

Interactive comment on “The pelagic microbial food web structure in Sanggou Bay, Yellow Sea: Spatial variation over four successive seasons” by X. Chen et al.

X. Chen et al.

wuchangzhang@qdio.ac.cn

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Problem N°1

What is new is to address the structure of the microbial food web (MFW) as a complex system and as such, it is a systemic approach. This is different from considering separately relationships between two components independently of the others.

It was natural to start from the relationship between HNF and heterotrophic prokaryotes (HP) abundances that was already well established. After establishing that a similar relationship existed between HP and 5 other components of the MFW, the common ground to these relationships was obviously HP. Indeed, HP permanently occupy the

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whole oceanic space and their abundance was shown to be confined between empirically defined upper and lower macroecological limits in the world ocean as reported by Li et al. (2004).

Consequently, normalisation of the MFW component biomasses by HP biomass, provides an image of the MFW structure at the sampled site and at a given time. There is no dynamics regarding the MFW structure, dynamics usually expressed in terms of top-down or bottom-up control. Nevertheless, the analysis of the MFW structure in terms of preys and predators may provide some clues to estimate if the MFW is under top-down or bottom-up control as we did in this work.

Although Garrison et al. (2000) firstly applied the cluster analysis to the biomass of the MFW components to find that the MFWs in different seasons were different, they did not identify what the differences were. We applied the normalization method (normalizing the biomasses of other microbial communities to that of HP) on the data of Garrison et al (2000). Our aim of the normalisation is not to confirm their result, but further to identify what the differences of the MFWs were. We are sorry that we did not express clearly in the current version of the manuscript. Here are the differences of MFWs in Garrison et al (2000): We further pointed out that the differences among four different seasons were the changes of SYN, PNF and HNF biomass. The biomass of SYN was extremely high in the early NE Monsoon, and remained low in the other seasons. The biomass of PNF and HNF were low in the early NE Monsoon and high in the other seasons.

Problem N°2

The reviewer stated that our dataset (n=72-76) was not sufficiently large. Sanders et al. (1992) and Gasol (1994) analysed a dataset HNF-bacteria from n>250 samples. This would make 500 values or so. In the paper, 6 components of the MFW are concerned, which represents about 450 values, of the same order as the number of values handled by Sanders et al. (1992) and Gasol (1994) and covering 6 components of

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the MFW instead of 2. We can reasonably conclude that our dataset is more robust to represent the MFW. On the other hand, our dataset in each season were collected from 19 stations which covered the most of Sanggou Bay. Because of the large ranges of temperature and Chl a concentration in Sanggou Bay, there was a large range of HP abundance, $(0.2-6.3) \times 10^6$ cells cm^{-3} , which was closed to the ranges that Sanders et al. (1992) and Gasol (1994) had reported. So we think our dataset can cover a wide range of environmental conditions, and was allowed for generalization. We must add that the overall number of data might not be a criterium of confidence. Though we did not apply in our case the Shannon information theory, we know by using it under other circumstances that this theory enables to estimate the additional information brought by a measure with respect to the others. This defines domains where additional measurements have no weight and others where data points highly contribute to the information. Briefly, this theory helps in defining where to make the effort to collect measures and where accumulation of data is useless. In other words, a large number of data might not provide a better information than a smaller one that would gather data points well selected and thus more informative.

Gasol (1994) proposed a qualitative model to address the control (top-down or bottom-up) on HP abundance. It is more a reasoning about the comparison of HP abundance with respect to the upper and lower empirical boundaries. In our work, we did not develop too a real model of the MFW. We addressed the MFW control through prey-predators considerations concerning the MFW components and were able to derive some conclusions about the control to point out situations corresponding to top-down or bottom up control. We consider that our dataset could be very useful to test a real model dedicated to MFW functioning. The trophic relationship was very complex in the MFW. HNF grazed not only on HP, but also on SYN (Guillou et al., 2001; Christaki et al., 2002; Christaki et al., 2005; Apple et al., 2001). There were several trophic levels among different sized HNF. Ciliates grazed on NF (Sherr et al., 1991; Kamiyama, 2000; Chen et al., 2010; Verity, 1985), PEUK, SYN (Christaki et al., 1998; 1999; Apple et al., 2011) and HP (Rivier et al., 1985; Sherr et al., 1987; 1989a; 1989b; Seong et al.,

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2006). PNF also grazed on HP and SYN. Therefore, all these organisms are trophic interacting with each other. Therefore, the MFW is a like multi-enzymatic systems as stated in the manuscript. Here, we extended the relationships between HNF and HP to the other members of MFW, which include relationship between SYN and HP or between PNF and HP, and so on. Our aim was to find out the temporal and spatial changes of MFW by using HP as a basis, rather than to find out a certain relationship. And the reason lying behind the changing pattern were complex.

Specific comments

p.6 line. How was the biovolume estimated? How many individuals were analyzed for this estimation?

We were not sure about which biovolume the referee mentioned in p.6. The length (L) and width (W) of a nanoflagellate cell were measured on photomicrographs using the Leica DM 4500 self-carried software. At least 60 cells (PNF plus HNF) were measured per sample. Cell volumes of NF individuals were estimated by assuming their nearest geometrical figures (Sun and Liu, 2003). The biovolume of ciliates were estimated from appropriate geometric shapes, and 5-10 individuals of each species (a total of 75-150 individuals) were picked out randomly for calculating the biovolume.

p. 12 line 21. Are these ranges really similar?

The HP abundances reported by Sanders et al. (1992) were in the range $0.1-15.2 \times 10^6$ cells cm^{-3} . Our HP abundance range was $(0.2-6.3) \times 10^6$ cells cm^{-3} , which was within the data ranges of Sanders et al. (1992) and Gasol (1994), and our data range could cover most of the ranges of Sanders et al. (1992) and Gasol (1994). So we called them similar. But it was impossible that our data range be exactly the same as the data ranges of Sanders et al. (1992) and Gasol (1994), because our data were collected in a single bay area, whereas the data of Sanders et al. (1992) and Gasol (1994) were collected from many different ecology systems (including marine, lakes, rivers and sediments).

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p 14. lines 16-18. Could correlations be interpreted as indicators of strong grazing pressure? lines 19-20. Are there data supporting these statements?

The $P < 0.01$ means the relationships were statistically significant, and $r^2 > 0.25$ means the relationships were strong. So the data indicated that HNF can be predicted from HP, SYN and PEUK. And the strong grazing pressure were the explanations which can be proposed for the strong correspondence between HNF and HP, SYN and PEUK. In the same way, we gave possible explanations according to the significant but quite weak relationship between HNF and NF.

p. 15 line 13 – 15. It is necessary to have similar sampling dates to apply this approach?

After applying our approach to the Garrison et al. (2000) dataset that also addressed the MFW structure and biomass, we can say that it is not necessary to have similar sampling dates and we will highlight this point in our conclusions. What is necessary is to deal with a data set the concerns the MFW as a whole and documenting most of its components. The Garrison et al. dataset also covered 4 seasons and the successful application of our approach demonstrates that it is not date dependent. If we had applied it to a single sampling period, doubts could have been raised about its efficiency at other periods. What was important was not so the similarity of sampling dates but the fact that we could consider a dataset covering MFW at different time periods. We also will make this point more clear in the revised version.

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