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Biogeosciences Discussions

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.

Anonymous Referee #3

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I think that the paper overall contains useful information because it incorporates a 20 year time record of data that is important to be presented in the published literature. However, I do not think that paper is ready for publication. It needs significant revision before being accepted for publication.

Based on the title, abstract and conclusions I expected that this was an analysis of the long term trends in hydrographic, chemical and biological variables at these sites. That is not the case. This is a 20-year climatology of the annual cycle of these variables at these two sites. This should be clear from the title, abstract and conclusions. I strongly urge the authors to change these three components to reflect the true nature of the paper.

A set of papers have just come out in a JGR special issue on the northern Adriatic. I suspect that the authors may not had access to these as they were preparing this manuscript, but given that the paper needs revision, it would be useful to include references to at least a couple of these papers to help explain the variability that is being described in this paper. Jeffries and Lee (2007) discuss the physical climatology of the northern Adriatic. Mauri et al. (2007) discuss modes of variability from the MODIS chlorophyll including discussions of the variability in this region and correlations with wind forcing.

The order of presentation seems a little peculiar. In the results section the authors jump right into a discussion of the Cox-Stuart test which is a statistical evaluation of trend. This is before the actual data have been presented in figures 4, 5, and 6. The fact that the statistics preceded the data presentation confused me initially because I wasn't sure what the authors were trying to say. I was still thinking in terms of a multi-year trend, versus a climatological seasonal trend.

It also seems to me that the authors have tried to use several statistical tools to show what is obvious in the data set. The statistics get in the way of clarity. Simply plotting the nearsurface temperatures as a seasonal line plot would more clearly show the seasonal temperature changes than all of the elaborate statistical analysis and corresponding tables that have been provided. I don't think that you need to employ the Cox-Stuart analysis to see the trends that are apparent.

Specific comments:

The figures as they print out from the PDF are nearly impossible to read - the numbers are much too small. This is especially true for Figures 5, 6 and 7, which are key figures in the paper.

Figures 5 and 6 - I strongly recommend that you make the color scales the same for the two sets of figures. It would be much easier to compare the two stations not only for trends, but also for the specific magnitudes of variation between the two stations.

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When I magnified the figures in Acrobat to the point where I could read the numbers, it was clear that specific contour intervals showed up as different colors in the two figures. For instance, in the two salinity plots the range for Figure 5 (C10) was from 35 to 38 psu, but for Figure 6 (E06) the range was from 32 to 38 psu. This is a significant difference. If both figures had the same color scale this would have been evident. The following variables should have these scale changes: Figure 5 Figure 6 Sigma-t 24-29 Kg/m3 22-29 Kg/m3 NH4 1-2 μM 1-4 μM NO2 0-1.25 μM 0-1 μM NO3 0-6 μM 0-21 μM DIN same as for NO3 PO4 0-0.15 μM 0-0.2 μM Si(OH)4 0-10 μM 0-18(?)μM DO OK Chlor. 0-2 μg/dm3 0-3.5 μg/dm3

For consistency make the color scales consistent with whichever plots have the larger range so that one color range will work for both sets of plots.

The authors have referred to Figures 5 and 6 as Hövmöller plots. I don't know if this is technically correct. The name Hövmöller plot typically refers to time/longitude or time/latitude plots, not time/depth plots. I have not seen a time/depth plot referred to as a Hövmöller plot before.

Figures 5, 6, and 7 - it should be stated in the figure legends that you are plotting the medians. It took a little digging, but I did find where you stated it on page 656 (line 20).

Page 653, Line 9 - "frontal system located 8-16 km from the coast". I think that this distance is a bit arbitrary given the recent evidence published by Lee et al. (EOS, 2005), and Mauri et al. (JGR, 2007). This distance is highly dependent on the type of physical forcing affecting the dispersion of the plume from the Po and other river systems, whether it be a Bora wind, or a Sirocco wind.

Page 654, Lines 4-6. Again, it appears that the authors are setting the reader up for a long-term multi-year analysis of the data based on these lines, particularly the reference to water temperature rise.

Page 654, Lines 24-26 (Objective (i)): Again, you state that you are analyzing the "long-

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term variations in chlorophyll a". This suggests a multiyear analysis of trends. It must be made clear that this is a 20-year climatology of the annual variability of chlorophyll and related hydrographic and chemical variables.

Page 658, Lines 4-6. "The high spatial and temporal variability Ě.. needs to be analyzed at the proper scale." This implies that the system has been measured sufficiently well resolving both space and time such that you can clearly state what those scales are (determined perhaps by decorrelation time and space scales - e.g. Jeffries and Lee, 2007, JGR). I would drop this statement since this evaluation is not provided, nor has an appropriate reference from the literature been provided.

Page 658, Line 24 "In the last 15 m of the water column" - I think you are referring to the bottom 15 m of the other water column. "Last" is not very descriptive in terms of the vertical structure. Similarly, "first" in line 13 might be better stated as "top".

Page 659, Lines 22-24. It looks to me like elevated Si(OH)4 extends throughout the water column in January and there is not evidence (Fig. 5i) of low salinity in January as stated in the text of lines 22-24.

Page 659, Line 2-3 "Close to hypoxic phenomena" My understanding of hypoxia is that it corresponds to concentrations below 30% of saturation. Based on that I would not call the plotted data "close to hypoxia". It is certainly well below saturation, but not what I would call hypoxic.

Page 661, Lines 16-19. "investigate which factors drive the variability". The PCA is simply giving a sophisticated type of correlation. Therefore the relationships among the variables can be correlated, but correlation does not directly imply forcing ("driving").

Page 662, Line 2 - add references if possible.

Page 663, Lines 12-13 - "since no effect is detected of the Po water spreading there during autumn." I disagree with this statement. The salinity at C10 as contoured in Figure 5b is less than 38 psu throughout the upper 10 meters throughout the year. If

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not Po water, at least there is the influence of river water continouously. Any salinity less than 38 psu is indicative of the presence of river water from some source whether it is the Po or another river. Since what is plotted is the median of the data, it means that the presence of river water in the upper 10 meters occurs at this station at least 50% of the time throughout the time record.

Page 663, line 25 - “fitted” should be “fit”.

Page 664, line 11 - Line 11 and Page 663, line 12-13 seem potentially in conflict.

Page 664, line 20 - I would reword this as “Generally, the nutrient concentrations are higher in the nearsurface region where the lowest salinities are observed. This is consistent with the role of the rivers as the major nutrient sources for the region.”

Page 664, line 23-24 - Is the ammonium peak due to the release by the phytoplankton or by the grazers that are feeding on the phytoplankton? I would think that the grazers are the actual source of the ammonium as they digest the phytoplankton.

Page 665, 1ines 1-7 The two stations are within the Po Plume gradient in both time and space that will affect the concentrations (increasing dilution away from the source) and the evolution of the biological community within the plume. I think that this difference between the two stations is expected.

Line 10 - Again, an appropriate reference for the rapid PO4 cycling would be appropriate here.

P. 666, line 1 “periodical” should be “periodic”

P. 666, line 24 “temporal trend” - I would replace this phrase with something like the “climatological seasonal variability”

P. 667, Line 9 - Usually a chlorophyll of $<$0.5 μg/l is used as a guideline for defining a system as oligotrophic on the basis of biomass. Since the concentration in the surface layer at E06 is all $>$0.5 and mostly $>$1 μg/l, I would not consider it oligotrohpic. Station

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C10 is borderline with it going below 0.5 μg/l at two depths in August.

Interactive comment on Biogeosciences Discuss., 4, 651, 2007.