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

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Received and published: 21 September 2012

Each one of the comments of the referee #2 is marked with a bullet (-) and it starts with ‘RC:’ (referee’s comment). The authors’ replies follow each comment and start with ‘AR:’ (Authors Reply).

- RC: The author say that the ocean colour products ‘are calibrated for open ocean waters and not specifically for coastal waters, and a lower performance is therefore expected’. Since the vast majority of the Eionet in-situ data used for the comparison are collected along the European coasts, a careful discussion of the error of the algorithms (calibrated for Case 1 waters) and data (representative of Case 2 waters) must be carried out. A comparison between the products used in this study in some areas where ocean colour has been processed with Case 2 coastal algorithms will be very

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beneficial.

AR: We agree with the referee and we underline now in the paper in section 2.1 'Satellite and in-situ data sources' that the complexity of optical properties of the European coastal zone requires the use of specialized local algorithm and processing system but these products are presently not available at pan-European level and therefore we used the MyOcean global and regional products to investigate trend in both open ocean and coastal areas of the European Seas. The availability of in situ coastal data will be use at posterior to check potential use of MyOcean ocean colour products to estimate trends also in the coastal waters. Following the advice of the referee we have added a part of the text that highlights the error of the algorithms and available in situ data (see section 2.1).

- RC: As the author correctly declare, the OC-in-situ data comparison has made only during Summer: it is positive for the Southern European Seas, as the river loads impact on the coastal waters is limited but badly affects the results in areas such Baltic Sea where the runoffs in this period are relevant. There is any difficulty to extend the comparison at least over 9 months?

AR: Following the advice of the referee we have extended the comparison to the entire year (see Section 3 Validation against in-situ data and new Table 3). To be noticed that the availability of the satellite data during autumn-winter-spring periods is lower in the northern region and therefore we do not increase considerably the number of corresponding satellite and in situ data.

- RC: The statistical approach in the trend detection procedure is correctly based on a nonparametric statistics (Sen's method), but as far as I understand the of the standard deviation is made according a normal (parametric) distribution hypothesis for a stationary stochastic process. This rises a amin concern about the estimation of $\bar{\chi}$ (pg. 1490 r.18): the mean value is calculated over time i.e. $\langle \text{Chl} \rangle = \langle \text{Chl} \rangle_{\text{time}}$. This mean estimate imposes the stationarity and the ergodicity of the process, which is not the case

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if the presence of the slope is statistically detected by Sen's algorithm and corroborated by the Mann-Kendall test. In other words, it is incorrect to calculate a statistical mean if the time series has a temporal trend. Therefore the right plate in picture 1 is wrong, but possibly all the other considerations must be seriously reconsidered. Other approaches should be considered in order to guarantee a better statistical consistency.

AR: We understand the referee's concern but we would like to explain our approach. We calculated the Chla climatology using the standard approach of 12-year average of the Chla as it is for instance similarly proposed in Vanderpotte and Melin 2010 for the Global SeAWiFS dataset. Moreover we would like to explain that the normalization of the Chla trends by its STD has been done with the main scope of being able to provide a Pan-European picture of chl-a trends and include in the same map areas with different characteristics: high Chl-a concentrations, high variability and high absolute trends (i.e. Baltic Sea) and areas with low Chla concentrations, low variability and low absolute trends (i.e. Mediterranean Sea). Trends normalized by the STD allow a better visualization and provide a more readable Pan-European picture. The STD is not affecting the statistical analysis used to estimate the trend (Sen's slope) and the statistical analysis used to estimate the significance of the trends (Mann-Kendall) because STD is applied only as a last step in the calculation of the CSI023(+). Finally following the referee's comment we have calculated the STD also according a non-parametric approach and we have calculated CSI023(+) now with the non-parametric STD. Formula 4 at page 1490 has been revised accordingly and figure 2 (now figure 4) has been substituted with the CSI023(+) calculated with the non-parametric STD and with the non-parametric STD. Differences between the parametric and non-parametric STD are small and the CSI023(+) calculated with the non-parametric STD shows in general stronger trends both in the areas with negative and positive trends but a very similar general pattern.

- RC: The CSI023+ has been compared only in coastal areas but the performances of the indicator should be tested against the literature that exists in some open-sea areas.

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No effort is made under this aspect.

AR: As the referee clearly underlines we tested the products used to calculate the CSI023+ with available in situ datasets at Pan-European scale. The corresponding in situ and satellite matchup used to calculate the summer and yearly mean at the comparison location are reported in table 3 (last column). This in situ dataset represent an independent dataset to be compared with MyOcean ocean colour products as we do in this paper. Additional validation has been done by MyOcean project itself especially in open ocean waters for the validation of the ocean colour products. Following the advice of the referee we have underlined this aspect in section 2 where the validation done by MyOcean is cited for the 3 satellite products used in this paper.

- RC: The correlation plots (fig. 4-8) seem to be ‘shaved’., and all the OC data whose difference with the relative observations is above a fixed threshold have been eliminated. Which are the original plots? Is it possible to demonstrate, if I’m right, that the cancelled data are really outliers?

AR: We agree with the referee and following the referee’s advice, also shared by the referee#1, we decided to show the original plots without applying any filtering. The entire section 3 has been revised in this sense, new figure have been prepared (Figure 6,7,8 and 9).

- RC: Some formal points: The abstract is too long, contains comments and future developments issues that generally belong to the text. The introduction is vague, largely focused on eutrophication, subject that is only marginally touched during the rest of the paper.

AR: The abstract has been revised following the advice of referee. Also the introduction was simplified and is now less focussed on general issues related to eutrophication. We believe that now introduction is more balanced and focus on the arguments treated in the paper. We remover the following phrases: - page 1484 line 3: ‘The WFD requires the achievement of good ecological status or good ecological potential of transitional

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and coastal waters across the EU by 2015, while the MSFD's aim is to achieve good environmental status of the EU's marine waters by 2020.'

- page 1484 line 8: 'aimed at reducing nitrate pollution from agricultural land'
- page 1484 line 9: 'aimed at reducing pollution from sewage treatment works and certain industries'
- page 1484 line 11: 'aimed at controlling and preventing pollution of water from industry'

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

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