

Interactive comment on “NW Adriatic Sea variability in relation to chlorophyll-a dynamics in the last 20 years (1986–2005)” by L. Tedesco et al.

L. Tedesco et al.

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General comments

We thank the Referee 1 for his/her detailed comments.

We agree with the Referee that this is only part of the work, yet it is a first fundamental step for better understanding the northern Adriatic long-term trends as part of the inter-annual variability. We also mentioned in the Conclusions section (page 668, line 19–22) that this work will be integrated with an analysis of the abundance, biodiversity and distribution of phytoplankton species to look for any possible change in communities composition. We also suggested the importance of coupling this kind of study with the analysis of zooplankton cycles, as an essential controlling factor of the phytoplankton growth (page 668, line 26–29).

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As mentioned in the introduction (page 654, line 23-30), the aim of the ms was, firstly, to analyse long-term changes, especially regarding the chlorophyll-*a* trends in relation to the physical variables, since very few such long data set analysis of the NW Adriatic have been published before (Bernardi Aubry et al., 2004; Degobbis et al., 2000). Secondly, since no trends were detected at both stations for chlorophyll-*a*, we aimed at describing the intra-annual variability of this variable, since such kind of analysis, based on two decades of data and performed on monthly scale, was missing in the scientific literature as well. We understand that the wide meaning of the word "variability" in the title of the paper can cause some misunderstanding. We will thus add more explanations in the text to clarify this aspect.

Specific comments

1. The small regional scale does not allow us to appreciate differences in meteorological variability, since long-term reanalyses (e.g. ECMWF ERA40) have coarse spatial resolutions. The focus of the ms was on the analysis of the variability connected to ocean physical features, and not to explore the climate-related variability of a coastal region. The data we are analyzing come from two stations located 10 and 20 nautical miles from the coast and that have a maximum depth of 29.5 and 32 m (as mentioned in the section Sampling and Methods, page 655, line 4–6). To our knowledge, there is no clearly proven correlation between climatic indices (e.g. NAO) and coastal areas in the Mediterranean Sea. Furthermore, in a recent work of Rixen et al. (2005) on NAO correlation in the Mediterranean Sea, it is mentioned that data closer than 15 km from the coastline or in areas shallower than 50 m were rejected to avoid biases by coastal processes in the interpolation algorithm.

About the data set, we also mentioned in the Sampling and Methods section (page 655, line 9–13), that some data were missing in the time series, but also we explained that since the northern Adriatic Sea is characterized by short-term variability, we decided to analyse the data on a monthly scale, even if winter months were less represented. However, as it is shown in Fig. 3, the number of data per month are comparable. This

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is also discussed at the beginning of the Discussion section (line 23–29).

About the Po runoff long-term data, we have analysed the correlations between the sea water temperature and salinity and the Po river discharge (C10: Temp-Po $r=-0.332$ $p<0.001$, Sal-Po $r=-0.168$ $p=0.048$, E06: Temp-Po $r=-0.274$ $p=0.002$, Sal-Po $r=-0.194$ $p=0.031$), but we did not include those since we did not detect any trend in chlorophyll-*a*. Following Referee 1 suggestion, we will show the results as additional electronic material to our ms (<http://flux.ve.ismar.cnr.it/ibm/html/socal/data/aem.htm>). Instead, we presented only Fig.2 since we believe that a plot with monthly average and standard error and deviation is sufficient to depict the high variability of the Po runoff. We included the plot in the ms to relate the seasonal cycle of the Po discharge with the analysis of the other variables in the part of the ms where we analysed the intra-annual variability.

2. The data set we studied in our ms has never been completely analysed, discussed and published previously, neither for the long-term trends or for the intra-annual variability. Considering that our results bring us to different conclusions than some previous literature does, we consider this ms relevant for future studies on the northern Adriatic biogeochemistry and on its effects on the productivity of the rest of the Mediterranean basins.

3. Following we summarize our main conclusions that will be further added in the ms, as also Referee 2 has suggested:

- (i) generally, the two stations cannot be considered trophically different, as some previous literature had done. Our data analysis show similarities between the two areas, concerning the chlorophyll-*a* response to different physical and biological features.
- (ii) the two stations may be characterized by different controlling factors regarding the chlorophyll-*a* dynamics.
- (iii) our data analysis show that phosphorous may not be a factor limiting the growth

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of the NW Adriatic phytoplankton.

- (iii) there is an agreement between the EMTS and the Cox-Stuart Test for the physical variables. Furthermore, we present explanations (and some possible future scenario) regarding the response of the two areas to EMTS (Section Discussion, page 663, line 7–14).
- (iiii) we also show that nutrients concentration is not a sufficient criterion to characterize the trophic differences between different areas (section Discussion, page 664, line 4–7).

We believe that those above are rather substantial conclusions of our ms.

4. As we mention in the last paragraph of the answers to the initial general comment of Referee 1, we looked for long-term trends of chlorophyll-*a* in relation to the physical variables and we used a proper statistical test (Cox-Stuart, subsection 3.1) to do so. We did not study the anomalies since we did not find any trend in chlorophyll-*a* and our interest was then to analyse the intra-annual variability. About the Po runoff, see our answer in the last paragraph of 1.

5. See our answer to 1 and 3. Besides, we believe that TRIX is not a suitable index to use for our data set. A paper of Giovanardi and Vollenweider (2004), that have published on TRIX for long time, compares two coastal areas in the Adriatic and Tyrrhenian seas that are considered trophically different. In their work, the authors show the potentiality and the limitations of using TRIX. Despite the fact that TRIX, as a synthetic index, works as an integrating function and summarises more than the sole variability expressed by the single components, the authors find apparently similar trophic levels for the two areas. They explain their results since TRIX, as a static index, may indicate either the actual (for the Adriatic) and the potential (for the Tyrrhenian) productivity of the area. They also mention the importance of considering other factors, such as the photic regime and the water column stability, that co-determine dynamics and be-

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haviour of the systems. They finally cite a work by Platt (1981) that states that a "a static representation of a dynamic system is of but limited utility". Instead, we used PCA and Ridge Regression tests, as sophisticated statical tools to relate, not only chlorophyll–a to oxygen and nutrients loads, but also to physical oceanic variables, such as temperature and salinity, and to the depth of the areas, to explain surface/bottom processes. We believe that this kind of analysis is more suitable, informative and complete for our data set.

8. We are in favour of shortening the title of our ms, as suggested by the Referee 1.

We thank Referee 1 for all the other BG questions to which he answered positively. We are looking forward to receiving further explanations about the answer of the Referee 1 with "no" to question 15.

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