linear regression. They should be completed by the number of points and indicators of differences (typically RMSD and bias). A table containing these statistics for the European seas and each basin would be appropriate. The same point applies to the regional product.

2nd paragraph: I am not sure that Test 2 (and Fig.9) is useful. It is for one year and mostly qualitative.

3rd paragraph: There are problems of signs with the trends. On the figures, some trends are negative whereas they look positive. Fig.10 to 14 could be combined in one plate. What is the degree of significance of the trends of Fig.10 to 14?

I.12: How are summer means and trends computed here? Are all in situ data just averaged for the summer period? Are the satellite products averaged for the locations corresponding to in situ data or for the different basins? Is it possible that the observed trends are partly the result of a varying spatial coverage? It is essential to be very clear about the areas concerned by Fig.10 to 14, and about the matching of satellite and in situ trends, to be able to interpret these results. From Fig.10, one might deduce that Chla increased by 1.6 mg.m⁻³ over the last 10 years (i.e., more than doubled) for the European seas. The least the authors could do is to comment on that. The fact that the trend obtained for the European seas is larger than the trends obtained for the single basins is puzzling and worth discussing.

I.16: ‘where no significant trend’

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

I.17: 'similar': this description is questionable: the trends differ by a factor 2 to 3.5. 4th paragraph: An equivalent to Fig.11 for the regional product appears as a logical thing to do. The scatter plot on Fig.15 seems to show that there is an overestimate for high Chla values.

I.25: 'preferable with respect'

p.1499

I.17: 'investigations need to be performed to compare the trends at the level of Chl a areas': I would have expected to see some of that in this paper.

I.25: 'consist'

References:

Lavender et al and O'Reilly et al are not cited in the paper.

Table 2:

Test 1 / Aggregation level: I'd say 'daily'.

Test 3: 'at the level of European seas'

'no significant trend is detected'

Test 4 / Aggregation level: 'daily' or 'monthly'?

Interactive comment on Ocean Sci. Discuss., 9, 1481, 2012.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)