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Biogeosciences
Discussions

Interactive comment on "Zooplankton communities fluctuations from 1995 to 2005 in the Bay of Villefranche-sur-Mer (Northern Ligurian Sea, France)" *by* P. Vandromme et al.

Anonymous Referee #1

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General comments The manuscript 'Zooplankton communities fluctuations...' by Vandromme et al. focuses on the long-term variation of zooplankton on one station in the Ligurian Sea. One major conclusion of the paper is that bottom-up processes likely play an important role in the inter-annual variation of zooplankton stocks. This challenges the view that physical or top-down processes control the zooplankton in the area. In its present state, the manuscript can only be seen as preliminary. I have especially problems with the presentation of data and the conclusions drawn in the discussion for the following reasons:

- The originality of the manuscript is very difficult to assess. Throughout the manuscript,

authors frequently refer to Garcia-Comas et al., a paper that is in review and not available for the public yet. This paper apparently provides similar data (without phytoplankton though) and one wonders what the submitted manuscript distinguishes it from the one in press.

- Many of the conclusions, including the suggested bottom-up control and the conceptual model, are speculative and not based on original data provided in the manuscript. The manuscript provides data on standing stocks of phyto- and zooplankton, the interpretation focuses on potential mechanisms and processes for which no data or even evidence is presented. One example for this is the potential top-down control of phytoplankton by zooplankton grazing. There is little evidence for this in the literature, and the authors do not try to estimate the increased grazing pressure.

- The presentation of data is inadequate. While the discussion focuses largely on different mechanism acting seasonally (e.g. mixing in autumn/winter; light limitation in summer), the zooplankton data is presented as annual means and not even separated seasonally. The graphs suggest that some of the annual signals are caused from different seasons over time (e.g., nitrate maxima irregularly occur also in summer; similarly, the zooplankton anomaly can be driven seasonally by different groups). All in all, the authors focus too little on their data and the conclusions lack rigorous statistical support.

- Problematic is furthermore the lacking evaluation of sampling variability and how representative the chosen station is for the dynamics of the zooplankton in the Ligurian Sea (as the title suggests). Frontal systems can be important as well as advection.

Considering these principal problems and the poor presentation quality, I am surprised about the list of co-authors.

Specific comments

Title

The title is misleading as the work mostly focuses on the inter-annual variation in annual stocks. Very little detail is given to the composition of the zooplankton.

Introduction

The introduction lacks structure and should be condensed to the main hypotheses concerning the control of zooplankton in the western Mediterranean.

Page 9179

– line 3: Why also?

– line 7: The submitted paper by Garcia-Comas et al. is cited throughout the manuscript, but not available yet.

Page 9180

– line 1: Please specify what is new in the time series

Material, Methods

Page 9180

– line 12: How representative is this station for the area? Does it reflect inside bay conditions? Do fronts occur in the area, what about lateral transport and currents?

Page 9181

– line 7: Please specify why the WP-2 net what chosen. Apparently, this has the disadvantage of being different from previous nets used to study zooplankton in this region and of sampling the small zooplankton only inadequately. Any replicated tows in order to establish a measure of sampling variability?

Page 9183

– line 10: Criteria for the identification of the beginning and maxima of peaks should be given here.

3 Results

Page 9184

– line 1 ff: The discussion focuses largely on seasonal aspects of the zooplankton and ecosystem dynamics. The description of the results, however, focuses on the annual characteristics. Many interesting and important details are lost. I suggest that the authors do not only provide the anomalies of taxonomic zooplankton groups, but include the seasonal dynamics of the groups. Are there any estimates of sampling variability or patchiness available during the study? How are start and maximum values defined for the estimation of timing? Do all groups show similar timing? How are anomalies calculated (running average or average during investigation)? With regard to processes acting in winter and summer, wouldn't it be better to show seasonal stocks (with estimates of seasonal variability) and their anomalies?

Page 9184

– line 1 ff: Why do the authors decide to show annual averages of abundance and estimated biomass? There is little to be extracted from that, especially when the discussion focuses largely on the winter/spring conditions which are conceptually divided from the summer situation. It would be also interesting to see which groups, e.g. small copepods or large ones, respond to the suggested changes over time.

Fig 3 A, B shows the annual anomalies. Information is lost here, particularly on the seasonal aspects, which play a role in the discussed changes over time. The monthly values, shown in Fig. 3 C, D consist of likely 4 estimates of abundance and biomass. I would like to see the variability within in these estimates.

Timing of start and maxima should be shown separately for the 10 groups. The criteria for the identification should be given.

How much do the secondary peaks in late summer contribute to the anomalies? Fig. 3 D suggests that part of the positive anomalies comes from this period. This is important, because in the discussion potential mechanisms vary seasonally.

Page 9185

– line 1 ff: Data on the changes in size should also be presented on a seasonal basis.

Page 9187

– line 1 ff: Fig 6 labelling and legend contradict each other with regard to what they should show. In contrast, to the biology, no seasonal data and changes over time are shown.

