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Biogeosciences
Discussions

Interactive comment on "Long term changes in the ecosystem in the northern South China Sea during 1976–2004" *by* X. Ning et al.

Anonymous Referee #1

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General comments

This paper reports a set of valuable time-series data of hydrography, nutrients and biology from the continental shelf off the Pearl River mouth in the northern South China Sea over a time span of nearly 30 years. Many parameters showed significant trends of changes over the years, which allow us to understand how the marine ecosystem responds to human perturbation in the anthropocene. The perturbation may come from direct impacts, such as human enhanced nutrient loads carried by rivers, as well as indirect impacts, such as global warming. Consequently, the impact processes are complicated and the marine ecosystem responses are equally, if not more, complicated. Therefore, it is inherently difficult to delineate the processes responsible for the observed changes. Paradoxically the science will make little progress, if the authors merely report the observed changes without serious attempts to attribute the cause-effect relationships or to find the cross correlation among various aspects of the marine environmental changes. In fact, the authors who supposedly understand the system best are charged with the duty to provide at least a conceptual framework to interpret the observed changes in terms of plausible mechanisms responsible for the changes. At the least, the authors need to demonstrate the ecosystem responses are compatible with the changes in environmental conditions that may have caused the changes in biology. Unfortunately, the authors did not give us much to chew on aside from a few trivial attributes in interpreting their valuable data set. In addition, a few additional items of ancillary data, which are missing, may help to better depict the overall picture. To raise this contribution above the level of a data report, the authors are encouraged to identify the most significant ecosystem responses demonstrated by the data set and to provide a plausible mechanism that may produce these responses.

Specific comments

1. Introduction

1.1. The authors rightfully reported the riverine contributions to the study area because of the proximity of the study area to the coasts with high river discharges, but the authors reported only the average discharge and mean chemical loads. It would be illuminating that the time series of river discharges and, perhaps, the nutrient loads are also reported. Since the authors discussed the inter-annual variations of the environmental parameters, the fluctuation of river discharge could have caused some of the observed variations. Any long-term trends in river discharges could be responsible for the observed trends.

1.2. Line 27: [29 y term series] should be [29 y time series]

1.3. It is preferred to use only geographic names instead of geopolitical names for description of the study area in scientific papers.

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2. Data and methods

2.1. The data were collected over a long time span. It is worrisome whether the older data are compatible with the more recent ones. It would lend much confidence to the data set, if independent observations can be given to corroborate the evidence provided by the authors. For instance, the air temperatures at nearby coastal stations or the long-term data of global SST may be compared with the temperature trend reported here. It is also useful to list the parameters in a table with description of the method of measurement and their precisions.

2.2. The authors are thoughtful to present the average values of environmental parameters in three ways, the sea surface values, the water column averages and the value at 200 m. However, it would be instructive to show the mean sectional distributions of a few key parameters, such as temperature, salinity, DIN and Chl-a, along the six-station transect for summer and winter respectively. These sections would reveal many important features of the data set, such as the topography of the study area, the structure of the observation grid, the surface and subsurface contrast, the seasonal changes in stratification, etc.

2.3. The description of the method for calculating t the average values are not clear. Equation 1 only describes the calculation of water column average. Even this description is problematic. Does b represent the water depth or 200 m, if the water depth is greater than 200 m? Because there are six stations instead of one, it is not clear how the averaging over the six stations is done. For sea surface values, one may calculate the average as Xav = (X1 + X2 + ...+ X6)/6. For water column properties, there are possible ways to do the calculation: (1) the same as above except Xi represents the water column average instead of a single observed value from the surface, and (2) Xav may be calculated from a 2-D integration divided by the area in the integration domain. The two methods do not produce the same results and have different meanings. The method description need be clarified.

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3. Results: If the data for different depth levels are sufficiently distinctive, it may provide more insight to plot the data of each parameter in one plot instead of three. This should reveal the relative changes in different water levels. At the least, the surface and the bottom data should be plotted together. For those time series for which the statistical test do not show significant correlation, they should not be referred to as showing positive or negative trends. 3.1. Temperature: The meaning of the sentence on Line 11-14 is not clear. What does [phase] mean? I do not see periodicity nor clear distinctive phases; in the time series. As noted by the author but not discussed, the contrasting temperature tends observed at the surface and the bottom warrant in-depth discussion. It is suggested to plot the air temperature observed at nearby coastal stations for comparison with the SST, if the plot does not get too crowded. This may lend some support to the data quality.

3.2. Salinity: It would be interesting to compare the SSS with discharge data of the Pearl River. The contrasting salinity tends observed at the surface and the bottom warrant in-depth discussion. Did the T-S relationship change with time?

3.3. Dissolved oxygen: Did the decreasing D.O. in surface water merely a reflection of the decreasing solubility corresponding to the increasing temperature? The bottom D.O. also decreased despite the temperature decrease. What could be the main cause? An increasing POC flux?

3.4. DIP: The decreasing DIP tends are striking, especially the bottom trend, which is against the temperature trend. What could be the cause? Was the DIP fluctuation correlated with Si fluctuations? If yes, why?

3.5. Dissolved silicate: There was no clear long-term trend, but was the fluctuation related to river discharge fluctuation?

