

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.

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The introductory paragraph does not seem an objective account of the manuscript content. The study was conducted off shore at 4 successive seasons which is a critical point to follow the evolution of an ecosystem and such studies are not so frequent in the literature. Together with abundances of MFW components, environmental variables were also determined. The common thread of the paper was completely overlooked. The Referee mentions “correlations between abundances of some of these groups” as if it was a random process whereas the paper focused on relationships between HP abundances and those of the other MFW components. This led to the generalization of the empirical relationship of Gasol between HP and HNF, enlightening a pivotal role

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of HP in the MFW. It was then natural to refer the MFW component biomasses to that of HP in a normalization process to express the MFW structure that was not derived from the generalized relationships as inferred by the reviewer.

*/*1* The above remarks reveal serious flaws in the review, particularly when it is stated that “authors rely only on abundance (cell counts) to attempt a food web reconstruction”. As mentioned above, biomasses normalized by that of HP were used to express the MFW structure, not at all abundances. To our knowledge, this was not applied in the past in contrast with the statement of Referee 2, nor was before the empirical relationship of Gasol generalized to the other MFW components. We do know that there are vertical and horizontal interactions, trophic cascades among the members of microbial food web as well as internal predation in microzooplankton. Those complex trophic relationships render any effort of bottom-up and top-down analysis to be questionable. In the case of the HNF and HP empirical relationship of Gasol (1994), we should keep in mind (and we think that Gasol also knows) that HNF also graze on *Synechococcus* and picoeukaryotes, HP are also grazed by ciliates. Therefore, when we analyze the relationships between MFW components, we do not intend to characterise their trophic activity as criticized by Referee 2. The aim of this paper was to look into the numerical relationships among the MFW components. We did not “take potential trophic cascade effects into account, mention potential horizontal interactions between groups sharing trophic levels, discuss potential “intra-group” variability among the studied taxa” just because we want to focus on certain aspects. If all those aspects were discussed, that will make a book. On the other hand, every effort to solve a problem begins with the simplification of the problem. It is normal not to consider all these aspects as a first step to look into the relationships between the MFW components.

*/*2* We consider our dataset as representative because HP abundances in our study covered the full range of reported HP abundances around the world ocean. Referee 2 questions how representative our study is. However, he did not give a standard about the representability. This question is always open without a standard. A possible an-

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swer to this question might be the more the better. However, in the case of MFW structure studies, abundance and biomass of MFW components are poorly documented. Most of the previous studies only consider part of the MFW members. Therefore, we could not add more available data from other studies. In our reply to Referee 1, we stressed the point that it might not be the number of measurements that matters, but the information brought by an additional measurement compared to the others. In that sense, exploring the range of variation of a given variable is more important than multiplying measurements over days within identical conditions.

/*3 As the first effort to try to characterize and depict the MFW, this study begins from a particular study site. Answers to every biogeoscientific question have seasonal and geographical variations. An answer of general value should be the integration of works from different sites. Therefore, a particular study has both local and general value simultaneously. The evaluation of local or general value may depend on the questions that are addressed. For questions already having general answers derived from many investigations, any study supporting the general answer is of local value. For questions addressed by few investigations, any study is of general value. This paper belongs to the latter case. We used the results of Garrison et al (2000) in order to test our approach on a different site, the Arabian Sea, the only site that we found with a comparable dataset, and to conduct this test over different seasons. This is the classical way to proceed when proposing a new approach, to demonstrate that it delivers results that do not contradict those well established by conventional approaches. Such an agreement does not mean that the new approach is useless but, to the opposite, that it may represent advantages to reliably reach results that were less easily accessible by conventional means and to provide access to new knowledge. We found that the Arabian Sea MFW structure exhibited a very low PEUK biomass and higher SYN biomass compared with that of Sanggou Bay. We further pointed out in the responses to Referee 1 that differences among four different seasons were the changes in SYN, PNF and HNF biomasses. SYN biomass was extremely high in the early NE Monsoon, and remained low in the other seasons. PNF and HNF biomasses were low in the early

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NE Monsoon and high in the other seasons. Garrison et al (2000) only considered the absolute changes of different MFW components, but what we focused on was the relative structure changes.

As an effort to answer the overall conclusion of Referee 2, we argue that our manuscript aims to analyze the structure of microbial food web in a particular site (Sanggou Bay) using a new approach. We are not writing a methodological paper. The scientific question we addressed is in the scope of Biogeosciences. And we think Biogeosciences is a good broadcast platform for our paper due to its reputation among marine scientists, its open discuss and free access style.

Even though we disagree with remarks and conclusions of Referee 2, we are taking into consideration that the presentation of our work might not be clear enough to be taken as we see it and we will take a particular care to make the discussed points more clear and straightforward in the revised manuscript.

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