Page 9188

– line 1 ff: Here, data is shown for the autumn/winter. Biological data should be presented adequately.

4 Discussion

In their discussion, the authors review to a large part existing literature and focus on the explanation for an increase in the zooplankton abundance since 2000 and their conceptual model. Although it is a logical outcome of the work to provide a hypothesis on the potential mechanisms for the zooplankton increase, this should principally follow from own data. This is not the case, and the manuscript is not original. In contrast, the authors focus very little on their own zooplankton data, which is also inadequately presented. While the discussion concentrates on the spring time, the zooplankton is presented on a annual basis. Details in the compositional changes in the seasonal development of zooplankton are not shown, although this is important in the explanation for the lacking increase in Chla. I also miss a critical evaluation of how well the station reflects the zooplankton dynamics of the area. Do currents, advection or fronts (see for instance Molinero et al. 2008) play no role in influencing the seasonal community composition?

-chapter 4.1 reviewing the major hypotheses concerning the causes for a change in

zooplankton community composition is too long and should be condensed Problematic is that Garcia –Comas et al. is not available- how do the recent results with a different net compares with the published data from the same station? It is hard to judge, therefore, if the present paper is original and provides the innovation as suggested by ‘ more complete data on climatology, hydrology, nutrients and phytoplankton’.

9190

– lines 16 - 26: A different net type and analysis method were used in comparison to Garcia-Comas et al. and Molinero et al. Moreover, the presented data parallels the investigation of Garcia-Comas, and only follows the work of Molinero et al. Thus, there is no overlap with Molinero et al. Before the authors can conclude that their data supports a reversal trend – for which they don’t present data themselves -, they need at least to demonstrate that their data reflect a similar trend as observed in Garcia-Comas.

9191

– Lines 6 - 9: Authors state that a shift in the winter water salinity occurred with less saline water until the year 2000 and higher values in the period 2001-2005. Figure 6 shows that the anomalies in the years 1995, 1996, 1999 and 2000 were higher than 2001-2003. This is not a shift.

– Lines 14 ff: Does the decreased precipitation in winter have an immediate effect on salinity or does this occur with delay (which one would expect to be the case)? From comparing Fig 6 a, b and 7 b this cannot be extracted because it is not clear what Fig 6 is showing in the different panels.

– Line 24: With the conducted sampling scheme (weekly measurements), the authors should be able to directly demonstrate the intensification of winter convection in a graph.

9192

– Lines 1-16: I would like to see a graph regarding the correlation of density and nitrate. Figure 5 shows only annual means, and not winter values. Which have been used to establish the correlation? The lower plot suggests little relation between density and nitrate as years with very high nitrate display only average density (e.g. 2002, 2003). So, where is the trend coming from? The figure 5 shows also that a large part of the annual nitrate signal in 2005 (the year with highest convection) occurs in September and unlikely results from winter mixing. How much does nitrate reflect the availability of other nutrients, e.g., silicate, which could explain that diatoms would get more abundant?

– Lines 17 ff.- p. 9193: The following discussion on the paradox of low phytoplankton at high nutrient conditions and potential top down control by zooplankton focuses largely on published ideas. Although little evidence is provided in the literature (zooplankton abundance in mesocosms and at the selected station should be compared!), authors conclude a potential top down control. Part of their data, however, provides evidence, that the zooplankton follows the phytoplankton with delay, as it would be expected from the duration of zooplankton lifecycles and published evidence (e.g., Smetacek, Kiørboe etc..). At this point, one would expect a thorough analysis of the winter zooplankton concentrations and their potential to keep the phytoplankton low. However, as it looks like, zooplankton abundance is very low in winter spring, which is a counterargument for any top down control. Instead of reviewing published data and speculating about top-down effects, the authors should provide evidence.

9193

– Line 24: The criteria for start and max values in Table 2 need to be defined (especially for the start levels) The constant delay in the zooplankton maxima by 0-2 weeks is surprising considering that the composition changed largely during the investigation (see Fig 4). Any explanations? Which groups are 'hidden' in the spring zooplankton? The authors show mostly annual data, which surprises when the processes are discussed on a seasonal basis.

9194

– Line 7: Although the concept is okay, where is the evidence from the conducted work for both processes acting at the same time?

– Lines 10-25: Again, most of the discussion is speculative and based on literature, but not own measurements, as is its continuation on page 9195. Why should summer irradiation be important for the spring zooplankton development (as most of the biomass occurs March-May, see Figure 3)?

9195-9196

The conceptual model is mostly a repetition of the preceding paragraphs and should be condensed. Evidence should be provided for 'memory' effects, e.g., for the benefit of good zooplankton conditions for the succeeding year. 9197: I could have missed it, but I haven't seen any results for a correlation of zooplankton with the NAO.

Interactive comment on Biogeosciences Discuss., 7, 9175, 2010.