3.6. DIN: The increasing trends are clear, but the data of DIN species listed in Table 2 do not help with the interpretation of the trends. The data in Table 2 seem a set of randomly selected observations without spatial specificity or temporal regularity. A more

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systematic compilation of directly relevant data, such as the consumption of nitrogen fertilizers, or indirectly relevant data, such as the growth of population or food production in the Pearl River watersheds, is needed to support the notion that anthropogenic contribution caused the observed DIN trends.

3.7. Inter-annual variations: For inter-annual variations, it is better to show the detrended data, which are compared to the ENSO index, if the ENSO oscillation is thought to be a major control of the inter-annual fluctuations. Again inter-annual variation of Pearl River discharge should be presented here.

3.8. Fish catch data: The authors might want to demonstrate the changes in the upper trophic level of the ecosystem by showing the fish catch data, but I suspect much of the steady increase in fish catch might be due to increasing efforts rather than changes in the upper trophic level biology. The linear trends certainly do not represent the actual trends borne by the data.

4. Discussion

4.1. Increasing trends

4.1.1. How is the reported SST increasing trend compared to that found in the widely available global SST data set?

4.1.2. The DIN increase at surface: The qualitative arguments, such as “Along with the rapid economic development in China …” (Line 16), are not sufficient to account for the fast increasing trends in a quantitative manner. (See Comment 3.6.) It may be a useful exercise to calculate how much increase in average DIN concentration in the surface layer can be attained by the input of riverine discharge of DIN.

4.1.3. The DIN increase at 200 m is attributed to “no uptake by phytoplankton and possible strengthening of the deep water upwelling”, but these can hardly qualify as viable arguments to explain the observed trend. There has never been DIN

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uptake at 200 m and, therefore, it cannot be used as an argument to explain the increase. Concerning upwelling, why should upwelling, if intermediate water intrusion onto the shelf edge can be regarded as upwelling, increase over the period examined? On the other hand, the water column stratification has been increasingly strengthened due to increasing temperature and decreasing salinity in the surface water. Does the stronger stratification enhance intermediate water intrusion or reduce vertical mixing or prolong the shelf water residence time? These physical processes must be considered for adequate interpretation of the data.

4.1.4. N/P ratio: Calculation of the N/P ratio does shed some light on the puzzling trends. In fact, the increasing N/P ratio towards the average phytoplankton uptake ratio of about 16:1 should make the condition more favorable for phytoplankton growth. The authors noticed this favorable trend, but did not stress it.

4.2. Decreasing trends

4.2.1. Salinity trends: The decreasing salinity trend in surface water is attributed to increasing river discharges, but no data are shown to substantiate this statement. The reported event (Lines 10-17) near the Dongsha Island in 1999 shows the complicated processes occur in the study area and may help explain the inter-annual variability.

4.2.2. Decreasing dissolved oxygen: Since the hydrography data are available, it is strongly suggested to calculate the degree of oxygen saturation and AOU from the existing data set. These values will clearly demonstrate how much effect can be attributed to each of the processes responsible for the decrease.

4.2.3. The time-series of DIP data do not show any significant trend except for the bottom water. This is puzzling in light of the DIN increase. The very large N/P ratios (up to 35) reported for the bottom water is even more puzzling. Previous studies (e.g., Wu et al., GBC 17(1), 2003) have demonstrated that the seawater of the northern South China Sea has N/P ratio lower than the commonly known Redfield ratio of 16 throughout the water column. The reported anomalies are a problem in need of careful

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examination. It is strongly suggested to include other independent observations for comparison with the reported data set. It is crucial to ensure the quality of the data with special attention given to systematic differences.

4.2.3. The time-series of Si data do not show any significant trends as clearly indicated in Table 1. The authors should not ignore their own statistical analysis.

4.3. Nutrient limitation: As noted above (4.1.4), the increase of the N/P ratio towards 16 in the first phase was probably a decisive trend for the increasing productivity. More discussion should be done on this issue.

4.4. ENSO events: The inter-annual variability is an important feature in the marine ecosystem, but it is not clear whether the time-series have high enough sampling frequencies to represent the inter-annual variability adequately. The way it is presented now is rather crude. (See Comment 3.7.) There is little information or argument provided to justify the use of data or to explain the variability. For instance, why is Sta 4 used to represent the inter-annual variability? Do other stations show the same variability? Why did the parameters vary following the ENSO signal? Were the changes caused mainly by changes in marine system or in the atmospheric system (i.e., precipitation that controlled river discharge)?

4.5. Response of ecosystem: This part is problematic because the data are not presented in Results but discussed anyway. Most of the results are extraordinary and, therefore, deserve careful description. As reported, in less than 20 years, the Chl-a increased 650%, PP increased 150%, phytoplankton abundance increased 240%, cephalopod catch increased 700% and demersal fish catch increased 270%. Such drastic changes are amazing and need convincing supporting evidence or reasoning to make them credible. One cannot help but wonder whether the observations in the two phases were representative. No indication was given concerning the sizes of the samples that produced the average values listed in Table 6. As mentioned earlier (Comment 3.8), the fish catch must be strongly affected by fishing efforts, but little light

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Interactive Discussion

Discussion Paper

was shed on this issue in the paper.

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