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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.

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We are grateful to the anonymous referee for her/his insightful comments and suggestions on our manuscript. We agree with most of the comments and we will modify/update the manuscript accordingly. Details and answers are provided below. Since some comments were also made by the first referee, we thus suggest to have a look on reply of this referee for additional details.

The manuscript "Zooplankton communities fluctuations from 1995 to 2005 in the Bay of Villefranche-sur-Mer (Northern Ligurian Sea, France)" submitted for publication to the Journal Biogeoscience presents an integrated analysis of the Ligurian Sea pelagic ecosystem. The authors used data on zooplankton, nitrate, chlorophyll-a, sea water temperature, salinity and density as well as air temperature, precipitation and irradiance for their analyses. The authors suggest that the patterns in ecosystem trophic state (lower nutrients and zooplankton - higher chlorophyll vs. higher nutrients and zooplankton - lower chlorophyll) they observed are mainly due to winter forcing on the upwelling of nutrients and that the grazing impact of the zooplankton controls phytoplankton in well mixed years. Other factors (e.g. light availability in spring/summer) were employed to explain years with contradicting patterns.

General comment

Principally, the paper lies within the scope of BG. The title clearly reflects the contents of the paper and the applied methods and statistics are presented in a traceable way. However, there are some major issues which have to be clarified before the manuscript is ready to be published.

The authors themselves report on similar studies previously conducted at the same sampling site which covered a much longer period of sampling (Molinero/Garcia-Comas) but did not include nutrients etc. - even if the present paper includes more background data (nutrients, weather etc.) it does not include data on the whole pelagic plankton community (i.e., phytoplankton and microzooplankton composition) which would have completed the picture of the pelagic ecosystem and would strengthen the conclusions of the authors in a more robust way (they themselves suggest that in their last paragraph).

The lack of microzooplankton composition is one of the main limits of our work. We have recently (2010) started to measure this component of the pelagic ecosystem using the FlowCAM methodology. Further works will discuss their influence on the gen-

eral hypothesis that we have proposed in the present paper.

The authors should give a stronger statement what exactly is the novel concept of their study especially with respect to the short investigation period in comparison to previous works at the sampling station.

Compared to Garcia-Comas et al., paper now accepted in the Journal of Marine Systems (10.1016/j.jmarsys.2011.04.003), and Molinero et al. (2005, 2008), the present paper is distinct and novel because different nets were used (Juday-Bogorov 380µm in Molinero et al. and Garcia-Comas et al., versus WP2 200µm in mesh size in this work) and the time period is different (1966-1993/1974-2003 vs. 1995-2005). Hence, Molinero et al. (2005, 2008) could not observe the change ca. 2000. Size of the net is very important because different nets collect different plankton communities: Juday-Bogorov is efficient for collecting large fragile organisms such as gelatinous plankton and large crustacean but does not collect efficiently small copepods that dominate the Mediterranean sea (Calbet, 2001; Siokou-Frangou et al., 2010): for example for the common period the total abundance recorded by the Juday-Bogorov was 3.63 times less (median value) than the WP2 (1^{st} Quartile = 2.37, 3^{rd} Quartile = 6.03). Our studies confirm and supplements in part the observations of Garcia-Comas et al., but more importantly includes another size fraction of the plankton and also other groups. Contrary to Molinero et al., we do not study target species but analyze all the individuals of 10 plankton groups. In addition to Garcia-Comas et al., we have analyzed more zooplankton taxa and we used more environmental data. Unfortunately, the WP2 time series that we have used was modified in 2005 and results for the new time series are not available.

We added to the revised manuscript more details on the new aspects of the present work compared to Molinero et al. (2005, 2008) and Garcia-Comas et al. (Accepted).

I see also a problem with the conclusion that spring/summer irradiance (April-August) can counteract or reinforce the effect of winter convection and that phytoplankton was

not nutrient but light limited. The authors should clarify the mechanisms they see behind this pattern.

We fully agree with the referee. This point is the most speculative of our work since no data could be used to test this hypothesis that is based only on a modeling framework. From the dataset it is striking that the irradiation inter-annual variability is correlated to the zooplankton change at annual scales. Other mechanisms than the one we proposed may also be important. We modified this point by presenting other hypotheses such as an indirect effect through an increase of stratification beneficial for the ecosystem (i.e. optimal stratification).

Data on Secchi depth (turbidity), stratification and mixing depth and also phytoplankton composition (Nitrogen-fixing cyanobacteria?) should be taken into account, at least be discussed to see a clear relationship between irradiance and zooplankton abundance.

We do not have data on Secchi depth and phytoplankton composition. We have calculated the stratification and mixing depth anomalies during spring/summer. During the 11-year period, no significant correlations appear with solar irradiation. There is no evidence, to our knowledge, of nitrogen fixation in the Ligurian Sea. We added a discussion concerning the links between irradiation and stratification in the revised manuscript.

Data on turbidity are unfortunately lacking. It is only since the beginning of 2010 that PAR measurements are recorded.

I also see no cause for discussing NAO if there are no significant influences measurable and no data or figure is shown in the results part!

We agree. As both reviewers see no reason for having this part in the manuscript we will omit it.

Specific comments

Generally English should be checked throughout the whole manuscript, as there are several mistakes that are consistently present (e.g. lacking articles, plural/singular mistakes). Especially the results section should be improved significantly.

Abstract

9177 line 4: it is "decapod larvae" (see throughout the manuscript and in figures also) line 12: please specify: it is chlorophyll-*a* not phytoplankton you measured!

corrected

Introduction

Garcia-Comas 2010 is cited several times throughout the whole manuscript but is not available to the public yet; therefore the authors should give details on hypotheses (e.g. 9179 line 23) and results when they refer to this paper to make it easier for the reader to follow the arguments of the authors.

The paper of Garcia-Comas was accepted on April 4th at Journal of Marine System (10.1016/j.jmarsys.2011.04.003). The review process was long, it was submitted in June 2010. This paper will be available on line in about one month. We can send it to the reviewer if necessary.

9179 line 26: it is "occur"

line 28ff: wrong citation - in Molinero 2008 there was also weekly sampling and data afterwards monthly means were calculated!!! We will correct the citation The authors also refer to monthly means or less in their figures, so what is the novel strategy?

It is a different time series, sampled with a different net and covering a different time period. It allows examination of different mechanisms explaining why zooplankton abundances have increased since 2000 (see above and in comment to reviewer 1 for more details). Molinero's work stopped in 1993.

9180 line 7: it is "nitrate concentrations" line 9: it is "parts"

Material and methods

9182, line 19: add "see" Fig. 2... line 28: better "time period" instead of lag 9183 line 2: please rephrase the sentence - why you start "Results will focus..."? 9184 line 1: please specify what ?l is.

Results

The whole section needs proof-reading for English!!!

Only few examples: 9184 line 6: it is "terms" line 7: please rephrase: The less abundant... "The year with the lowest abundance..." also several other similar sentences in this section! line 11: "The" spring peak... line 23-26: The sentence needs to be rephrased! line 27: "highlights" 9185 line 1-2: does that average biovolume reflect changes in composition of zooplankton (I guess so) and why do you not show these in figures (e.g. pie charts)? line 16: in year 2000 copepods also showed a negative anomaly whereas pteropods were already positive. Probably you wanted to say: Copepods reacted earlier with a positive trend than the bigger zooplankton species...?

You are right, we will correct the mistake.

We will rephrase the paragraph in the revised version taking into account all the suggestions. We did not add a pie chart because it did not provide additional information. In the revised manuscript, we describe more the change between zooplankton taxa by using a PCA. The main results show that all groups vary together looking at both annual and monthly values. Only chaetognaths show a slightly different monthly dynamic since they appear later in the year, i.e. around August.

The PCA will be added in the manuscript.

Section 3.2.1: it is always "nitrate concentrations"!

9187 line 1: from "the" average. . . line 2: between "week 4 and 17" of the year line 5: "constant difference" line 11: "would not have changed"

Done

Section 3.2.4: The title is misleading - you show no data on phytoplankton growth! Even more important would be to show data on light availability during winter/early spring which can directly affect the spring bloom development, which you do not show here - why?

We agree and we will remove the mention of phytoplankton growth. We have shown data on light availability in late autumn/winter (fig. 7C) and found no link with biological components. The light availability during winter/early spring (February to April) do not show any correlation with chlorophyll-a values (Spearman, r=-0.086, p=0.634, N=33 months). In the revised manuscript, we will modify the light sections because both authors found it to be too speculative (see answers above and also to referee 1).

Discussion

9189 line 14: "1980's onwards" line 24: "benefit from" line 26: "supported" 9190 line 11-15: verb is lacking line 23: "the condition of the 1980's in the first years of 2000" line 24: "did not occur" 9191 line 9: "was observed" line 12: "river flow" line 20: "salinity anomalies" 9192 line 5: "the year 2005" line 8: "nutrient availability" line 9: "was always" line 12: showing data on the "quality of phytoplankton" (i.e. functional groups like diatoms and flagellates or size classes) would significantly improve the quality of the manuscript. . . line 13: please rephrase: zooplankton identified taxonomic groups??? line 14: "levels was" line 17: "A striking result"

line 17ff-9193 end: The "strong top down control" of zooplankton on phytoplankton should be discussed with greater attention for other grazers like microzooplankton. These have been shown the most important grazers on phytoplankton (not the meso-zooplankton!!! compare Calbet (2001); Calbet and Landry (2004)) and are most possibly the most important food source for the mesozooplankton in the Ligurian Sea as well (phytoplankton carbon channeled through microzooplankton to mesozooplankton).

Unfortunately, microzooplankton was not collected during the 1995-2010 time period. We agree that it is an important component. In the revised manuscript, we will discuss in detail the possible role of the microzooplankton within the scheme we have proposed.

9193 end: Are there no top down factors on zooplankton like fish etc.?

There is no data on fish in this area. There are no large fisheries because there is no continental shelf like in the Gulf of Lion (200 km west of the point B location). It is then difficult to answer to this important question. We will briefly discuss this possibility in the revised manuscript.

9194 line 1ff: I see no reason for discussing the "mach-mismatch" hypothesis.

This part will be removed.

Section 4.3: As already stated above I have problems with the light limitation in spring/summer and its influence on zooplankton. line 23ff: The irradiation values were integrated over 75 meters of depth - stratification should not reach this depth *(stratification reached about 50-60m depth in June-September)* and therefore phytoplankton which circulates in the upper water should receive a lot more light than the calculated values. Is turbidity from terrestrial discharge really a problem at station B if so - there is a great need for the addition to the model as the authors them-selves stated in this section.

As already noted, this section was the most speculative, and considering the comments of both reviewers need to be reduced.

We are therefore concentrating more on the observation showing that the irradiation and zooplankton stocks are correlated at the inter-annual time scale; and that taking irradiation into account may be necessary to account for the variability not explained by the winter forcing. Alternate hypotheses for the irradiation-zooplankton link will be discussed.

9195 line 10ff: Not necessary but interesting: could you go into more detail which zooplankton groups would show a time lag and why - plus cite more literature on that issue?

While small copepods abundance increased in 2000 (pteropods also), most of the large plankton abundance increased in 2001 (crustaceans other than copepods, decapod larvae, chaetognaths, appendicularians, gelatinous predators, others), thaliaceans increased in 2002 and large copepods in 2003. It was mentioned in p9185 lines 12-13 and in p9195 lines 11-13. Yet, in spite of its interest, it is a small paragraph since we did not found literature on that. Other zooplankton time series studies did not mentioned such lag (e.g., Valdes et al., 2007; Fernándes de Puelles et al., 2007; Eloire et al., 2010, . . .). Interestingly, this kind of time-lag was observed in lake zooplankton recovery after a stress, e.g. acidification (Frost et al., 2006). In Frost et al., the zooplankton community recovered from less than one-year to more than 4 years depending on the zooplankton groups, the longest to recover being copepods and daphnia. In such environment, populations reached low abundance during stressful periods as it was the case at Point B during the '90s. For example, appendicularians were on average of 0.1 $\mathrm{ind.\,m^{-3}}$ *from 1995 to 1999 and were present in only 15 % of the samples. Such extreme oligotrophy reached during the '90s as observed by Molinero et al. and Garcia-Comas et al. may explain the observed time-lag in the recovery of zooplankton.*

We added more details on that in the revised manuscript.

Section 4.4: It is "microzooplankton" not microplankton! The whole conceptual scheme should be improved according to the additional information added (phytoplankton, microzooplankton, light availability). line 26ff: The interaction between mesozooplankton and microzooplankton is more complex than the two examples that are discussed, you should go into more detail.

Thank you for your remark. We added more detail on microzooplankton role by using references given by the reviewer and other (e.g., Calbet and Landry, 1999; Vadstein

et al., 2004; Umani et al., 2005; Hernandez-Léon, 2009). We will discuss some potential role of the microzooplankton. We will include them in the discussion on the general functioning of the ecosystem.

9196 line 9/12: "moderated" line 13: sentence is incomplete - please rephrase line 14: "was weak with regard to winter. . .high with regard to. . ." line 28/29 "increases" "decreases"

9197 line 12: "proposed"

Section 4.5: I see no reason for discussion NAO in a whole section as there is no significant influence detectable (see also comment above).

Conclusions

This section should be adjusted according to the changes and improvements of the manuscript.

We will take into account the comments made in this review to improve this part.

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